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"*To the solid ground*
Of Nature trusts the mind that builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, NOVEMBER 6, 1873

THE GOVERNMENT AND OUR NATIONAL MUSEUMS

WE referred last week to the intention of the Government to transfer one of the Metropolitan Museums under the control of a responsible Minister of the Crown, to the fifty irresponsible Trustees of the British Museum, this step being contemplated without referring the question either for the opinion of the Science Commission, now inquiring into these subjects, or for the authority of Parliament. We have learnt since that the measures for effecting this change are in active progress. Lord Ripon and the Trustees of the British Museum having agreed that the transfer was to be made *if practicable*, Sir Francis Sandford, Mr. MacLeod, and Major Donnelly, on behalf of the Science and Art Department; and Messrs. Winter Jones, Franks and Newton, on behalf of the Trustees of the British Museum; are now busy as Commissioners to find out if the transfer *be practicable*, and they have been exploring the South Kensington Museum for this purpose during the last week, taking notes of its contents, inspecting its refreshment rooms, its waiting rooms and the like.

What the Commissioners will propose as practicable is of course known only to themselves, if it be known even to them. Thus much, however, is known: the South Kensington Museum must remain the head-quarters of Science and Art Teaching, unless that too is to be put under the Archbishop of Canterbury and his co-Trustees, and if not, then there must be a dual Government in one and the same building, unless Mr. Lowe's project be abandoned. Now the dual Government means that one officer will represent the Archbishop of Canterbury and his co-forty-nine trustees in the Museum, and another the Lord President of the Council. The officer representing the Department will take orders from the Lord President. The officer representing the Trustees must from time to time go to Mr. Winter Jones to ascertain what the fifty Trustees have decided, and to receive his instructions how their decision is to be interpreted. Mr. Winter Jones' labours, already said to be ill-remunerated, will be increased, and his well-known powers of organisation sorely taxed. If there be two things which nature puts in ferocious antagonism one to another, it is two public officers under different responsibilities. No envy, hatred, or malice like that between two public officers. How every officer adores the Treasury! how the Audit Office loves the Treasury! what models of civil Letters the Treasury always writes to the Officer of Works, and so on.

The public has had already a specimen of this kind of dual Government at the South Kensington Museum, which has had disastrous results for Science. When the "Boilers" were first erected in 1856, the Commissioners of Patents had assigned to them a portion at the south end of the building for exhibiting those Mechanical and Scientific objects, which under a fiction were supposed to have derived their origin in "Patents." It was necessary that the visitors to all parts of the "Boilers" and to the Picture Galleries should pass through the "Patent Division." The Lord President made sensible rules for admitting the public on three days, open from 10 A.M. to 10 P.M., and three days called "Students' days," when persons not students paid sixpence each, or ten shillings a year, the object being to have three days free from crowds and kept quiet for study. After a while the Commissioners of Patents were scandalised at thus receiving public money (they are the instruments for taking seventy thousand a year from Inventors and misapplying it to General Taxation) and they said they preferred crowds every day as the most convenient public arrangement. The authorities came to open discord on the point, and the matter could only be resolved by separating the "Patent" from the other collections. So the Patent Commissioners built a separate entrance for themselves. What has been the result? About eight millions of visitors to the South Kensington Museum who would otherwise have seen the "Patent Museum" have not done so, and the Commissioners have deprived themselves and their museum of the moral support of these great numbers. And what has been the result of this? The Chancellor of the Exchequer has been allowed to sack more than a million of pounds sterling realised from the taxes imposed on inventors' patent fees, and has not allowed one farthing to be spent for the provision of a suitable building for the "Patent Museum." Anything more discreditable to the nation than the building now crowded with models cannot be conceived. Many of the passages are not eighteen inches wide! What the present Lord Chancellor, the head Patent Commissioner, would say if he were ever to see it, cannot easily be imagined. We advise his Lordship to hold a Board in the building as soon as possible. It will probably be the first Board of Patent Trustees that ever sat there. We are satisfied that the result would be that he would instantly cause the present exhibition to be closed, and adequate space found elsewhere. Then what have inventors got in return for the tax of a million drawn from them? And what may not invention have lost by this indefensible principle of taxation?

Here then is already a very practical illustration of dual government in the South Kensington Museum already; one part of that government being composed of Trustees, who, it is reported traditionally, have never once met as a Board in their own Museum to see what was imposed upon a suffering public, upon their responsibility. We do not believe such a state of things would have been suffered under South Kensington administration. Mr. Lowe, when Vice-President, of the Council would not have suffered it.

The indifference of the British Museum Trustees to some of the best interests of Science in their own museum has been denounced again and again by commissions and committees, who report and report, but make no impression on a corporation of fifty trustees. That alone is a reason why they should not be allowed to meddle with South Kensington.

Although, as we have stated, this proposal was made without reference either to the opinion of those to whom the interests of Science and Art are more precious than they are to the members of the present Government, or to the opinion of the House of Commons, we learn that Mr. Mundella has extracted a promise from Mr. Gladstone that nothing shall be decided until Parliament meets again. Mr. Gladstone is perhaps surprised that there is any public interest in the subject. In the meantime, to assist him to form a correct judgment, we advise every learned society, which takes any branch of Science under its care, to memorialise the Prime Minister, and point out the crying necessity of a Minister, who shall be responsible to Parliament for Science, among other matters, and for all museums; that to transfer a museum already so represented to irresponsible trustees is a step worthy of the Middle Ages; and finally, that while the South Kensington system represents everything that is best in the way of progress, so much, to say the least, cannot be urged in favour of the present management of the British Museum.

We can well understand the reason for the proposed change. It lies in the individual responsibility of a Minister and the energetic executive management which have raised in a few years the South Kensington Museum into an institution of which the nation has the greatest reason to be proud; which has made it the centre of the chief intellectual activity of the country, which has utilised its resources for the teaching of hundreds of thousands of our teeming populations. The British Museum Trustees have done none of these things; they have given no trouble; they have borne snubbing admirably when they *have* moved, which has not been often. They have, in fact, proved an admirable buffer between subordinates anxious for progress and the Government; and, further, they have not been represented in the Cabinet. The moral which the Government has drawn from these facts is, that the South Kensington energy should have such a buffer, and in the existing members of the British Museum have found one ready to their hand. Hence the proposal which, if we mistake not, will, when it is generally known, not find a single supporter out of the Cabinet. It is quite possible that already it finds not many supporters in it.

BAIN'S REVIEW OF "DARWIN ON EXPRESSION"

Review of "Darwin on Expression." Being a Postscript to "The Senses and the Intellect." By Alexander Bain, LL.D., Professor of Logic in the University of Aberdeen. (Longmans, Green, and Co.)

THERE is nothing in this Postscript to "The Senses and the Intellect" so important to psychology as the declaration and announcement contained in the following sentences: "In the present volume I have not made use of the principle of Evolution to explain either the complex Feelings or the complex Intellectual powers. I believe, however, that there is much to be said in behalf of the principle for both applications. In the third edition of 'The Emotions and the Will,' now in preparation, I intend to discuss it at full length." No man can claim to have done more for the study of psychology than Prof. Bain; and in now recognising the principle of evolution and in incorporating it with his system, he is doing the science the greatest possible service. This is more than in some quarters was ever hoped from Prof. Bain, and more than was ever feared by those of his disciples who—after the manner of disciples—have clung most tenaciously to the defects of his system.

Though accepting the principle of evolution, Prof. Bain does not, it would seem, always look at phenomena from the evolutionist's point of view, as we understand it. Thus, in speaking of the large extent to which Mr. Darwin uses the principle of inheritance to account for the phenomena of expression, he says:—"Wielding an instrument of such flexibility and range as the inheritance of acquired powers, a theorist can afford to dispense with the exhaustive consideration of what may be due to the primitive mechanism of the system; he is even tempted to slight the primitive capabilities, just as the disbeliever in evolution is apt to stretch a point in favour of these original capabilities." But whence the so-called "primitive mechanism" which is here made separate and distinct from, set over against the products of inheritance? is not the "primitive mechanism" the "original capabilities" of every creature the results of evolution?

Mr. Darwin is accused of not having given sufficient attention to "spontaneity of movements," which, according to Prof. Bain, "is a great fact of the constitution." Now it may be that a "readiness to pass into movement, in the absence of all stimulation whatever," is a fact of the constitution; but we fail to see that Prof. Bain has given any proof that such is the case. He says :—"We may never in our waking hours be wholly free from the stimulation of the senses, but in the exuberance of nervous power, our activity is out of all proportion to the actual solicitation of the feelings." What is the right proportion of activity to feeling? the proportion that Prof. Bain takes as his standard by which to discover that at times our activity is out of all proportion to feeling. Is not the simple and the whole fact this, that the amount of bodily movement that goes along with a given amount of feeling is different in each individual, and in the same individual from hour to hour. He continues :—"The gesticulations and the carols of young and active animals are mere overflow of nervous energy; and although they are very apt to concur with pleasing emotion, they have an independent source? their origin is more physical than mental." Is not the origin not of these only, but of all movements, entirely physical, though it is also a fact that some movements, and certainly these among the number, concur with pleasing emotion? Mr. Darwin has instanced the frisking of a horse when turned into an open field, as an example of joyful expression; on which it is remarked, this "is almost pure spontaneity it does not necessarily express joy or pleasure at all. How curious! One must really be a psychologist before he can see common things in such an uncommon light. Perhaps no movement necessarily expresses any state of consciousness whatever: but no ploughboy, we venture to think, ever doubted that the frisking of his horse, when he turned it loose in the field, was an expression of delight. But, then, ploughboys have no theories about spontaneous activity. All mental states correspond to certain physical conditions; that "the nerve-centres and the muscles shall be fresh and vigorous" is the physical condition of much bodily activity, and at the same time of the pleasure that goes along therewith. Granting that "the kitten is not seriously in love with a worsted ball," it thoroughly enjoys the sport nevertheless. Its amusement being mere play does not preclude its being real pleasure. And if our memories can be trusted, the worsted balls of our childhood were far more delightful than the gold and substantial realities we seriously love in our old age.

S.

"LAHORE TO YARKAND"

Lahore to Yarkand. By Geo. Henderson, M.D., and Allen O. Hume. (L. Reeve & Co.)

TO Mr. Forsyth, the able conductor of the expedition which they describe, the authors dedicate this handsome volume, which, instead of being a continuous narrative, is divided into three separate parts, each of which will appeal to a different class of readers. The description of the route, and the incidents encountered on it, are given by Dr. Henderson, who with Mr. Forsyth and Mr. Shaw were the only Europeans that went to Yarkand on this "friendly" visit, sent by our Government to the Atalik Ghazi; it occupies two-fifths of the work. The natural history of the living forms met with, mostly by Dr. Hume, fills about one-fourth; the rest consisting of meteorological observations taken by Dr. Henderson on the journey.

The difficulties that had to be encountered *en route* were many and severe; the desert nature of the road between the districts of Ladak and Yarkand made it almost necessary to discontinue the expedition, and the great heights that had to be surmounted put a check on rapid progress, in some parts rendering it impossible.

Several opportunities occurred for the observation of the physiological effects of higher altitudes and rapid changes of barometric pressure, one pass near Gnishu which had to be traversed, named Cayley's Pass after Dr. Cayley its discoverer, being 19,600 feet above the level of the sea. From Dr. Henderson's remarks, however, it appears that mountain sickness is not dependent on the rarity of the air alone, for during the time that the expedition was in the pass mentioned, no note was recorded of any of the number suffering from it, whilst previously, on the Chang-la, which is 18,000 feet, most of the camp suffered from severe headache, nausea, prostration of mind and body, together with irritability of stomach and temper; nevertheless observations at the time showed that the pulse was not unusually rapid and the respiration was but little, if at all, increased. The feeling of suffocation occasionally experienced on waking during the night usually passed off after a few deep inspirations had been made. It is much to be regretted that, with the opportunities of verifying and extending Dr. Marcet's observations on the effects of ascending and descending mountains, Dr. Henderson was not in a position to do so, which he undoubtedly would have done if he had been acquainted with them.

Shortly after leaving Patsalung, and when on the southern boundary of Hill Yarkand, "nearly ten miles of the way was over a plain about five miles wide, which was covered to a depth of many feet (in one place where cracks existed, to not less than twenty feet) with sulphate of magnesia (Epsom salts), pure and white as newly-fallen snow." This shows the abundance of a magnesian limestone in the surrounding higher ground, and as the water-supply of the city of Yarkand was from rivers which rose in this or similar hills, the author's remark that "about every third man we saw was afflicted with goître," is scarcely more than was to be expected, and we think that if, instead of making "over to the Dád Khwah a quantity of iodine, for the treatment of goître, at which he was very much pleased," he had proposed a change in the water-supply, the Yarkandis would have been the gainers in the long run.

As the Atalik Ghazi was away at the time Mr. Forsyth arrived at his destination, and as the latter had strict orders to return before winter, the mission was partially unsuccessful. The return journey being later in the year, the cold and discomfort were greater than on the march north; an idea may be formed of the acuteness of the cold from the author's note on the Sukat pass. "My ink was constantly hard frozen, and on several occasions when I thawed it before the fire and attempted to write in my pencil notes, it froze at once on the point of the pen. Several times I tried to photograph, and once or twice succeeded, but usually the tepid water used for washing the plate froze as I poured it from the jug, and instantly destroyed the picture."

The illustrations of scenery, which in many books of travel are but indifferently drawn, and disappointingly inaccurate, are in this work replaced by "heliotype prints" from photographic negatives taken by Dr. Henderson himself, and nothing can, in most cases, be more satisfactory. What is wanted on such occasions is not only a picture, but a representation sufficiently full of detail to enable the reader by simple inspection to form a truthful idea of the country described. Such are found in the photographs of the Valley above Paskyum, and the fort and bridge over the Indus river at Kalsi, and others before us, which, from the contrasts of light and shade, and the evident glare, bring vividly before the mind the intensity of the heat, as well as the desolateness of the locality, a combination scarcely possible in any character of engraving.

The Natural History notes are mostly ornithological and botanical. In his *résumé* of the ornithological results of the expedition, Mr. Hume informs us that "altogether, 158 species were observed, but of these only 59 pertain to the ornithologically unknown hills and plains of Yarkand. . . . Of these fifty-nine species, 7, *Falco hendersoni* (? *F. milvipes*, of Hodgson), *Saxicola hendersoni*, *Suya albo superciliaris*, *Podoces hendersoni*, *P. humilis*, *Galerida magna*, and *Caccabis pallidus*, are probably new to Science." An excellent illustration, by Mr. Keulemans, is given of each of these new species, except the last, which is very closely allied to *C. chukar*, and the coloration of the drawing of *Sturnus nitens* (Hume) exemplifies very successfully the propriety of the specific name. Mr. Gould's description of *S. purpurascens* is compared with that of the new species, the former being absolutely speckless and much smaller. *Podoces hendersoni* and *P. humilis* are both new species of this genus, which the author, following Bonaparte, places with the Choughs and not with the Jays and Magpies, remarking, however, "remembering their ground-feeding, dust-loving habits, . . . I cannot avoid the suspicion that these birds may constitute a very aberrant form of the great Timaline group."

On the Chang-la pass above referred to, Mr. Shaw obtained a butterfly, which Mr. H. W. Bates places in the mountain genus *Mesapia*, naming it *M. shawii*; it closely resembles *M. peloria*. Several specimens of the moth, *Neorina shadula* were also obtained.

Dr. Hooker and Mr. Bentham have written the descriptions of the new species of flowering plants, which are figured; they include, from the Tamaricaceæ, *Hololachne shawiana*; from the Compositæ, *Iphiona radiata* and *Saussurea ovata*; and from the Apocynaceæ, *Apocynum hendersonii*. Dr. Dickie of Aberdeen describes the Algæ and Diatomaceæ, and has also named some new forms.

OUR BOOK SHELF

The Internal Parasites of our Domesticated Animals. By T. S. Cobbold, F.R.S. (The *Field* Office.)

In this short and concise work Dr. Cobbold has embodied a series of articles which have appeared from time to time in the *Field*. They, having been originally written for the perusal of the non-scientific public, are put in a simple and elementary manner, and much stress is laid on the practical bearing of the science of helminthology, the true value of which the author clearly shows to be but little appreciated by the growers of stock. Several excellent illustrations accompany the descriptions, which will greatly assist the amateur reader. The entozoa infesting the ox are first described,—flukes, tapeworms, and measle, together with round worms. The importance of more perfect sewage arrangements whereby the ejecta of one animal are not allowed to contaminate the ingesta of another, is laid great stress on. The great carelessness on this point in India evidently leads to the preponderance of parasitic diseases in that country, where the heat and attending thirst cause the frequently small supplies of water to be employed for drink when in a very unfit state, on account of the abundance of ova of parasites that it may contain. A description is also given of the manner in which the Burates or Cossacks of the region of Lake Baikal are nearly all infested with tapeworm, from the custom prevalent amongst them of eating their meat—the flesh of calves, sheep, camels, and horses—in an almost raw condition, and in enormous quantities. We think that there is one point in which this work is particularly suggestive. The great gaps there are in our knowledge of helminthology, such as the imperfect information that can be given as to the source of the liver fluke, must cause most readers who have opportunities at their disposal to wish to develop further a subject which has so many obvious practical bearings on the prosperity of this country; for England in the opinion of many competent authorities is developing more and more into a meat-producing and not seed-growing land. The parasites of the sheep, dog, hog, and cat are those which form the rest of this instructive little volume.

Chapters on Trees: a Popular Account of their Nature and Uses. By Mary and Elizabeth Kirby (London: Cassell, Petter, and Galpin.)

The Amateur's Greenhouse and Conservatory. By Shirley Hibberd. (London: Groombridge and Sons, 1873.)

We have here a brace of books on arboriculture and floriculture, each of which will be welcomed by a certain class of readers, and will fill a useful place in popular scientific literature. Both are written in an agreeable and attractive manner, and are bound and generally got up in a style to suit the drawing-room table. The authoress of the first (or authoresses, for though two names appear on the title-page, the pronoun used is sometimes the first person singular) must not be taken too implicitly as a guide in her scientific and structural details. Many of her statements are, to say the least, very doubtful, and bear the marks of a want of acquaintance with the recent results of botanical science. Passing by this defect, we have a great deal of interesting information and gossip about a great number of our forest-trees. There are also very good descriptions, forming the best part of the book, of many other trees of great economic value with which we are not so familiar, as the ebony, the camphor, the nutmeg-tree, &c. The illustrations—one full-sized one for every tree, besides smaller ones—are, with a few exceptions, excellent.

The second volume, like all Mr. Shirley Hibberd's, contains a great amount of practically useful information on the culture of plants. Indeed anyone who is interested in the matter will find here advice on almost every point connected with the construction and management of plant-houses, and with the selection, cultivation, and improvement of ornamental greenhouse and conservatory plants. There are a large number of woodcuts and some well-executed coloured plates. The book comes, however, more within the range of the gardener than of the scientific student.

Tenth Annual Report of the Belfast Naturalists' Field Club. (Belfast: 1873.)

We are glad to see from the Committee's report that the condition of this club is in every respect satisfactory, both as to numbers, finances, and, most important of all, amount and value of work done by the members. The first part of the Report is concerned with the six summer excursions of the club in 1872, interesting accounts of the history, antiquities, and natural history of the various places visited being given. Of the papers contained in the volume, we mention the following :—"The Lignite of Antrim and their Relation to the True Coal," by Mr. William Gray, in which the author considers the subject both geologically and economically. The Rev. Dr. MacIlwaine, in a paper on "Life," gives an account of the various theories as to the nature of life held by philosophers from the earliest times to the present day. A different aspect of the same subject is discussed in Mr. Robert Smith's paper on "Darwinism," in which the author briefly sketches the nature of the Darwinian theory of development, and gives practical exemplifications of its working in every-day life. Mr. William Gray gives an entertaining account of some of the doings of the notorious "Flint Jack" in Ireland; and the longest paper in the volume, by the Rev. Edmund M'Clure, is one of considerable ethnological value, on "Family Names as indicative of the Distribution of Races in Ireland." The Society offers a considerable number of prizes, competition for which will no doubt tend to encourage the practical study of the various subjects with which the Society is concerned. Altogether it seems to be in a thoroughly healthy condition.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Prof. Young and the Presence of Ruthenium in the Chromosphere.

I HAVE been asked by Prof. Young, of Dartmouth College (U.S.) to say, with reference to the statement made on p. 244 of the third edition of my "Spectrum Analysis" concerning the presence of Ruthenium (Ru) in the solar atmosphere, that possibly by a *lapsus calami* he may have written the symbol (Rb) when giving the information of his discovery to Dr. Huggins, from whom I received a note on the subject.

Although, in accordance with Prof. Young's desire, I make these remarks, I cannot help feeling that they are quite unnecessary, as no one who knows the careful exactitude of Prof. Young's work could for a moment suppose that he was capable of making a confusion between Rubidium and Ruthenium.

H. E. ROSCOE

Owens College, Manchester, Nov. 4

The Miller Casella Thermometer

I WAS surprised on reading Messrs. Negretti and Zambra's letter published in your journal of October 23.

I was under the impression that it had been conclusively established that the principle upon which the Casella-Miller or Miller-Casella Deep-Sea Thermometer is constructed is identical with the one originally made in 1857 by Messrs. Negretti and Zambra at the suggestion of Mr. Glaisher, F.R.S., by the late Admiral Fitz-Roy's directions for the Board of Trade.

I was present when Mr. Scott, F.R.S., Director of the Meteorological Department of the Board of Trade, read a paper upon the subject at the Meteorological Society, January 17, 1872; he said :—"I submitted one of these instruments, made for the late Admiral Fitz-Roy, to hydraulic pressure; it proved good and trustworthy. The history of these instruments was perfectly familiar to many gentlemen interested in deep-sea soundings in 1869."

I may add that I saw the original instrument at the Hydrographic Office ten years ago; in justice I am bound to say that Messrs. Negretti and Zambra were the first manufacturers of a deep-sea thermometer unaffected by pressure.

208, Piccadilly, Oct. 29 P. PASTORELLI

Captain Hutton's "Rallus Modestus"

IN the notice of the current *Ibis*, which appeared in Vol. viii. p. 519, reference is made to a paper by Captain Hutton, contending for the validity of his *Rallus modestus*, as distinct from *R. dieffenbachii*.

The next number of the *Ibis* will contain my reply to Captain Hutton's communication. In the meantime I will merely state that the whole of his argument rests on the assumption that *Rallus dieffenbachii* and *R. philippensis* are the same, in which he is entirely mistaken.

It is a fallacy, therefore, to suppose that because he has shown his bird to be distinct from *Rallus philippensis*, with which he compares it, he has proved it to be distinct from *Rallus dieffenbachii*, which, by his own admission, he has never seen.

Oct. 18 WALTER L. BULLER

Flight of Birds

IN NATURE, vol. viii. p. 86, Mr. J. Guthrie calls attention to, and asks explanation of, a curious phenomenon in the flight of birds observed by him :—"In the face of a strong wind," he says, "the hawk remained *fixed in space* without fluttering a wing for at least two minutes. After a time it quietly changed its position a few feet, with a slight motion of its wings, and then came to rest again as before, remaining as motionless as the rocks around it."

I have often observed the same phenomenon, but, until recently, not carefully enough to warrant any attempt at explanation, though always convinced that it was *not* due to any invisible vibratory motion of the wings, as suggested by Mr. Guthrie. During the past summer, however, while on a tour through the mountains of Oregon. I had a fine opportunity of watching very closely a large red-tailed hawk (*Buteo montanus*) while performing this wonderful feat, and of noting the conditions under which alone, I believe, it is possible. These conditions are precisely those described by Mr. Guthrie, viz., a *steady wind*, blowing across an *upward slope*, terminated by a ridge. For a half-hour I watched the hawk, with wings and tail widely expanded, but motionless, balancing himself in a fixed position for several minutes in the face of a strong wind; then changing his position and again balancing, but always choosing his position just above the ridge.

I explain the phenomenon as follows :—The slope of the hill determines a slight *upward* direction to the wind. The bird inclines the plane of his expanded wings and tail very slightly *downwards*, but the inclination *is less than that of the wind*. Under these conditions it is evident that the tendency of gravity would be to carry the bird *forward and downward*, while the wind would carry him *backward and upward*. The bird skilfully adjusts the plane of his wings and tail, so that these two opposing forces shall exactly balance. He changes his place and position from time to time, not entirely voluntarily, but because the varying force or direction of the wind compels him to seek a new position of equilibrium. JOSEPH LE CONTE

Oakland, Cal., U.S., Sept. 19

Collective Instinct

IN response to the appeal which closes Mr. Buck's interesting letter (NATURE, vol. viii. p. 332), the following instance of "collective instinct" exhibited by an animal closely allied to the wolf, viz., the Indian jackal, deserves to be recorded. It was communicated to me by a gentleman (since deceased) on whose veracity I can depend. This gentleman was waiting in a tree to shoot tigers as they came to drink at a large lake (I forget the district) skirted by a dense jungle, when about midnight, a large Axis deer emerged from the latter, and went to the water's edge. Then it stopped and sniffed the air in the direction of the jungle, as if suspecting the presence of an enemy; apparently satisfied, however, it began to drink, and continued to do so for a most inordinate length of time. When literally swollen with water it turned to go into the jungle, but was met upon its extreme margin by a jackal, which, with a sharp yelp, turned it again into the open. The deer seemed much startled, and ran along the shore for some distance, when it again attempted to enter the jungle, but was again met and driven back in the same manner. The night being calm, my friend could hear this process being repeated time after time—the yelps becoming successively fainter and fainter in the distance, until they became wholly inaudible. The stratagem thus employed was sufficiently evident. The lake having a long narrow shore intervening between it and the jungle, the jackals formed themselves in line along it, while concealed within the extreme edge of the cover;

and waited until the deer was water-logged. Their prey being thus rendered heavy and short-winded, would fall an easy victim if induced to run sufficiently far,—*i.e.* if prevented from entering the jungle. It was, of course, impossible to estimate the number of jackals engaged in this hunt, for it is not unlikely that, as soon as one had done duty at one place, it outran the deer to await it in the another.

A native servant, who accompanied my friend, told him that this was a stratagem habitually employed by the jackals in that place, and that they hunted in sufficient numbers "to leave nothing but the bones." As it is a stratagem which could only be effectual under the peculiar local conditions described, it must appear that this example of collective instinct is due to "separate expression," and not to "inherited habit."

Cases of collective instinct are not of infrequent occurrence among dogs. For the accuracy of the two following I can vouch. A small skye and a large mongrel were in the habit of hunting hares and rabbits upon their own account, the small dog having a good nose and the large one great fleetness. These qualities they combined in the most advantageous manner, the terrier driving the game from the cover towards his fleet-footed companion, which was waiting for it outside.

The second case is remarkable for a display of sly sagacity. A friend of mine in this neighbourhood had a small terrier and a large Newfoundland. One day a shepherd called upon him to say that his dogs had been seen worrying sheep the night before. The gentleman said there must be some mistake, as the Newfoundland had not been unchained. A few days afterwards the shepherd again called with the same complaint, vehemently asserting that he was positive as to the identity of the dogs. Consequently, the owner set one watch upon the kennel, and another outside the sheep-enclosure, directing them (in consequence of what the shepherd had told him) not to interfere with the action of the dogs. After this had been done for several nights in succession, the small dog was observed to come at day-dawn to the place where the large one was chained: the latter immediately slipped his collar, and the two animals made straight for the sheep. Upon arriving at the enclosure the Newfoundland concealed himself behind a hedge, while the terrier drove the sheep towards his ambush, and the fate of one of them was quickly sealed. When their breakfast was finished the dogs returned home, and the large one, thrusting his head into his collar, lay down again as though nothing had happened. Why this animal should have chosen to hunt by stratagem prey which it could easily have run down, I cannot suggest; but there can be little doubt that so wise a dog must have had some good reason.

Dunskaith, Ross-shire, Aug. 18 George J. Romanes

In your number of August 14 (Vol. viii. p. 302) Mr. E. C. Buck alluded to the curious and interesting instances of instinct and gregarious action in lower animals, and mentioned that this action has been more particularly observed in the case of wolves in India. These remarks remind me of a curious instance of combined action between two foxes for the capture of their prey, which I witnessed myself more than once; and as similar proceedings, on the part of these animals have been so frequently observed in the hilly country of the department in which I reside, I cannot but conclude that the same habit will prevail among them, wherever they are found. The case is as follows:—One of the two foxes, in the pursuit of a rabbit or hare, continued yelping at short and regular intervals and thus drove the unsuspecting victim in the direction of the appointed bush, where the other fox was concealed and ready to seize its prey as soon as it came within its reach. The capture being effected, they generally divide the prey between them; but if the ambushed fox, in jumping at its prey, has not gained the end in view, the two baffled compeers alternately repeat many times the unsuccessful leap, in order probably to find out the cause of the miscarriage.

The above allusion to foxes leads me to mention another instance of the ingenuity of these animals, which is very remarkable, and one, I believe, which is but little known. On one occasion, in early life, when I happened to pass my College vacation at the Chapelle d'Angillon (Department of the Cher), my attention was attracted twice or three times, when rambling by the side of a small stream called the Petite-Saudre, by a floating mass of moss, which, when drawn to the bank, was found to be swarming with fleas. An old peasant of the neighbourhood, who observed my surprise, gave me the following explanation of the fact, the correctness of which, said he, he could warrant:—Foxes are much tormented with fleas, and when the infliction becomes severe, they gather, from the bark of trees, moss which they carry in their mouths to the side of a stream where the water deepens by degrees. Here, they enter the water, still carrying the moss in their mouths; and, going backwards beginning from the end of their tail, they advance by slow degrees, till the whole body of the animal, with the exception of the mouth, is entirely immersed. The fleas, during this proceeding, have rushed successively in rapid haste to the dry parts and finally to the moss, and the fox, when he has, according to his calculation, allowed sufficient time for all the fleas to take their departure, quietly opens his mouth. The floating moss, with its interesting freight, is carried away by the stream, and the animal finds its way back to the bank, with an evident feeling of much self-satisfaction at having thus freed himself from his tormentors.

Many persons, and very trustworthy ones, confirmed to me the old peasant's account.

Montpellier, Oct. 17 A. Paladilhe

Venomous Caterpillars

Once before I wrote to you on this subject, and had hoped that the entomological mountain had long since been safely delivered of its mouse. But from recent communications such appears not to be the case.

Any large caterpillar with tolerably stiff hairs that will not, in different degrees, affect tender skin when brought incautiously in contact, may probably be looked upon as a phenomenon. That any larva with stiff spines will occasion inconvenience by more violent contact is, I should think, evident to any thinking naturalist. That inflammatory symptoms will most probably follow in either case is also evident. The puncture made by a single steel filament would occasion little or no inconvenience; but if multitudes of these filaments were simultaneously directed on a limited surface of skin, the result would be very different. The best analogue of the irritation caused by larval hairs is, as I before hinted, to be found in that following the handling of certain boraginaceous plants—*Echium vulgare*, *Symphytum officinale*, &c.

Mr. Riley, the State Entomologist for Missouri, has, in his fifth annual report, devoted a chapter to this subject, and states that he is acquainted with fifteen indigenous larvæ having so-called urticating powers, and in every instance the action is mechanical. Those observers who place so much stress upon the fact of contact with a hairy larva causing pain should not let surprise get the better of their judgment; nor, in the case of those residing abroad, should they allow themselves to be influenced by native superstitions. The position is simply this: any hairy larva is likely to cause irritation mechanically, from particles of the numerous hairs piercing the skin; no case has yet been proved in which such irritation is the result of *venom*, such as that of *Urtica* among plants.

Lewisham, Oct. 10 R. McLachlan

Harmonic Echoes

The phenomenon mentioned by W. G. M. of notes higher in pitch than the sound producing them being reflected from railings, is not at all uncommon, and is very easy of explanation. Suppose a person standing close to a line of upright bars, the distance between the bars being . a . If he now makes any sharp sound, so as to propagate a single wave, this wave will be successively reflected by each of the bars; so that, in answer to the single wave he propagates, he will have an echo of the pitch corresponding to $\frac{V}{2a}$ vibrations per second (V being the velocity of sound). If, however, he stands at any distance, say x, from the row of bars, he ought to get a slightly descending echo, as then each wave succeeds the last at a distance increased by twice the difference between $\sqrt{x^2 + n^2 a^2}$ and $\sqrt{x^2 + (n - 1)^2 a^2}$, where n is the number of the bar measured from opposite the observer. Arnulph Mallock

Brampford Speke, Oct. 13

Evolution as applied to the Chemical Elements

When so little is really known about evolution, even in the sphere of organic matter, where this grand principle was first prominently brought before our notice, it may perhaps seem premature to pursue its action further back in the history of the universe. However, it seems but natural that we should apply this hypothesis to explain the close connection that holds between certain of the so-called elements. Pre-supposing that this theory has not been discussed before, I will just mention the chief grounds for holding it, and leave the examination into its truth or falsity in the hands of more experienced chemists. Herbert Spencer defines evolution as the integration of matter at the expense of force; this integration being accompanied by a loss of polarity, and by specialisation in a certain direction. Thus much being granted let us see how far this change from simple to complex is traceable in the qualities of certain of the elements, as seen especially in those that fall under natural groups.

In the first place, we may call some of the metals more generalised than others. Thus all hydrogen salts are soluble in water; so, to a less extent, are those of lithium, sodium, and potassium; but as the atomic weight (or mass) increases, so the salts of those metals become less and less soluble. This is only true speaking generally, for we see that, in particular cases, the hydrate of barium is more soluble in water than that of calcium, &c. But, as a rule, the salts of barium are less soluble than those of strontium; these, again, than calcium salts. But, on the whole, we may say that with increase of atomic mass of the metals, their salts lose their general properties and become more and more specialised, the salts taking their character from the metal in combination.

Secondly, according to this hypothesis, increase of atomic mass should be accompanied by absorption of motion. Just as the very complex molecules, of which living organisms are built up, are deficient in polarising or crystallising force, so are also the more massive chemical atoms: for it is evident that the heavy atoms of lead and bismuth have far less of this force, called chemical affinity, than have the light sodium, or the still lighter hydrogen atoms. In colloid bodies, the atomic attractions are mostly used up in keeping together the comparatively great masses of the molecule: hence but little polarity, or attraction among the molecules themselves, is manifested, and the compounds from the union of these molecules are unstable. So, too, the more massive atoms of elements enter with more difficulty into combination, and the products formed are unstable. Thus, the chlorides of platinum, or the oxides of lead, &c., are less stable, and more difficult of formation, than the corresponding salts of potassium or magnesium. Whereas colloids and crystalloids readily unite together: this is paralleled by the strong affinity that hydrogen, or any metal, has for chlorine or oxygen. Here the metal is the light crystalloid, the non-metal, the colloid, so to speak. It is only with the more specialised of the metals, those which we have seen have massive atoms, that hydrogen will unite, viz., antimony and arsenic; and the compound it forms with the former is very unstable, whilst the hydride of bismuth is unknown. These compounds are not alloys like that of hydrogen with palladium, but they show the comparatively non-metallic nature of arsenic and antimony. This consideration leads us to suppose that the non-metals are still more highly evolved than the metals, and that in the special direction towards electro-negative polarity. Besides we know that the intermediate links differ in degree, not in kind.

The lessening of the atomic heat with increase of mass shows a further absorption of motion, besides the potential energy possessed by the more massive atoms. It might be objected that motion has never been extracted from these massive atoms; on the contrary, as a rule, the heat of combustion is greater as atoms of the element entering into combustion are lighter. But the molecules of organic matter must be decomposed by suitable means before they can do any work; just so with the elements, which receive their name for the very reason that, as far as we know, they are incapable of decomposition. Perhaps, indeed, the increase in the number of rays in the spectra of highly heated sulphur and nitrogen will be regarded as an instance of such motion.

Thirdly, if we look at the atomic weight of groups of the elements, it is seen that the increase of mass occurs by a simple proportion. Gladstone, Dumas, Odling and others have shown the close relation of the numbers for particular groups; whilst lately Mendelejeff has given out a law of periodical recurrence, connecting the properties and the atomic weight, either received or theoretical, of all known elementary bodies. Thus we have—

$$\left\{\begin{array}{l} \mathrm{Ca} = 40 \\ \mathrm{Ba} = 137 \end{array}\right. \mathrm{Sr} = \frac{40 + 137}{2} = \begin{array}{c} 88\cdot5 \\ (\mathrm{Sr} = 87\cdot5) \end{array}$$

$$\left\{\begin{array}{l} \mathrm{Cl} = 35\cdot5 \\ \mathrm{I} = 127 \end{array}\right. \mathrm{Br} = \frac{35\cdot5 + 127}{2} = \begin{array}{c} 81\cdot25 \\ (\mathrm{Br} = 81) \end{array}$$

$$\left\{\begin{array}{l} \mathrm{Li} = 7 \\ \mathrm{K} = 39\cdot1 \end{array}\right. \mathrm{Na} = \frac{7 + 39\cdot1}{2} = \begin{array}{c} 23\cdot05 \\ (\mathrm{Na} = 23) \end{array}$$

These instances suffice to show how near the calculated atomic weights come to those found by experiment.

In the fourth place it is a significant fact, that the elements themselves become changed in properties under different circumstances; the allotropic forms that result may be said to correspond with "varieties" among organised bodies. In the case of the elements greater atomic mass was said to denote evolution; in the best known allotropic varieties we find change from the normal form to be accompanied by increased density. Thus ozone (allotropic oxygen) and red phosphorus have both a greater density than the usual forms of these bodies.

With greater evolution, the so-called elements become more electro-negative; so in these instances, ozone has a greater affinity for hydrogen and the metals than has oxygen, and amorphous phosphorus less affinity for oxygen than ordinary phosphorus.

The varieties of sulphur would seem to be exceptions, for they are of less density than the usual form; the specific gravity of crystallised sulphur is 2·05, that of plastic sulphur, 1·95. However Berthellot terms the crystallised octagonal variety, electro-negative, plastic sulphur, on the contrary, electro-positive. Hence the octagonal form is at once denser and more electro-negative, and should be regarded accordingly as more highly evolved.

In the fifth place, let us note some of the actions and reactions of matter and forces.

(*a*) Heat: In any organic group, generally speaking, the greater the vapour density, accompanying greater complexity, the higher is the boiling point. So it is with the elements, taken according to natural groups, the greater the atomic weight, the higher the fusing or boiling point. This is seen in the case of chlorine, bromine, and iodine; arsenic, antimony, and bismuth, &c. Exceptions to this rule are the three closely allied metals, zinc, cadmium, and mercury, the most volatile of which is the heaviest, the least volatile, the lightest. Again, the more complex the chemical constitution of bodies is, the worse, generally, do they conduct heat and electricity: so too the more highly evolved and massive the atoms, the worse conductors are they as a rule. This applies strictly only to groups, as calcium conducts better than barium or strontium, but silver, though heavier and of greater atomic weight, nearly five times better than calcium. The difference of conducting power between metals and non-metals is very apparent. Where the atomic mass is greater, as the body verges more towards the electro-negative, this loss of conductibility and the high fusing point is easily accounted for by the mechanics of motion. The heavier atom takes longer to communicate its motion in the one case; or is more difficult to move in the other.

Some natural groups of the elements offer good examples of what has just been stated, *e.g.*

	Atomic Weight	Specific gravity	Solubility of Salts	Electric conductivity
Ca	40·0	1·5	Maximum.	22·14
Sr	87·5	2·5	—	6·71
Ba	137·0	4·5	Minimum.	Minimum.

	Atomic weight.	Physical state	Chemical activity.	Vapour density
Cl	35·5	Gas.	Maximum.	2·4
Br	81·0	Liquid.	—	5·4
I	127·0	Solid.	Minimum.	8·7

Hydrogen has the greatest conducting power of all gases. To the principle that lighter atoms have greater polarity or chemical affinity, Bunsen has found an exception, that cesium is heavier and yet more electro-positive than potassium or sodium.

The order of solubility or of chemical activity shows that chlorine and calcium are the more generalised of their respective groups, as we should expect.

(*b*) In the case of Light, not much can be said as yet: but with regard to radiation and absorption of radiant heat, Tyndall has shown that the complex molecules of organic vapours are the best radiators, and that uncombined atoms can hardly be said to radiate or absorb at all. So we see that the simple, "metallic" vapours radiate but ill, whilst the more complex atoms do not reflect, but rather absorb light and heat rays. Indeed, we may suppose, that as in the case of complex vapours, the more highly evolved atoms, requiring a greater supply of force, turn these rays that fall on them to account; whilst the metals dispense with them by reflecting them.

(*c*) The chief relations of electricity have already been alluded to. The chemical affinity between elements increases as they differ in electric polarity; and the more highly evolved, the more chlorous or electro-negative are they.

Lastly, late researches have shown that the elements nitrogen and sulphur at a high temperature, give more complex spectra. This fact, if it be a fact, has thrown some doubt on their claim to be regarded as absolute elements.

In explaining the phenomenon, we should probably consider the sulphur particle to be composed of several groupings of the ultimate element, which, driven apart by the action of heat, are made to vibrate separately with various velocities. Thus the allotropic form of oxygen, ozone, has been represented by a simple formula $\left.\begin{matrix}O\\O\end{matrix}\right\}O$, being made up, as it is supposed, of two groupings of the element oxygen, that being the ultimate atom.

The above statements seem to me to agree in showing, that if the hypothesis of evolution is tenable at all, it can be extended to explain all or nearly all the relations between the elements at present existing on this globe.

C. T. BLANSHARD

Queen's College, Oxford

Ancient Balances

Apropos of Mr. Chisholm's interesting account of ancient weighing instruments, in your last number, I venture to call his attention to the representation of an equal-armed balance in an Egyptian papyrus of the nineteenth dynasty, about 1350 B.C. It is to be found in the celebrated "Ritual of the Dead," a hieroglyphical papyrus of Hunnefer, of the reign of Seti I. In the "Judgment Scene" the heart of the deceased is represented as being weighed in a balance in the Hall of Perfect Justice, and in the presence of Osiris. The balance is of the ordinary equal-beam construction, the final adjustment being attained by a sliding weight on one side of the beam, exactly like the "rider" on our exact balances. The papyrus may be seen in the British Museum.

G. F. RODWELL

Brilliant Meteors

ON Saturday evening (Oct. 18), about half-past 8 o'clock, I observed, from Boltsburn, Durham, a meteor of considerable brilliancy in the north-western part of the sky; it shot downward from an elevation of about 40°, and left a streak of very red light on its path. The streak continued visible for nine or ten seconds.

JOHN CURRY

Boltsburn, Oct. 20

LAST evening, October 26, when returning home I observed a brilliant meteor stream across the sky. It may be worth while to record it.

Not having my watch, I can only guess the time as about 8.20 P.M. The first appearance was like a flash of lightning intensely white, arresting attention at once. When observed it streamed from ξ Persei above Capella (in altitude) and disappeared in Lynx. For two-thirds of its course its light was very bright, and it left a brilliant train of sparks, but for the remaining third it merely showed its own single expiring light.

Later in the evening when observing with the telescope in Cepheus, two shooting stars crossed the field at different times, apparently from the same radiant.

T. T. S.

Thruxton Rectory, Hereford

SIR HENRY HOLLAND

ALTHOUGH the late Sir Henry Holland, whose name has been familiar to the world during the greater part of the present century, cannot be regarded as a man eminent in scientific research, still, as a Fellow of the Royal Society of nearly sixty years' standing, as President of the Royal Institution, as one who was ever ready to contribute towards the advancement of scientific research, and as the friend of all the most eminent men of science of his time, which was a long one, we deem him worthy of more than a passing notice.

As much as for anything else, Sir Henry was known as an indefatigable traveller; his fondness for travelling, indeed, having led to the illness which was the immediate cause of his death on October 27 last, his 86th birthday. He had very early in his career deliberately determined to set aside two months each year for the purpose of indulging his favourite recreation. This year, immediately after his return from a visit to Russia, he set off for Naples in September last, staying a short time at Rome and Paris on his way home. He arrived in London on October 25, suffering from a slight cold, which was sufficient, notwithstanding the wonderful robustness of his constitution, to cut him off in two days. He began his travelling career by a visit to Iceland in 1810, since which he has explored almost every corner of Europe, and been eight times in America. In his "Recollections of Past Life," published in 1872, he speaks thus of his travels:—

"The Danube I have followed with scarcely an interruption, from its assumed sources at Donau-Eschingen to the Black Sea—the Rhine, now become so familiar to common travel, from the infant stream in the Alps to the 'bifidos tractus et juncta paludibus ora' which Claudius with singular local accuracy describes as the end of Stilicho's river journey. The St. Lawrence I have pursued uninterruptedly for nearly 2,000 miles of its lake and river course. The waters of the Upper Mississippi I have recently navigated for some hundred miles below the Falls of St. Anthony. The Ohio, Susquehanna, Potomac, and Connecticut rivers I have followed far towards their sources; and the Ottawa, grand in its scenery of waterfalls, lakes, forests, and mountain gorges, for 300 miles above Montreal. There has been pleasure to me also in touching upon some single point of a river, and watching the flow of waters which come from unknown springs or find their issue in some remote ocean or sea. I have felt this on the Nile at its time of highest inundation, in crossing the Volga when scarcely wider than the Thames at Oxford, and still more when near the sources of the streams that feed the Euphrates, south of Trebizond."

It was mainly on account of the reputation which even then he had achieved as a traveller, that he was elected a Fellow of the Royal Society in 1815.

Sir Henry was elected President of the Royal Institution in 1865, and took the very warmest interest in its success, and in the promotion of scientific research, being seldom or never absent from his post, doing much to popularise science among the upper classes, among whom, as our readers know, he was always a welcome guest. For fifteen years Sir Henry contributed 40*l*. annually to a fund specially set apart for the promotion of research, and was always ready to take by the hand promising young students who were diffident of their own abilities. Sir Henry himself never knew what it was to struggle, no man ever slid more easily into the highest professional and social position, and no man was ever probably less spoiled by his success. He counted from the very first among his patients, many of whom became his intimate friends, the highest in social and political rank both at home and abroad, and the most eminent in literature, science, and art, knew nearly everyone whose name during the last sixty years has been before the public, and was respected and loved by all with whom he came in contact. Sir Henry had naturally good abilities, great tact and knowledge of the world, a mind stored with knowledge gained from books, from travel, and from his intercourse with men, which, combined with his genial bearing, rendered his society wonderfully delightful. As a physician, he was possessed of high skill.

Of Sir Henry's contributions to literature, his "Medical Notes and Reflections" (1839) and his "Chapters on Mental Physiology" (1852) are well known to the medical profession. He contributed a considerable number of articles to the *Edinburgh*, and other reviews, which, in 1862, were published as "Scientific Essays." In 1815, he published his celebrated "Travels in the Ionian Isles and Greece," of which a second edition appeared in 1819; a work abounding in classical, antiquarian, and statistical information, interspersed with interesting details respecting manners and customs, scenery and natural history. In 1816 he contributed to the "Philosophical Transactions" a memoir on the manufacture of sulphate of magnesia at Monte della Guardia, near Genoa, and afterwards papers to various other scientific journals. Last year he published his well-known "Recollections of Past Life," a volume which must long keep Sir Henry Holland's name alive. His memory will be cherished by all who knew him as something ever pleasant to recall.

The Royal Institution has thus, within a year, lost its Secretary and its President, not to mention the resignation of its Professor of Chemistry, who has not yet been replaced. Whoever is elected to fill the Presidential office will, we doubt not, keep up the traditions of the place, and do what in him lies to carry out the original design of the founders and donors of the Institution, never losing sight of the fact that above everything it is meant to be one of the few temples of original scientific research in the country. Its laboratories have recently been rebuilt, and we hope they will ever continue to be taken ample advantage of for purposes of study and research, not only by the earnest successors of the great men who have rendered them famous, but also by competent members, for whom they were originally equally intended by the enlightened and science-loving men to whom the conception of the Institution was originally due.

We conclude this notice by giving a few of the dates, in addition to those already given, which mark Sir Henry Holland's career. He was born at Knutsford, Cheshire, Oct. 27, 1787, and was educated at Newcastle-on-Tyne, and at the school of Dr. Estlin, near Bristol, where he became head boy. In 1804 and 1805 he attended Glasgow University, and in 1806 he entered the Medical School at Edinburgh, where he became acquainted with many of the notable men that then frequented "the grey metropolis of the north"—Sir Walter Scott, Brougham, Sydney Smith, Horner, Jeffery, Dugald Stewart, Sir William Hamilton. In 1816, after spending some time in travel, he established himself in London, and at once achieved high professional success. He became Physician in Ordinary to the late Prince Consort in 1840, and to the Queen in 1852; and next year was created baronet. Sir Henry was twice married, his second wife, who died in 1866, having been the daughter of his old friend Sydney Smith.

THE AMERICAN MUSEUM OF NATURAL HISTORY IN CENTRAL PARK NEW YORK[*]

FOR many years a large number of the generous and public-spirited citizens of New York had long felt the need of a museum and library of natural history that should be on a scale commensurate with the wealth and importance of their metropolitan city, and would encourage and develop the study of natural history, advance the general knowledge of kindred subjects, and to this end furnish popular amusement and instruction. In 1868 a remarkable opportunity presented itself of securing a rare collection that would form an admirable nucleus for such a comprehensive museum. The most extensive dealer in specimens in the world, Edouard Verreaux, of Paris, suddenly died, leaving in the hands of his widow a collection, which, at the rates he was accustomed to sell specimens, would have brought over 500,000 francs, 100,000 dols. in gold .... Dying suddenly, he left the rich gatherings of an industrious lifetime seriously embarrassed with debt. This opportunity it was decided to try to improve, and a subscription of nearly 50,000 dols. was at once made up as a beginning, and since that time about 100,000 dols. have been contributed in money, though the present property of the institution, including the large donations of specimens which have been steadily coming in, could not be replaced, nor could other as interesting and valuable specimens for less than 250,000. A rare and nearly complete collection of American birds, and many fine birds of paradise and pheasants were first purchased by Mr. D. G. Elliott. While negotiations were about to be opened for the Verreaux collection, a second museum unexpectedly became available. Prince Maximilian of Neuwied on the Rhine above Bonn (not the Emperor Maximilian of Austria and Mexico) died, and the young son inheriting the estate had no scientific taste, and offered the results of his father's life-work for sale. The elder Prince, who formed the collection, passed 1815, 1816, and 1817 exploring Brazil from Rio up to Bahia, and of course a large proportion of the great collections he secured had never at that early date been seen by scientific men in Europe before, and were therefore types of new species.

This collection the American Museum purchased entire. An agreement was soon after made with Mme. Verreaux by which all the choice specimens in her cabinet not contained in the Elliott and Maximilian purchases were selected for the museum, and all these specimens have been safely received from Europe, and are now on public exhibition in Central Park. Large donations of shells, corals, and minerals have been received, and one collection of 20,000 insects. The liberal subscriptions first made induced the principal subscribers to consent to act as trustees for the fund and property acquired by it, and by a special Act of the Legislature they were created a body corporate—they and their successors to have entire and unrestricted control for ever over all the museum property. They have limited their number to twenty-five, and the survivors fill every vacancy, thus securing a fixed policy and stable character to the institution. An arrangement has been made between the trustees and the Department of Public Parks in New York by which the city may furnish lands and buildings, while the collections are to be bought and cared for by moneys contributed by the trustees themselves and the generous public. In pursuance of this plan, by which the authorities of the city and private citizens might cooperate toward the common end of establishing a large museum, 500,000 dols. were appropriated by the city to commence a suitable thoroughly fire-proof edifice, and the Department of Parks was authorised to set apart so much of the public lands under their control as they might deem proper and necessary for the proposed structure and its future extensions.

The great object of the museum is twofold. First, to interest and instruct the masses which already throng its halls, and occasionally number over 10,000 in a single day; and, secondly, and especially to render all the assistance possible to specialists. These wants are shown to be amply met by the large, palatial saloons for the public, and over the whole building a high Mansard story, containing spacious and well-lighted rooms with every modern convenience, where naturalists from every part of the country may pursue their favourite studies for any length of time, and be secure from all possible interruptions. The building will undoubtedly be ready for occupation in the spring of 1875.

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[*] A Paper read by Albert L. Bickmore, Ph. D., Superintendent, at the Meeting of the American Association.

THE COMMON FROG *

III.

TO prosecute successfully our inquiry "What is a Frog?" it will be well now to make acquaintance with the more remarkable forms contained in its *Order*, after which, by considering the other Batrachian orders, we may arrive at a certain appreciation of its *Class*.

The Frog's own genus (Rana), which contains about 40 species, has its head-quarters in the East Indies and in Africa, but extends over all the great regions of the world, except Australia, and parts more southerly still, and except countries situate above 66° north latitude. In South America, however, but a single species is as yet known to exist.

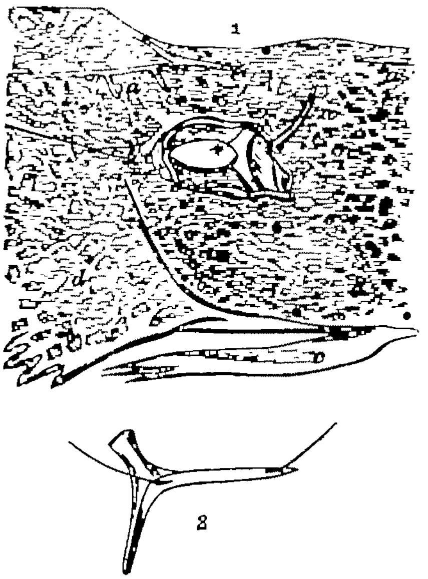

FIG. 7.—Poison Organ of *Thalassophryne reticulata* (after Gunther). 1, Hinder half of the head with the venom-sac of the opercular apparatus *in situ*. * Place where the small opening in the sac has been observed. *a*, Lateral line and its branches; *b*, gill-opening; *c*, central fin; *d*, base of pectoral fin; *e*, base of dorsal fin. 2. Operculum, with the perforated spine.

Amongst the largest species are *Rana tigrina*, of India and the Indian Archipelago, and the bull-frog (*R. Mugiens*) of North America. The latter animal may often be seen in the Gardens of the Zoological Society, where it is fed on small birds—a sparrow being easily engulphed within its capacious jaws.

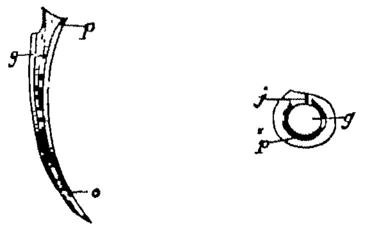

FIG. 8. FIG. 9.

FIG. 8.—Vertical, Longitudinal Section of the Poison-fang of a Serpent (after Owen). *g*, deep groove; *o*, its lower termination, which affords exit to the poison; *p*, pulp-cavity. FIG. 9.—Magnified Transverse Section of a Serpent's Poison-fang (after Owen). *g*, groove round which the substance of the tooth (containing *p*, the pulp-cavity) is bent; *j*, the point where the sides of the tooth meet and convert the "groove" into what is practically a central cavity.

The Edible Frog, *par excellence* (*R. esculenta*), is found in England as well as on the Continent of Europe. It is as widely distributed over the old world as is *R. temporaria*, but it is unknown in America. It is easily to be discriminated from the common species (see Fig. 4 on p. 510) by the absence of that dark, sub-triangular patch which extends backwards from the eye in *R. temporaria*.

The male of *R. esculenta* is further to be distinguished from the male of the common Frog by the fact of its having the floor of the mouth on each side, distensible as a pouch—the pouches, when distended, standing out on each side of the head. These pouches are called "vocal sacs," and no doubt aid in intensifying these animals' croak, which is so powerful that (on account of it, and because of the country where they are common) they have been nicknamed "Cambridgeshire Nightingales." Specimens from Cambridgeshire are preserved in the British Museum.

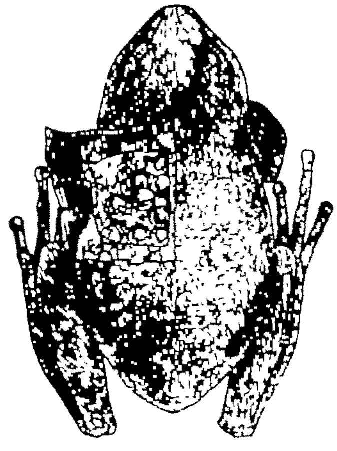

FIG. 10.—The female of *Nototrema marsupiatum*, with the pouch partly cut open (after Gunther).

A large South American Frog (*Ceratophrys cornuta*), which devours other smaller Frogs as well as small birds and beasts, is noteworthy on account of the singular bony plates which are enclosed in the skin of its back: a character which it shares with a small South American Toad (*Brachycephalus ephippium*), and which we shall hereafter see to be a point of special interest.

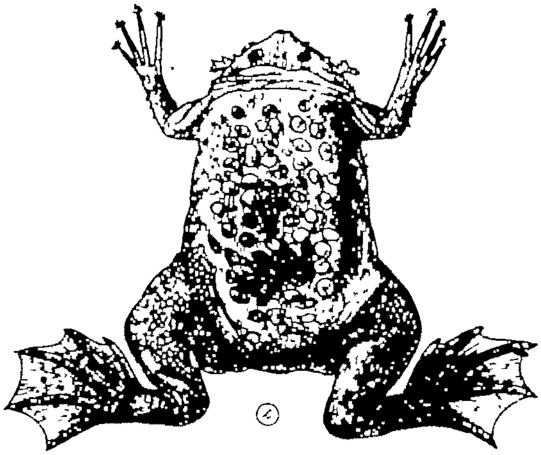

FIG. 11.—The Surinam Toad (*Pipa americana*).

A Frog newly discovered* (of a new genus but one allied to *Rana*), called *Clinotarsus*,† has been

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* Continued from vol. viii p. 512.

* The type of this genus is a species which was in my own collection (with no clue to the locality whence it originally came), but is now deposited in the British Museum. It was first described in the Proceedings of the Zoological Society for 1868, under the name *Pachybatrachus*.

† Proc. Zool. Soc., 1869.

(see Fig. 5, vol. viii. p. 511) represented, in the hope that by the wider circulation of a figure of it, it may be recognised, and its habitat so ascertained.

The common Toad (*Bufo vulgaris*) is as widely distributed over the earth's surface as is *Rana esculenta.* It is less aquatic than the frog, and more sluggish in its motions. In shape it resembles the frog, but is more swollen, with much shorter legs and a warty skin (see Fig. 6, vol. viii. p. 511). The toes are less webbed, and the margin of the upper jaw, as well as the lower, is entirely destitute of teeth. The jaws are similarly toothless in *all* toads.

The toad is provided with an oblong, elongated gland called *Parotoid*) behind each eye. These glands emit a milky secretion which is acrid and very unpleasant to the mouth of some carnivorous animals. Those who have observed a dog attacking a toad can hardly have failed to notice the disgust which the former animal seems to exhibit by the copious flow of its saliva, its many head-shakings, &c. The toad's secretion, however, cannot be said to be poisonous, and certainly it is not so in the mode in which the venom of serpents is poisonous, since a chicken may be inoculated with it, and yet appear to suffer no injury whatever beyond the infliction of the slight wound necessary for the performance of the operation. Nevertheless the secretion exercises a very decided effect upon certain animals, since the tadpoles both of frogs and of salamanders are very powerfully affected by being kept in the same water with a toad, if the latter be specially irritated in order to make it discharge its pungent and irritating secretion.

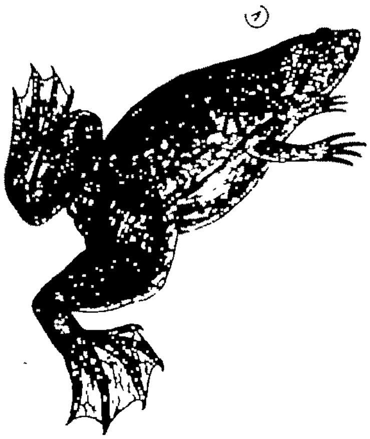
FIG. 12.—*Dactylethra capensis.*

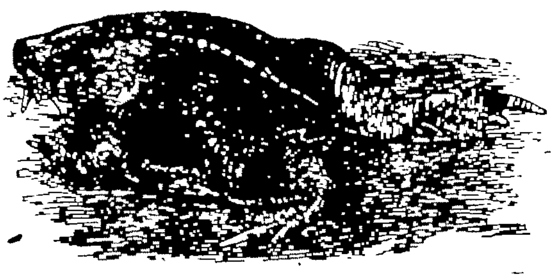
FIG. 13.—*Rhinophrynus dorsalis.*

True poison and organs fitted both to inflict wounds and to convey the venom into them are not indeed found in any animals which are even near allies of the frogs and toads. Nevertheless a very perfect organ for both wounding and poisoning has been discovered by Dr. Günther to exist in a certain fish (*Thalassophryne reticulata*), belonging to a group which, on account of their superficial resemblances to frogs, are termed "Batrachoid."

He found in the fish no less than four spines each perforated like the tooth of a viper, and each having a sac at its base. One such poison-spine was situated on each side of the hinder part of the head in front of the gill opening. Two others were dorsal spines placed one behind the other on the mid-line of the back. These

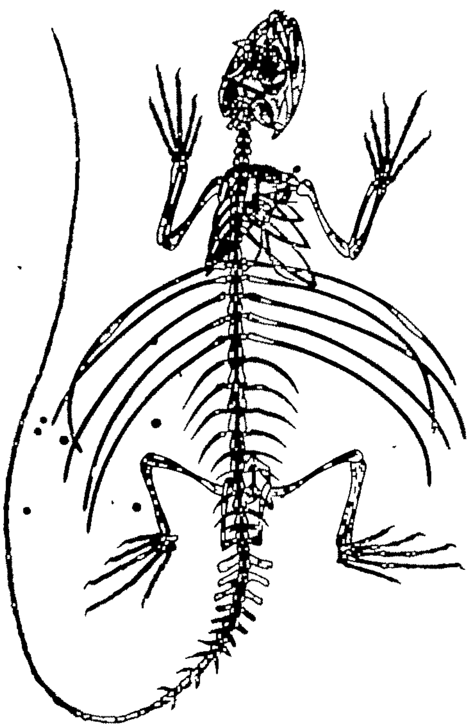
FIG. 14.—Skeleton of the Flying-dragon.
(Showing the elongated ribs which support the flitting organ.)

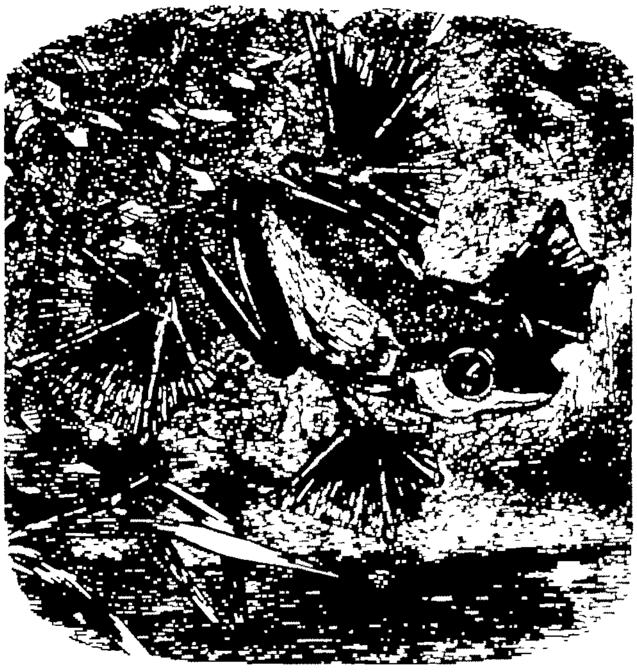
FIG. 15.—The Flying-frog (from Wallace's "Malay Archipelago")

poison-organs are probably only used for defence. They are formed, however, on the very same type as are the poison fangs of vipers. Unlike the latter, however, they are not modified teeth, nor are they situated within the mouth as they always are in poisonous serpents.

A Frog (*Pelobates fuscus*) which is common in France (and which is interesting on account of the form of its skull hereafter to be pointed out), though really harmless enough, has a singular power of making itself offensive.

Both males and females of this species utter a kind of croak, and both, if their thigh is pinched, produce a sound like the mewing of a cat. At the same time they emit a strong odour, which is like that of garlic, and becomes stronger as the animals are more disturbed. This emission not only affects the sense of smell, but even makes the eyes water as mustard or horseradish does.

This singular power, together with the acrid secretion of the toad, are the nearest approximation to venomous properties possessed by any members of the order, no toad—not even the giant of the order *Bufo agua*—being really poisonous.

A small Frog, by no means uncommon in France and Germany (*Alytes obstetricans*) has a very singular habit. The female lays its eggs (about sixty in number) in a long chain, the ova adhering successively to one another by their tenacious investment. The male twines this long chaplet round his thighs, so that he acquires the appearance of a courtier of the time of James I. arrayed in trunk hose or puffed breeches. Thus encumbered, he retires into some burrow (at least during the day) till the period when the young are ripe for quitting the egg Then he seeks water, into which he has not plunged many minutes when the young burst forth and swim away, and he, having disencumbered himself of the remains of the ova, resumes his normal appearance.

Certain Frogs (forming a very large group) are termed Tree-frogs, from their adaptation to arboreal life by means of the dilatation of the ends of the digits into sucking discs, by which they can adhere to leaves. One of them, the common green Tree-frog (*Hyla arborea*) is spread over Europe, Asia, and Africa, in the same manner as *R. esculenta*, except that it is not found in the British Isles. A few toads also have the tips of their digits similarly dilated. Such, *e.g.*, is the case in the genera *Kaloula* of India, and *Brachymerus* of South Africa.

The female of a peculiar American Tree-frog (*Nototrema marsupiatum*) has a pouch extending over the whole of the back and opening posteriorly. Into this the eggs are introduced for shelter and protection. A dorsal pouch also exists in the allied American genus, *Opisthodelphys*. An American species of *Hylodes* has the habit of laying its eggs in trees singly in the axils of leaves, and the only water they can obtain is the drop or two which may from time to time be there retained.

A still more remarkable mode of protecting the egg is developed by the Great Toad of tropical America (*Pipa americana*). In this case the skin of the females' back at the laying season thickens greatly and becomes of quite a soft and loose texture. The male, as soon as the eggs are laid, takes them and imbeds them in this thick, soft skin, which closes over them. Each egg then undergoes its process of development so enclosed, and the tadpole stage is, in this animal, passed within the egg, so that the young toads emerge from the dorsal cells of the mother completely developed miniatures of the adult. As many as 120 of these dorsal cells have been counted on the back of a single individual.

The only instance of a similar cutaneous modification is that pointed out by Dr. Gunther* in the skin of the belly of the Siluroid fish, *Aspredo batrachus*. Here he found that "the whole lower surface of the belly, thorax, throat, and even a portion of the pectoral fins, showed numerous shallow, round impressions, to which a part of the ova still adhered." He concludes that "it is more than probable that towards the spawning time the skin of the lower parts becomes spongy, and that, after having deposited the eggs, the female attaches them to it by merely lying over them." "When the eggs are hatched the excrescences disappear, and the skin of the belly becomes smooth as before. Even in the highest class of animals (*Mammalia*) we are familiar, in the Kangaroo and Opossum order (*Marsupialia*), with a special external receptacle (the marsupial pouch) for the protection and secure development of the young; but nothing of the kind exists amongst birds or reptiles. In fishes, however, the male of the little Sea-horse (*Hippocampus*) is provided with a ventral pouch in which the eggs are sheltered, and the same class presents us with a mode of carrying the eggs still more bizarre than that of *Alytes obstetricans* just related. In the fish *Arius fissus* the male actually carries about the ova in the mouth, protected by the jaws, till relieved of the inconvenience by the hatching of the young fry.

A South African Toad (*Dactylethra capensis*) is interesting, as we shall hereafter see, on account of certain anatomical points in which it agrees with *Pipa*, and differs from all other Anoura. No interesting facts, however, are known as to its habits.

Another noteworthy form is the Mexican *Rhinophrynus dorsalis*, the exceptional characters of which are the tongue, which is free in front instead of behind, and the enormous spur-like tarsal tubercle.

Almost all Frogs and Toads pass the first stages of their existence in water, going through a free, tadpole stage, and all are more or less aquatic when adult. The only exceptions are *Pipa*, *Nototrema*, *Opisthodelphys*, and the *Hylodes* before referred to. Very many kinds, however, are, when adult, inhabitants of trees. The question may suggest itself to some, "Are there any which can be said in any sense to be aërial animals?" Birds are almost all capable of true flight, as also are those aerial existing beasts the Bats, and as were those extinct reptiles the Pterodactyles. Certain squirrels and opossums can take flitting jumps by means of an extension of the skin of the flank, and a similar, though much greater extension, supported by elongated freely ending ribs, is found in the little lizards (*Draco*) called Flying Dragons.

The class of Fishes supplies us, also, with an example of aërial locomotion in the well-known Flying Fish.

Since, then, every other class of vertebrate animals (Beasts, Birds, Reptiles and Fishes) presents us with more or fewer examples of the aërial species, we might perhaps expect that the Frog-class would also exhibit some forms fitted for progression through the air. We cannot say with certainty that such is the case; but Mr. Alfred Wallace, in his travels in the Malay Archipelago, encountered in Borneo a Tree-frog (*Rhacophorus*) to which he considers the term "flying" may fairly be applied, and of which he says, it "is the first instance known of a flying-frog." Of this animal he gives us the following account:—

"One of the most curious and interesting creatures which I met with in Borneo was a large tree-frog which was brought me by one of the Chinese workmen. He assured me that he had seen it come down, in a slanting direction, from a high tree as if it flew. On examining it I found the toes very long and fully webbed to their extremity, so that, when expanded, they offered a surface much larger than the body. The fore-legs were also bordered by a membrane, and the body was capable of considerable inflation. The back and limbs were of a very deep shining green colour, the under surface and the inner toes yellow, while the webs were black rayed with yellow. The body was about four inches long, while the webs of each hind foot, when fully expanded, covered a surface of four square inches, and the webs of all the feet together about twelve square inches. As the extremities of the toes have dilated discs for adhesion, showing the creature to be a true tree-frog, it is difficult to imagine that this immense membrane of the toes can be for the purpose of swimming only, and the account of the Chinaman that it flew down from the tree becomes more credible."

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* See Catalogue of the fishes in the British Museum, vol. v. p. 268.

The great group of Frogs and Toads, rich as it is in genera and species, and widely as it is diffused over the earth's surface, is one of singular uniformity of structure. The forms most aberrant from our type, the common frog, have now been noticed, except that perhaps the maximum respectively of obesity and slenderness may be referred to. In the former respect the Indian Toad *Glyphoglossus* may serve as an example, and for the latter may be selected *Hylorana jerboa.*

St. George Mivart

(*To be continued.*)

A FOSSIL SIRENIAN FROM THE RED CRAG OF SUFFOLK

At the opening meeting of the Geological Society, Prof. Flower communicated a description of a fine fragment of a skull of an animal of the order *Sirenia,* which is of great interest as affording the first recorded evidence of the former existence of animals of this remarkable group in Britain. The specimen forms part of the very rich collection of Crag fossils formed by the Rev. H. Canham, of Waldringfield, near Woodbridge. It was found in the so-called "coprolite" or bone-bed at the base of the Red crag, and presents the usual aspect of the mammalian remains from that bed, being heavily mineralised, of a rich dark brown colour, almost black in some parts, with the surface much worn and polished, and marked here and there with the characteristic round or oval shallow pits, the supposed *Pholas* boring.

The fragment consists of the anterior or facial portion of the cranium which has separated, probably before fossilisation, from the posterior part at the fronto-parietal suture, and in a line descending vertically therefrom. This portion has then been subjected to severe attrition, by which the greater part of the pre-maxillary rostrum, the orbital processes of the maxillaries, and other projecting parts have been removed. In consequence of this, what may be called the external features of the skull, which are especially necessary to determine its closer affinities, are greatly marred, though enough remains of its essential structure to pronounce with confidence as to its general relationship to known forms. Fortunately, the whole of the portion of the maxillæ in which the molar series of teeth are implanted is preserved; and though the teeth have fallen from the alveoli in the front part of the series, and in the posterior part are ground down to mere stumps, so that the form of the crowns cannot be ascertained in any, many important dental characters may still be deduced from the number, form, size and position of the sockets and roots that remain.

As the intensely hard, ivory-like rostra of the ziphioid Cetaceans, the tympanic bones of the Balænidæ, and the teeth of terrestrial mammals almost alone remain in these deposits to attest the former existence of their owners; it is, doubtless, to the extreme massiveness and density of the cranial bones, as characteristic of the order Sirenia, that we owe the preservation of so large a portion of the skull under the very unfavourable conditions to which it, in common with the other fossils of the formation, must have been exposed.

After a comparison of the characters of the cranium with those of the several existing and extinct members of the order, Prof. Flower referred it to the genus *Halitherium,* and showed its relationship to *H. Schinzi* of Kaup from the miocene of the Rhine basin, a formation, it will be remembered, in which several of the animals of the Red Crag bone-bed occur. It is, however, of larger size than that species, the teeth are larger, both absolutely and relatively to the cranium, and certain other differences occur, though the imperfect nature of the materials makes exact comparison of fossils only known from fragments not altogether easy or satisfactory. Believing, however, that it does not belong to either the above-mentioned, or any other of the hitherto described species of *Halitherium,* the specific name of *H. canhami* was proposed. It should be mentioned that there are six teeth in the maxillary or molar series on each side, all present at the same time, the first two with single roots, the third with two roots, and the last three with three roots, precisely resembling in form those of the molar teeth in the existing Manati.

ON THE STICK-FISH (*Osteocella septentrionalis*) AND ON THE HABITS OF SEA-PENS

Mr. Coote M. Chambers has most kindly presented to the British Museum a specimen of the Stick-fish, from English Bay, Burrard's Inlet, British America. The specimen was placed alive, immediately it was caught, into a tin tube, filled with a solution of arsenic and salt.

Mr. Chambers observes that the Stick-fish are only to be found in Burrard's Inlet, English Bay, British Columbia. "It has only one bone in it, and appears to live on suction, and is a great prey to dogfish." Further. "I would mention that in summer only can they be caught. They are found to the least depth of from 30 to 40 fathoms, they move about rapidly in the water, and when brought to the surface, move for a few seconds like a snake, then make a dart as swift as lightning, and disappear."—July 23, 1873.

Unfortunately the specimen did not arrive in a good state for exhibition. The greater part of the animal portion had been washed off, probably by the motion of the solution during the transit; only about a foot of the flesh, which was loose on the axis, and the thick, swollen, naked, club-shaped base without polypes remained; but it was in a sufficiently good state to afford the means of determining its zoological situation and of examining its microscopical and other zoological characters.

Mr. Chambers' specimen is the animal of the axis, or stick, that I described as *Osteocella septentrionalis* (Ann. and Mag. Nat. Hist. 1872, ix. p. 406), and it proves that the axis belongs to a kind of *Pennatula,* or Sea-pen, nearly allied to the long Sea-rushes named *Pavonarius quadrangularis,* found on the West Coast of Scotland, and is evidently the same animal as *Pavonaria blakei,* described by R. E. C. Stearns. The idea of its being a fish, which seems so generally entertained by the people of British Columbia, is clearly a mistake, though one of the observers sent a figure of the Sea-pen, with mouth and eyes like an eel (!), which is copied in Nature, vol. vi. p. 436.

Osteocella.—The complete polype-mass very closely resembles *Pavonaria quadrangularis,* as figured by Johnston ("British Zoophytes," t. xxxi.), from Prof. Edward Forbes' drawings; but the animal is entirely destitute of calcareous spicules, and the axis is cylindrical, hard, and polished.

Two days after I received this specimen, I received by post Mr. Stearn's description of the Stick-fish (*Pavonaria Blakei*), from the San Francisco *Mining and Scientific Press,* August 9, 1873.

The description of Mr Stearn, made from a fresh animal, need not be repeated; but as he does not mention the microscopic structure, I sent a fragment of Mr. Chambers' specimen to Mr. Carter to be examined, who kindly writes:—"The fragment arrived safely, although the Post-office tried to crush the box to the thickness of silver-paper. The bit contains no spicules, nothing but a mass of contorted tubes filled with small nuclei like ova. "The nuclei are about 1-600th of an inch in diameter, and I suppose they are in tubes. The part you sent was boiled in *Liquor potassæ*; that is how the structure alone came out, but there were no spicules in it, examined in this way or in water alone, but many fat globules, and a few sheaf-shaped calcareous concretions, common in all preparations of animal matter."—September 5, 1873.

The habits of *Pennatulidæ* are very imperfectly known and not at all understood. Dr. Johnston observes in the "British Zoophytes," vol. i. p. 160, that the fishermen believe that the common Sea Pens, which they call Coxcombs, "are fixed to the bottom with their ends immersed in the mud." The *Virgularia mirabilis* are believed by the fishermen to have one end erect in the mud, and *Pavonaria quadrangularis*, according to Professor Forbes, "lives erect, its lower extremity, as it were, rooted in the slimy mud at a depth of from twelve to fifteen fathoms." Mr. Darwin, who observed a species on the coast of Patagonia, which he called *Virgularia patagonica*, says: "At low water hundreds of these zoophytes may be seen projecting like stubble, with the truncate end upwards a few inches above the surface of the muddy sand. When touched or pulled they suddenly drew themselves in with force so as nearly or quite to disappear. By this action the highly elastic axis must be bent at the lower extremity, where it is naturally slightly curved, and I imagine it is by this elasticity alone that the zoophyte is enabled to rise again through the mud."

Bohadsch, as quoted by Johnston, says that the *Pennatulæ* swim by means of their *pinnæ*, which they use in the same manner that fishes do their fins. Ellis says: "It is an animal that swims freely about in the sea, many of them having a muscular motion as they swim along." And in another place he tells us, that "these motions are effected by means of the pinnules or feather-like fins, these are evidently designed by nature to move the animal backwards and forwards in the sea, consequently to do the office of fins." Mr. Clifton describes the Australian species as swimming rapidly in shallow water; and the American naturalists all seem to agree that the Stick Fish, *Osteocella septentrionalis* of Burrard Inlet, which has only a slight crest of polyps, and not *pinnæ*, or fins, as Ellis calls them, swims about like a fish, and is eaten by the dog-fish.

There seems to be no doubt that the Sea-Pens and Sea-Rushes do live in groups together, erect, and sunk in the mud, and that they are sometimes found swimming free in the sea, but the question is, are the free specimens those that have been disturbed by the waves and currents, and do they afterwards affix themselves in the mud, or are they vagrant specimens that live for a time and then die or are eaten by fish, their struggling being mistaken for swimming? Dr. Johnston observes, that when the Sea-Pens are placed in a basin or plate of water, he never observed a change of position, but they remain in the same place and lie with the same side up or down just as they have been put in. That is my own experience even when they are placed in a deeper vessel, but this may arise from the animal having lost part of its vitality before it was taken.

It may be useful to give the synonyma of these animals.

Osteocella, Gray, Cat. Pennatulidæ, 1870, p. 40. Ann. and Mag. Nat Hist. 1872, ix. p. 405.

Pavonaria, sp. Stearns, *Mining and Scientific Press*. San Francisco, Aug. 9, 1873.

Verillia, Stearns, Californian Acad. Sci., Aug. 18, 1873.

1. *Osteocella cliftoni*, Gray, Cat. Pennatulidæ, 1870, p. 40; Ann. and Mag. Nat. Hist. 1872, ix. p. 406.

Hab., Western Australia (G. Clifton, Esq.), B.M.

2. *Osteocella septentrionalis*, Gray, Ann. and Mag. Nat. Hist. 1873, ix. p. 406 (style only).

"New Marine Animal," Sclater, Brit. Assoc., Aug. 20 1872; NATURE, vol. vi. p. 436 (with figure of fish, of which it is said to be the notochord).

"Axis of Pennatulid," H. N. Moseley, NATURE, Sept. 26, 1872, vol. vi. p. 432.

"Pennatulid," Dawson, NATURE, Oct. 24, 1872, vol. vi. p. 516; Whiteaves, Nat. Hist. Soc. Montreal, 1872.

"New Alcyonoid," Stearns, Proc. Cal. Acad. Sci., Feb. 1873, v. part 1, p. 7.

Pavonaria blakei, Stearns, *Mining and Scientific Press*, San Francisco, Aug. 9, 1873.

Verrillia blakei, Stearns, Proc. Acad. Cal. Acad. Sci., Aug. 18, 1873.

Hab., Gulf of Georgia, Barraud's Creek, near New Westminster, Washington Territory: Herd, Claudet, Doane, Stearns, Chambers. Fraser's River: Dick and Nelson. B.M.

Mr. Stearns's paper in the Proceedings of the Californian Academy of Sciences is a reprint of the paper in the San Francisco *Mining and Scientific Press*, with a few additions, and the addition of a new sub-genus, *Verrillia*, although he quotes *Osteocella*.

Since I have seen the proof of this paper, the Hon. Justice Crease has informed me that he has forwarded to me a series of the animals of *Osteocella*, and also an account of the animal from an examination of fresh examples by Dr. Moss; the latter has arrived, and I communicated it on September 25 to the Zoological Society; it is illustrated by figures.

J. E. GRAY

THE RELATION OF MAN TO THE ICE-SHEET IN THE NORTH OF ENGLAND

IN the interesting review of Sir Charles Lyell's "Antiquity of Man," communicated to NATURE of Oct. 2, Mr. A. R. Wallace mentions the fact that "there is as yet no clear evidence that man lived in Europe before the Glacial Epoch, and even if he did so, the action of the ice-sheet would probably have obliterated all records of his existence." The fact was true when it appeared, but both the fact and the remark which follows it, may now have to undergo considerable modification. The Committee for the Exploration of the Victoria Cave, near Settle, Yorkshire, assisted by a grant from the British Association, have just made a discovery which may prove to be of the greatest importance not only to the geologists of Europe, but to all those who take an interest in the origin and early history of man.

In May 1872 the Committee were exploring a bone bed in the cave, which occurred at a considerable depth beneath other deposits. It was full of hyæna-dung, broken bones, and teeth. A quantity of these were sent to Mr. Busk for determination, and he kindly returned the following list:—

Elephas primigenius
Ursus spelæus
Ursus priscus
Hyæna spelæa
Rhinoceros tichorinus
Bison
Cervus elaphus

These are well known to represent the fauna of the river gravels in the south of England. Among them was a bone which puzzled even Mr. Busk, and he has only now given his mature and definite opinion on the subject. He writes: "The bone is, I have now no doubt, human; a portion of an unusually clumsy fibula, and in that respect not unlike the same bone in the Mentone skeleton." When Mr. Busk has taken some time to consider the question there are few scientific men who will dispute his verdict. The occurrence of the bones of man with this group of animals is a new fact for this part of the kingdom, but one that might be expected from a similar co-existence in the south of England, in Kent's Cavern, Wookey Hole, and elsewhere.

But at Settle this discovery possesses a far greater interest from the evidence there of the relation of these animals and man to the great ice-sheet. This hyæna-bed dips into the cave, and has been worked only a short distance from its mouth; but at the mouth itself, vertically under the farthest projection of the overhanging cliff, lies a bed of stiff glacial clay containing ice-scratched boulders. This bed dips outwards at an angle of about 40°, and evidently lies on the edges of the beds containing man and the older mammals. It has been suggested that it may have fallen from the cliff above, and therefore may not necessarily have come into its position in glacial times, but, on a careful consideration, this is quite impossible. Upon it lies a great thickness of talus or scree, which is made up of fragments of limestone split off from the cliff above by the frosts of successive winters. If all this were now removed it would be barely possible for the glacial drift to fall from the cliff above to its present position, but if all the talus were restored to the cliff, of which it forms the waste, such a fall would be impossible. It is quite clear, from the waste of the cliffs which has taken place since the glacial drift came where it now lies, that the cliff then projected many feet farther out and would prevent such a fall.

A strong argument lies also in the fact that the loose talus all lies above the drift and is quite free from mud, whereas all the deposits below it are heavily charged with it, and the mud is just such a fine impalpable stiff mud as would result from the grinding of glaciers and the flow of glacier water. It seems probable that the drift is really the remnant of a moraine lateral or *profonde*, left here by a glacier or an ice-sheet, and that the remains of the older mammals and of man disinterred from beneath it are of an age at any rate previous to the great ice-sheet of the Irish Sea basin. But there is another line of argument which tends to the same conclusion. Three years ago it was believed by most geologists that the fauna here disinterred had never existed in this particular area—and why? because their remains had never been found in any of the river deposits of the district. It was supposed that the great extension of the ice prevented their migration hither. It is clear, now that we have found these remains in caves, that they must have peopled the northern district at one time as thickly as they did the south of England, where their bones are so common in river gravel. But their remains in the northern district occur now only in caverns, and have been removed from the open country. When we compare this removal of the mammoth-fauna over certain districts with the presence of evidence of land glaciation on a great scale, we begin to see that they bear a definite relation to one another, and that the ice-sheet was the great "besom of destruction" which swept away all remains of the older inhabitants from those portions of the country adjacent to the great ice centres.*

Again, there is another matter relating to this question which has hardly received the attention which it deserves. This is the complete absence of palæolithic implements and the fauna which is usually associated with them in the river gravels of the south, over co-extensive areas of the north of England, indicating the removal of palæolithic man from those areas by the ice-sheet. If I am not much mistaken, this discovery at Settle may have an important bearing in several ways. It will carry back the proofs of the antiquity of man to a time previous to the ice-sheet, that is to interglacial if not to preglacial times. It will corroborate the opinions expressed by Mr. Godwin Austen, Mr. James Geikie, and others, that the older valley gravels of the south of England are not of an age subsequent to the Till of the North. And it will give some support to the views of Messrs. Searles Wood and Harmer, that the Till of the north-west of England, though older than the great submergence, is probably of younger date than the greater part of the drifts of the east coast.

The Cave Committee will continue their work with redoubled vigour. It is much to be hoped that the scientific public will come to their assistance, and not let the expense of the undertaking fall, as now, almost entirely on the district of Craven.†

R. H. TIDDEMAN

ATLANTIC FAUNA

LAST May the s.s. *Hibernia* belonging to the Telegraph Construction and Maintenance Company, was despatched to repair the French Atlantic Cable, in which a fault was indicated some 200 miles from Brest. A brief account of some of the animal forms obtained by me in that expedition may not be without interest to some of the readers of NATURE.

To Mr. R. London, superintending the expedition, I am greatly indebted for the many facilities that he afforded me, of obtaining specimens of the deep-sea fauna. The first cast was made about 100 miles nearly due west of Brest, at a depth of 83 fathoms. Here we found numerous valves of *Pecten*, a fine *Ophiocoma*, with rays nine inches in length, which when handled broke itself into numerous fragments, *Echinus lividus*, *Spatangus purpureus*, &c.

At the surface we obtained by means of a towing-net a great abundance of a minute Entomostracous crustacean of a greenish-blue colour, with deep sapphire eyes, a *Cydippe*, two species of *Idotea* and *Polybius Henslowii*.

On the Atlantic cable, which was raised to the surface at a point 112 miles west of Brest, were found numerous shells of a small boring mollusc, one of the *Pholadidæ*, apparently *Xylophaga*. The outer covering of the cable, consisting of tarred manilla hemp, was perforated in many places by the round holes which they had formed and in which their shells were found. In places they had penetrated the outer covering, and had passed between the iron wires to the gutta percha core, in which they had made numerous shallow indentations, but in no case had they penetrated this to any depth. This cable, it will be remembered, was laid in 1869.

We now steamed about 87 miles westward to the edge of the Little Sole Banks, where the water deepens from 90 to 480 fathoms within a distance of a few miles. Here the cable was again hooked and brought to the surface from a depth of about 300 fathoms. Adhering to its surface was a species of *Pycnogonum* in great abundance. The specimens lived for some time after being brought to the surface, and moved about sluggishly.

A few bright red anomourous crustaceans were also obtained. These were very active, and lived for some days in a bucket of water.

They had, while in confinement, a peculiar habit of drawing their claws over their head, antennæ, and eyes, which suggested the idea that they were confused and dazzled by the extraordinary amount of light to which they were exposed.

A species of *Tubularia* of great beauty grew abundantly in clusters on the cable, and throve well in confinement. The cable was thickly overgrown with *Sertularias* of various species, moored to which by their hinder legs a species of *Caprella*, diabolic in appearance, but sluggish and inactive in nature, abounded.

A few miles farther westward the cable was raised from a depth of 480 fathoms. *Sertularias*, *Tubularias*, *Caprella*, &c., were still abundant; but the *Pycnogonum* was conspicuous from its absence.

In the recent expedition in which the *Great Eastern* and *Hibernia* have been employed in endeavouring to repair the Atlantic Cable of 1865, the natural history results have been much more meagre. Perhaps the most interesting objects obtained are some fragments of rock, consisting of Hornblende with interspersed crystals of quartz, found in lat. 51° 56′ N., long. 35° 45′ W., at a depth of about 1,760 fathoms.

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* *Geological Magazine*, vol. x. p. 140.

† Messrs. Birkbeck and Co., Craven Bank, Settle, have kindly consented to receive subscriptions.

FRED. P. JOHNSON

NOTES

PROF. SYLVESTER has recently made a discovery which is likely to create some interest, not only amongst mathematicians, but also amongst mechanicians and instrument-makers. By means of a sort of lazy tongs he has succeeded in converting spherical motion into plane motion, a result, we believe, hitherto looked upon as unattainable. This discovery will form the subject of a communication which Mr. Sylvester is announced to lay before the London Mathematical Society at its Annual General Meeting on Thursday next (November 13).

THE two gentlemen recently elected to Science-Fellowships at Oxford, are remarkable instances of success attending most irregular and unusual undergraduate careers. Mr. Yule was at one time a boy at Magdalen College School, he obtained the Brackenbury Scholarship for Physical Science at Balliol College, but was obliged to throw it up after a short time, on account of his failure to pass the classical examinations of the University. He bethought him of the more merciful ordinances of the sister University, and having obtained a Scholarship at St. John's College, Cambridge, proceeded on his undergraduate course unchecked by the lessened barrier of the previous examination. After being placed senior in the Natural Sciences Tripos, he returns to Oxford, we may hope bringing treasures from the East—and at any rate ready to use his vote for the improvement of the Oxford Examination Statutes. Mr. Macdonald is an individual who has come as near as is possible to achieving the feat of being in two places at one time. In fact, theoretically, he has been in two places at one time. He had the great courage and energy whilst holding a position in the Education Office, to enter as an Undergraduate at Merton College, and by consent of the College authorities he kept his term by sleeping in Oxford, which place he left every morning during term, so as to be at his official post, returning in the evening in time for hall dinner. His office-holidays he employed in practical work in the Oxford laboratories, whilst analytical chemistry had to be studied in his own sitting-room, converted for the time into a workshop. Such a history makes it very certain that the examination system has not failed at Merton College to secure at any rate a most worthy recipient of the fellowship.

THE election to the two vacant Fellowships at Merton College, took place on Oct. 30, when the choice of the electors fell upon Mr. John Wesley Russell, Lecturer of Balliol College, as Mathematical Fellow; and Mr. Archibald Simon Lang Macdonald, Commoner of Merton College, as Natural Science Fellow. Mr. Russell was placed in the first class in Mathematics under Moderators, in Trinity Term, 1871; and Mr. Macdonald in the first class in Natural Science at the final examination, in Michaelmas Term, 1871.

WE are glad to be able to add St. John's College, Cambridge, to the list of those which have opened their Fellowships to Students of Natural Science. Since 1868, the College has given Exhibitions yearly, and Foundation Scholarships since 1870, for the encouragement of a knowledge of Physics, Chemistry, and Biology. On Monday last the Master and Seniors, in proof of their desire to place the Natural Sciences on the same footing as Classics and Mathematics, elected one of their scholars, Mr. A. H. Garrod, B.A., who was a Senior in the Natural Science Tripos of 1871, to a Fellowship.

ABOUT the end of January 1874, there will be an election to a scholarship in Natural Science at Exeter College, Oxford, candidates for which will be examined in biology, chemistry, and physics. Candidates are not expected to exhibit *special* knowledge of more than one of the above subjects, and preference will be given to a candidate who excels in biology, or one of its branches. The candidate selected will have to satisfy the college that he has sufficient classical and mathematical knowledge to pass responsions. There is no limit of age disqualifying candidates for this scholarship. The scholarship is of the annual value of 80*l*., tenable for five years from matriculation. The scholar elected will have the use, during term, of a place in the histological laboratory of the college. For further information application should be made to Mr. E. Ray Lankester, Natural Science Lecturer, Exeter College.

MR. CHARLES J. F. YULE, of St. John's College, Cambridge, wishes us to state that he is not "the Cambridge B.A." whose letter appeared in last week's number.

AT the Comitia, held on Thursday, October 30, at the Royal College of Physicians, Dr. Robert Druitt was elected a Fellow of the College. The president announced that the Harveian Oration in the ensuing year would be delivered by Dr. Charles West. The Gulstonian Lectures will be delivered by Dr. J. F. Payne; the Croonian Lectures by Dr. Murchison; the Lumleian by Dr. Sibson.

WE regret to record the death, on Oct. 24, of Dr. Crace Calvert, F.R.S., F.C.S. The illness which caused it was contracted at Vienna, whither he had gone to act as juror in the International Exhibition. The *Journal of the Society of Arts* furnishes some particulars concerning the work of Dr. Calvert. As an analytical chemist his renown was European. He left England as a youth to pursue his education in France, and in the schools of that country secured many honours by the awards which he obtained. He subsequently pursued the study of chemistry, and was appointed assistant chemist at the Gobelin works, under his learned master, Chevreul. Soon after his return to England, he commenced reading a series of papers before the Society of Arts on chemistry applied to industry. At a later date, when the Society of Arts proposed to establish Cantor lectures, he gave the proposition his hearty support, and delivered two courses of lectures on "Chemistry applied to the Arts." He also delivered courses on "Synthesis and the Production of Organic Substances," on "Aniline and Coal Tar Colours," and on "Dyes and Dye-stuffs other than Aniline." In 1846 he settled in Manchester, and was soon after appointed Professor of Chemistry at the Royal Institution there. He was also for some time a lecturer at the Manchester School of Medicine. His connection with the Manchester Sanitary Association led him to hygienic investigations—one of the principal results of which was a patent for the application and preparation of carbolic acid. In scientific circles great interest attached to Dr. Calvert's protoplasmic investigations, some of the results of which were communicated in a paper read at the meeting of the British Association in Edinburgh some years ago, and afterwards published in the Transactions of the Royal Society. Dr. Calvert was a Fellow of the Royal Society of England, a Fellow of the Chemical Society, and an honorary Fellow of the Chemical Society of Paris. He was also a member of the Royal Academy of Turin, and of the Imperial Academy of St. Petersburg.

THE death is announced of Prof. J. A. F. Breithaupt, of Freiberg, the well-known Mineralogist, on October 22, at the age of 82 years.

Ocean Highways announces the death from scurvy on the Novaya Zemlya Coast, of the distinguished Norwegian Arctic Explorer, Captain Sivert Tobieson.

AT the meeting of the Royal Geographical Society last Monday, Sir Bartle Frere, the President, said that, though there was no further news of Dr. Livingstone, the latest accounts of both the expeditions sent out in the hope of meeting him, tell of satisfactory progress. Of the West Coast Expedition under Lieutenant Grandy, R.N., the latest direct accounts state that the expedition had just left San Salvador, about June 16, in good health, so that we may one day hope to hear of their further progress in their search for tidings of Livingstone, and every step of their progress will add to our knowledge of that most interesting, but little known, region. Comparing Consul Newton's dates with those of Dr. Beke's Portuguese informant, published on Saturday last, Sir Bartle Frere thinks we must await some confirmation of the latter report before concluding that Lieutenant Grandy had turned back. The other expedition started under Lieutenant Cameron on the east coast, and notwithstanding all delays, Lieutenant Cameron made a fair start for the lake region; and, by the latest accounts, was pushing on with every prospect of reaching a district where he was most likely to obtain tidings of Livingstone.—Mr. C. Markham, the Secretary, read a paper giving some interesting information connected with the voyage of the *Polaris* to the Arctic regions, and a discussion followed in which the desirability of another Arctic expedition was strongly urged, some of the members proposing that, if Government refused, the society itself should send one, but this view was controverted by Captain Sherard Osborne, who maintained that such an expedition, to be successful, should be under the auspices of the Government.

We have great pleasure in calling attention to a series of science lectures for clerks and working-men, which are to take place in South Place Institution, Finsbury. The first three lectures, on November 4, 11, and 18, are by Prof. Duncan, F.R.S., on the Geological History of the Earth, and these are to be followed by others on Light, &c. The gentlemen who get up these lectures deserve great credit, as they expect to be considerably out of pocket in their endeavour to place science lectures by the most eminent scientific men within the reach of the classes mentioned, who, we hope, will take ample advantage of the opportunity. The charge for admission is almost nominal.

Among the Local Societies, concerning which we have received information since we published our list, is the "Junior Philosophical Society," a London Society which meets on the second and fourth Friday of each month from October to June, at 8 P.M. The Society seems earnestly bent on work in the way of reading papers, and occasional excursions, no member being admitted who does not prove his willingness to take his share in the work of the Society. Many of the papers to be read this winter are on important scientific questions; and we would recommend the Society to the attention of those young men who are within convenient distance of the meeting-place, 6A, Victoria Street, Westminster.

His Excellency Senor Don Gregorio Beintes, Minister Plenipotentiary of the Republic of Paraguay, has appointed Mr. Charles Twite, M.E., late reporter to the Royal Commission on Mines, who explored the mineral resources of Siam; M. Balansa, botanist, late Commissioner of the French Government to New Caledonia and Egypt; and Mr. Keith Johnston, F.R.G.S., members of a scientific commission to inquire into and report on the natural resources of Paraguay. Dr. Leone Levi, F.S.S., Professor of Commercial Law in King's College, Consul-General of Paraguay in London, will edit the reports and exhibit them in relation to the economic condition of the country. Such reports will be published towards the end of next year.

The Exhibition which will be held in Manchester, by the Society for the Promotion of Scientific Industry, of appliances for the Economical Consumption of Fuel, will be opened on December 18 next. In connection with this subject, a gentleman has placed a gold medal at the disposal of the Council of the Society for the best specimen of peat fuel that shall come nearest to coal in its use and character, special regard being had to its cheap and rapid production.

The Council of the Institution of Civil Engineers have forwarded us a list of thirty-six subjects, on which they invite communications.

Mr. Albert Müller has sent us No. 2 of his "Contributions to Entomological Bibliography up to 1862;" further numbers will appear as materials accumulate. The list contains a great deal of information, and it will no doubt be valued by entomologists. It may be obtained from Mr. E. W. Janson, Museum-street.

The Director of the Imperial Russian Telegraph has given his consent to the transmission, free of cost, within the boundaries of the Russian empire, of messages announcing new astronomical discoveries.

Mr. James Dallas, of Benakandy, Cachar, writing us on the subject of inherited peculiarities, says that a friend of his has a black-and-tan English terrier dog, two inches of the end of whose tail is folded back so acutely as to come in contact with the upper portion. A pup, of which the dog is the undoubted father, has inherited the paternal peculiarity, with the difference that, instead of the end of the tail being turned up, it is turned down.

A series of methodical observations on the various movements of a ship affected by waves was carried out on board the ship *Norfolk* during her last voyage from Melbourne to London. The observations during the voyage (from July 24 until October 16) were effected by self-registering instruments, under the care of Mr. W. T. Deverell, on behalf of Mr. Spencer Deverell, of Portland, Victoria, who has devoted many years' study to the mathematical investigations of the movements of ocean waves and to their action upon a floating body. A complete report will constitute no doubt a valuable contribution to naval literature.

It is stated that the steamer *Tuscarora*, under the command of Capt. George E. Belknap, has lately been fitted up at San Francisco to undertake the labour of making soundings between the Pacific coast and Japan, in connection with the new cable route. On the detail of the *Juniata*, for service in the *Polaris* search, the sounding apparatus, which had been put on board for a similar service between New York and the West Indies, was transferred to the *Tuscarora*. This included a supply of new steel wire, with Sir William Thomson's patent reel. The vessel was to proceed early in July to Puget Sound, and thence, by way of the Aleutian Islands, to Hakodadi.

It is stated by the *Australian and New Zealand Gazette*, that the Government has signified its willingness to grant a site for th proposed Adelaide university; to give 10,000*l*. towards the cost of its erection, provided an equal amount is raised by private subscription; and to provide an annual grant equal to 5 per cent. on other subscriptions.

The great Exhibition of Vienna (we learn from the *Journal of the Society of Arts*) is to be commemorated by the establishment of an "Athenæum," as it is called, modelled after the Conservatoire des Arts et Metiers of Paris, and the Museum of Industry at Brussels, for the special instruction of workmen and small manufacturers. It is to be installed in the midst of the industrial quarters of the capital. A large quantity of drawings, designs, models, instruments, machines, tools, raw and partially manufactured materials, have been promised by exhibitors, and Baron Schwarz-Senborn, director of the exhibition, has presented a collection of between three and four thousand volumes of book connected with industrial exhibitions. The establishment starts with a capital of more than 11,500*l*.

On Sept. 1, an earthquake took place at 4.10 P.M. with slight shocks at Drama, in European Turkey. There was an earthquake at about 9 P.M. on Sept. 6, in Armenia, at Erzeroum, and elsewhere. Several shocks of earthquake were felt on Aug. 21, in the City of Guatemala, but very few houses were damaged.

La Nature records the recent death of M. Godard, senior, the oldest of French aeronauts.

The additions to the Zoological Society's collection during the past week include a Bosman's Potto (*Perodicticus potto*) from Africa, and a Blue Magpie (*Cyanopolius cyanus*) from China, presented by Rev. A. W. Peter; two Ursine Dasyures (*Dasyurus ursinus*) from Australia, presented by the Acclimatisation Society of Victoria; an Alpine Marmot (*Arctomys marmotta*), an Inconvenient Curassow (*Crax incommoda*) from S. America, a Red-bellied Thrush (*Turdus rufiventris*), a Red Oven-bird (*Furnarius rufus*), and two Yellow Trupials (*Xanthosomus flavus*) from Buenos Ayres; a Hoffmann's Sloth (*Cholopus hoffmanni*) from Panama, purchased; a Sun Bittern (*Eurypyga helias*) from S. America, deposited.

THE SELECTION AND NOMENCLATURE OF DYNAMICAL AND ELECTRICAL UNITS*

We consider that the most urgent portion of the task entrusted to us is that which concerns the selection and nomenclature of units of force and energy; and under this head we are prepared to offer a definite recommendation.

A more extensive and difficult part of our duty is the selection and nomenclature of electrical and magnetic units. Under this head we are prepared with a definite recommendation as regards selection, but with only an interim recommendation as regards nomenclature.

Up to the present time it has been necessary for every person who wishes to specify a magnitude in what is called "absolute" measure, to mention the three fundamental units of mass, length, and time, which he has chosen as the basis of his system. This necessity will be obviated, if one definite selection of three fundamental units be made once for all, and accepted by the general consent of scientific men. We are strongly of opinion that such a selection ought at once to be made, and to be so made that there will be no subsequent necessity for amending it.

We think that, in the selection of each kind of derived unit, all arbitrary multiplications and divisions by powers of ten, or other factors, must be rigorously avoided, and the whole system of fundamental units of force, work, electrostatic, and electromagnetic elements, must be fixed at one common level—that level, namely, which is determined by direct derivation from the three fundamental units once for all selected.

The carrying out of this resolution involves the adoption of some units which are excessively large or excessively small in comparison with the magnitudes which occur in practice; but a remedy for this inconvenience is provided by a method of denoting decimal multiples and sub-multiples, which has already been extensively adopted, and which we desire to recommend for general use.

On the initial question of the particular units of mass, length, and time, to be recommended as the basis of the whole system, a protracted discussion has been carried on, the principal point discussed being the claims of the gramme, the *mètre* and the second, as against the gramme, the *centimètre*, and the second; the former combination having an advantage as regards the simplicity of the name *mètre*, while the latter combination has the advantage of making the unit of mass practically identical with the mass of unit volume of water; in other words of making the value of the density of water practically equal to unity. We are now all but unanimous in regarding this latter element of simplicity as the more important of the two; and in support of this view we desire to quote the authority of Sir W. Thomson, who has for a long time insisted very strongly upon the necessity of employing units which conform to this condition.

We accordingly recommend the general adoption of the centimetre, the gramme, and the second, as the three fundamental units; and until such time as special names shall be appropriated to the units of electrical and magnetic magnitude hence derived, we recommend that they be distinguished from "absolute" units otherwise derived, by the letters "C. G. S." prefixed, these being the initial letters of the names of the three fundamental units.

Special names, if short and suitable, would, in the opinion of most of us, be better than the provisional designations "C. G. S. unit of . . . . . ." Several lists of names have already been suggested; and attentive consideration will be given to any further suggestions which we may receive from persons interested in electrical nomenclature.

The "ohm," as represented by the original standard coil, is approximately 10^9 C. G. S. units of resistance. The "volt" is approximately 10^8 C. G. S. units of electromotive force, and the "farad" is approximately $\frac{1}{10^9}$ of the C. G. S. unit of capacity.

For the expression of high decimal multiples and sub-multiples, we recommend the system introduced by Mr. G. J. Stoney—a system which has already been extensively employed for electrical purposes. It consists in denoting the exponent of the power of 10 which serves as multiplier, by an appended cardinal number if the exponent be positive, and by a prefixed ordinal number if the exponent be negative. Thus:—

10^9 grammes constitute a *gramme-nine*,

$\frac{1}{10^9}$ of a gramme constitutes a *ninth-gramme*.

The earth's circumference is approximately four metre-sevens, or four centimetre-nines.

For multiplication or division by a million, the prefixes *mega* * and *micro* may conveniently be employed, according to the present custom of electricians. Thus the *megohm* is a million ohms, and the *microfarad* is the millionth part of a farad. The prefix *mega* is equivalent to the affix *six*. The prefix *micro* is equivalent to the prefix *sixth*. The prefixes *kilo*, *hecto*, *deca*, *deci*, *centi*, *milli* can also be employed in their usual senses before all new names of units.

As regards the name to be given to the C. G. S. unit of force, we recommend that it be a derivative of the Greek δύναμις. The form *dynamy* appears to be the most satisfactory to etymologists. *Dynam* is equally intelligible, but awkward in sound to English ears. The shorter form *dyne*, though not fashioned according to strict rules of etymology, will probably be generally preferred in this country. Bearing in mind that it is desirable to construct a system with a view to its becoming international, we think that the termination of the word should, for the present, be left an open question. But we earnestly request that, whichever form of the word be employed, its meaning be strictly limited to the unit of force of the C. G. S. system; that is to say the force which, acting upon a gramme of matter for a second, generates a velocity of a centimetre per second.

The work done by this force, working through a centimetre, is the C. G. S. unit of work, and we propose to denote by it some derivative of the Greek ἔργον. The forms *ergon*, *ergal*, and *erg* have been suggested; but the second of these has been used in a different sense by Clausius. In this case also we propose for the present to leave the termination unsettled; and we request that the word *ergon* or *erg* be strictly limited to the C. G. S. unit of work, or what is, for purposes of measurement, equivalent to this, the C. G. S. unit of energy, energy being measured by the amount of work which it represents.

The C. G. S. unit of power is the power of doing work at the rate of one erg per second, and the power of an engine (under given conditions of working) can be specified in ergs per second.

For rough comparison with the vulgar (and variable) units based on terrestrial gravitation, the following statement will be useful:—

The weight of a gramme at any part of the earth's surface is about 980 dynes, or rather less than a kilodyne.

The weight of a kilogramme is rather less than a megadyne, being about 980,000 dynes.

Conversely, the dyne is about 1·02 times the weight of a milligramme at any part of the earth's surface, and the megadyne is about 1·02 times the weight of a kilogramme.

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* First Report of the British Association Committee on Units.

* Before a vowel, either *meg* or *megal* (as euphony may suggest), may employed instead of *mega*.

The kilogram-metre is rather less than the erg-eight, being about 98 million ergs.

The gramme-centimetre is rather less than the kilerg, being about 980 ergs.

For exact comparison, the value of g (the acceleration of a body falling in vacuo) at the station considered, must of course be known. In the above comparisons, it is taken as 980 C.G.S. units of acceleration.

One horse-power is about three quarters of an erg-ten per second. More nearly, it is 7·46 erg-nines per second, and one *force de cheval* is 7·36 erg-nines per second.

The mechanical equivalent of one gramme-degree (centigrade) of heat is 41·6 megalergs or 41,600,000 ergs.

SCIENTIFIC SERIALS

In the current number of the *Quarterly Journal of Microscopic Science*, Mr. E. T. Newton commences with a paper on the Structure of the Eye of the Lobster, his observation being the result of suggestions from Prof. Huxley. The structure of the eye is minutely discussed, and the accompanying illustrations are abundant. As a concluding remark, we read that "Notwithstanding all that has been written up to the present time concerning the mode of action of the compound arthropod eye, we are still unable satisfactorily to solve this difficult physiological problem."—A paper by Prof. Betz, of Kieff, on the methods of investigating the structure of the central nervous system in Man, will be found of special interest, the hardening, cutting, and tinting of specimens being discussed.—M. Pasteur's new contributions to the theory of Fermentation, are translated from the "Comptes Rendus," and Prof. H. L. Smith's paper on Archebiosis and Heterogenesis, is reprinted from the *Lens*.—A Résumé, by Mr. W. Archer, of recent observations on Parasitic Algæ, is followed by Dr. Klein's Contributions to the Anatomy of Auerbach's Plexus in the Frog and Toad, and this by a valuable series of observations by Prof. Lister on the Natural History of Bacteria, in which a study of the life of Bacteria under different circumstances as regards the fluid in which they grow, shows that their general appearance, size, and shape depend in great measure on the fluid in which they are growing, their removal from one to another fluid causing them to take on quite a different form, and their replacement the reassumption of the original condition. Many important facts are to be learned from this paper.—Mr. E. R. Lankester describes in detail the microscopic and spectroscopic appearances of a new Peach-coloured Bacterium, named by him *Bacterium rubescens*. The colouring matter he names Becterio-rubrin. This Bacterium does not generally occur in isolated plastids, but generally forming films, encrustations, or tufts. Most are aggregated in adherent masses, several excellent drawings of which accompany the paper.

The Journal of the Franklin Institute, Sept. 1873.—This number contains a useful paper by Mr. Hugo Bilgram, on the theory of steam governors.—In government reports on the decay and preservation of timber, Generals Cram and Gillmore recommend the Seely process as the best. It consists in subjecting the wood to a temperature above the boiling point of water, and below 300° Fahr. while immersed in a bath of creosote a sufficient length of time to expel the moisture. When the water is thus expelled the pores contain only steam; the hot oil is then quickly replaced by a bath of cold oil, by means of which change the steam in the pores of the wood is condensed, and a vacuum formed into which the oil is forced by atmospheric pressure and capillary attraction. Gen. Gillmore thinks a wooden platform, *thoroughly creosoted*, would last twenty to thirty years, and be better than a stone platform during that entire period.—An important paper by Prof Thurston (extracted from the *Iron Age*), treats of the molecular changes produced in iron by variations of temperature.—Mr. Mott points out the conditions of good construction in lightning rods, and Dr. Feuchtwanger gives some information as to nickel and its uses in the arts, coinage, and nickel plating.—An oil discovery of unusually rich character is announced from the neighbourhood of Titusville, Pa.; the production of the new region being estimated at 30,000 barrels per day.

Der Naturforscher, September 1873.—We note, in this number, two striking observations in animal physiology. One of these refers to the torpedo, which has been a puzzle to physiologists, inasmuch as, while giving shocks strong enough to lame or kill another animal, its own muscles do not show the least contraction. Du Bois Reymond's hypothesis is, that while the stimulation to discharge goes forth from the central organ, the same organ sends out at the same time a counteractive influence through the nervous system, which neutralises the excitability of the nerves. M. Franz Boll took a recent opportunity of experimenting with the fish on the Italian coast, and, among other things, he tested this theory by cutting some nerves, and watching their muscles when he stimulated the electric nerves. The neutralising stimulation being thus cut off, the muscles should, he thought, contract, if the hyphothesis were true; and they did so, the muscles of the unsevered nerves remaining at rest. Still, he hardly thinks the experiment decisive, because nerves are more excitable after section.—The other observation is by Prof. Fick, who has found, by manometric measurement, a less pressure of blood in the left ventricle than in the aorta; 80 mm. of mercury in the one case, 104 to 128 in the other (in a dog). He supposes the blood, only partially filling the ventricle, at the apex, to be shot against the semilunar valves, forcing them open by its *vis viva*. In the neighbourhood of the valves the pressure must quickly rise. In short, as the author puts it, the blood is not pressed, but hurled (*geschleudert*) into the aorta.—There is a useful abstract of the chief points in a paper by Prof. Abbé (to Schulze's *Archiv*) on the capability of the microscope and its limits. He seeks to show, by physical deductions, that the limit of magnification is as good as reached, in our best systems. Some curious observations by M. van Tieghem are given in a note on the independence of the individual organs of the embryo of plants.—M. Ebermayer, we find, has been examining the influence of forests on ozone-contents of the air; he states there is more ozone in and near forests than in the open, but among the denser branches there is somewhat less than in the open closely bordering the forest; and in the tops of the trees there is more than in the lower parts.—Several French Academy notes are abstracted: on the magnetic force of annealed steel, on development of electricity in liquid mixtures, on the planet Mars, &c; also Royal Society papers. Some meteorological observations as to distribution of heat in Switzerland deserve notice.

Bulletin Mensuel de la Société d'Acclimatation de Paris. August.—In a paper on the "Causes of the Depopulation of our Rivers," M. C. R. Wattel enters at length into the question of the French river fisheries, which will be read with interest by fish-culturists. Some interesting information as to the effect of navigation and trade on the rivers is given; but the great danger to the fisheries lies in the unrestricted destruction of immature breeding-fish: and M. Wattel recommends that steps should be taken to prevent over-fishing and to facilitate the erection of fishways on the rivers.—The notes of Dr. P. Marès on the acclimatisation of various sorts of Eucalyptus in Algeria, are interesting.—The results of the experiments to produce different coloured silks go to show that silk-worms fed on cherry-leaf produce a bright chromo-yellow-coloured silk, those on pear-leaves a darker shade of the same colour, those on apple-leaves a nearly white silk, but coarser than that of the silk-worms fed on mulberry-leaves.—An extract is given of a work by M. E. Perris, on "Birds and Insects," in which he considers the advisability of protecting small birds. M. Perris, granting all the birds are insectivorous, either continually or occasionally, acknowledges the good they may do, but doubts whether a large proportion of the insects destroyed are hurtful to man; and he raises the question whether, therefore, it is desirable to protect birds to kill what would otherwise do no harm.

The September number commences with a paper by the Secretary on some Australian vegetables, the introduction of which into Algeria is proposed.—An interesting paper on the breeding of ostriches in captivity is contributed by Capt. Crepu, who has kept several pairs of these birds. His observations throw much light on the natural history of the ostrich. M. Comber describes the mortality which has seized the deer and other animals in King Victor Emmanuel's park at La Mandria. The calamity is attributed partly to over-crowding and partly to the want of shelter and proper protection. In 1865, when the park and grounds were carefully cultivated, 13 deaths occurred. In 1873, the park being left in its natural state, 172 deaths are recorded.—An important paper on the production of milk is the result of a conference at the Jardin d'Acclimatation in July, and appears opportunely at the present moment, when the subject is attracting so much public attention.—M E. Perris continues his remarks on "Birds and Insects."

SOCIETIES AND ACADEMIES

MANCHESTER

Literary and Philosophical Society, October 7.—E. W. Binney, vice-president, in the chair.—"Atmospheric Refraction and the last rays of the Setting Sun," by Mr. D. Winstanley. It is recorded in the Proceedings of this Society that a letter dated from Southport and written by Dr. Joule was read at the meeting held on the 5th October, 1869 In that letter it is remarked that "Mr. Baxendell noticed the fact that at the moment of the departure of the sun below the horizon the last glimpse is coloured bluish green." Dr. Joule also observes that on two or three occasions he had himself noticed the phenomenon in question, and that "just at the upper edge where bands of the sun's disc are separated one after the other by refraction, each band becomes coloured blue just before it vanishes." During the past eighteen months the writer, from his residence in Blackpool, has had frequent opportunities of observing the setting sun, and has noticed the phenomenon of the final coloured ray certainly more than fifty times. To the naked eye its appearance has generally been that of a green spark of large size and great intensity, very similar to one of the effects seen when the sun shines upon a well-cut diamond. The colour, however, is by no means constant, being often, as in the case of Mr. Baxendell's observation, bluish green, and at times, as mentioned by Dr. Joule, quite blue. The period of its duration, too, is likewise variable. Sometimes it lasts but half a second, ordinarily perhaps a second and a quarter, and occasionally as much as two seconds and a half. When examined with the assistance of a telescope, it becomes evident that the green ray results at a certain stage of the solar obscuration, for it begins at the points or cusps of the visible segment of the sun, and when the "setting" is nearly complete, extends from both cusps to the central space between, where it produces the momentary and intense spark of coloured light visible to the unaided eye. From the fact of the green cusps being rounded I apprehend that irradiation contributes to the apparent magnitude of what is seen. The range of colour too as seen in the telescope is more varied, and the duration of the whole phenomenon more extended, than when the observation is made only with the naked eye. Respecting the increased range of colours seen when the phenomenon is observed with telescopic aid, I may mention that on the 28th of June the sea was calm and the sky quite cloudless at the setting of the sun. Of the final coloured rays fifteen diameters showed the first to be a full and splendid yellow, which was speedily followed by the usual green, and then for a second and a half by a full and perfect blue. Respecting the increased duration of the colour, I have found that when the atmosphere is sufficiently favourable to allow a power of sixty diameters being employed with a three-inch object-glass, the green effect is seen at that part of the sun's limb in contact with the horizon even when one half the sun is still unset, and of course from then till final disappearance. The different colours seen, together with the order of their appearance, are suggestive of the prismatic action of the atmosphere as the cause of their production, and the interception of the horizon or the cloud as the cause of their separation. Assuming the correctness of this view, it becomes evident that an artificial horizon would prove equally efficacious in separating the coloured bands, and also that if employed during an inspection of the sun's lower limb, the least refrangible end of the spectrum would be disclosed. By projecting a large image of the sun into a darkened room I was enabled to get the whole of the spectrum produced by the prismatic action of the atmosphere in a very satisfactory manner. In this case a semicircular diaphram was used, so placed that its straight edge divided the field of view into equal parts, from one of which it obscured the light. The diaphram was placed in the focus of the eyepiece, and by rotating it every portion of the sun's limb could be in turn examined, and that too in the centre of the field, so as to be equally subjected to the minimum of the peculiarities of the instrument. When the sun's lower limb was allowed to descend into the field of view the first rays were intensely red. After a momentary duration they gave place in succession to orange, yellow, and green, which were then lost in the ordinary refulgence of the sun. The upper limb gave green, blue, and finally purple, which latter colour I have thus far never seen upon the natural horizon. I apprehend that the results here given sufficiently prove that atmospheric refraction is the cause of the coloured rays seen at the moment of the sun's departure below the horizon.

Cambridge Philosophical Society, Oct. 20.—The following communications were made to the Society:—By Mr. J. C. W. Ellis, Sydney College: Mechanical means for obtaining the real roots of algebraical equations.—By Mr. A. Marshall, St. John's: Graphic representation by aid of a series of hyperbolas of some economic problems having reference to monopolies.—By Mr. H. H. Cunyngame, St. John's: A machine for constructing a series of rectangular hyperbolas with the same asymptotes.

PARIS

Academy of Sciences, October 27.—M. de Quatrefages, president, in the chair.—The following papers were read:—Sixth note on guano, by M. Chevreul.—Answer to Respighi's note on the magnitude and variation of the sun's diameter, by Father Secchi. The author defended his method from Respighi's criticisms as regards the effect of heat in distorting the image during the passage through the prisms. He found that the effect of heat on compound prisms was very considerable, and therefore used his object-glass prism; and stated that in a future letter he intended to show that there were true variations in the solar diameter.—On crystalline dissociation, by MM. Favre and Valson. The authors continued the account of their researches, the present portion of the paper dealing with the valuation of the work done in the various solutions.—Note on the tertiary supra-nummulitic formation of the Carcassone basin, by M. Leymerie.—On certain cases of human double monstrocity, by M. Roulin.—Note on the origin and method of development of omphalosic monsters, by M. C. Dareste.—New method of condensing liquifiable substances held in suspension by gases, a reply to M. Colladon, by MM. E. Pelouze and P. Audouin.—M. Guérin-Méneville sent a letter in which he asserted that the *Phylloxera* is not the cause, but a consequence of the vine disease.—Note on the swellings produced on vine rootlets by the *Phylloxera*, by M. Max. Cornu.—Results of experiments on the destruction of the *Phylloxera* by means of carbonic disulphide, by M. Bazille. The author found that this agent was very successful, and that the doses could be reduced considerably but that different soils require different doses.—On the action of the condenser on induction currents, by M. Lecocq de Boisbaudran.—On the purification of hydrogen, by M. Ch. Viollette.—On the Cape diamond fields, by M. Hugon.—On the sugar contained in vine-leaves, by M. A. Petit. The author found in 1 kilo of leaves as much as 33 grammes of cane sugar and 12 of glucose; this was, however, exceptional, the latter generally exceeding the former and the total quantity of both being less.—On the Rhizocephalous *Cirripedes*, by M. A. Giard.—On the irritability of stamens, by M. E. Heckel. The author has distinguished two orders of movement in these organs.

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mastered by a few months' residence in a neighbouring country, whilst the other had done more to develop true culture than almost all other writings since. It is not proposed simply to substitute German or French for Greek, the advantages to be derived from which are now fully absorbed into the spirit of the nation, but, by the change, to leave a sufficient time, in addition to the education in modern languages, for the study of the Natural Sciences during the school-boy period. That the dead languages form an excellent mental training no one doubts, but that Physics and Chemistry do the same is daily becoming more certain; and the time is not far hence when the facts and methods of Physiology and Comparative Anatomy will be so well known and assorted, that they may be placed in the same category.

THE SOUTHERN UPLANDS OF SCOTLAND*

THE range of hills, which in Scotland extends from the German Ocean to the Irish Sea, having a N.E. and S.W. direction, has been aptly designated the Southern Uplands. This range is nearly parallel in its course to that of the Highlands proper. It exhibits hills, some of which attain to an elevation approaching nearly 3,000 feet; but its physical features, although marked in many localities with scenes of great beauty, are devoid of the stern and rugged grandeur which characterises the more northerly mountains of Scotland. The hills of this range usually consist of rounded and grass-covered undulations, or long tracts of plateaux. They have been specially named the "pastoral district of Scotland," and their scenes have furnished subjects for many a pastoral song, and many a border ballad.

The Southern Uplands of Scotland are cut deeply into by some of the streams which flow into the Solway Firth, the Esk, the Annan, the Nith, the Urr, and the Dee being the most important of them. They are drained on the southward side by the Cree and the Luce; on the northward side they are the sources of the Ayr; and the Tweed and its tributaries drain a large portion of their north-east area.

In the early period of Scotch geology, the days of Playfair and Hutton, the Southern Uplands were regarded as affording no traces of the evidence of life in the rocks which compose them; and these rocks were referred to the "primary" group. It was not until the discovery of fossils in a limestone which occurs at Wrea in Peeblesshire, in their higher portion, by Sir James Hall, that the rocks which formed these hills were assigned to the "transition" age. The terms "primary" and "transition" have now ceased to be applicable to the nomenclature of geology; and the discovery by Prof. James Nicol in 1840, in the flaggy beds of Greiston in Peeblesshire, of graptolites, indicated the Silurian age of the strata here. Since the discovery of Nicol, several geologists have added greatly to our knowledge of the rocks which compose the Southern Uplands. Other bands of graptolites have been found richer in fossil contents than those first discovered; and these, along with a few other forms of organic remains, have still further confirmed the Silurian age of the great mass of strata which make up the hilly country in the South of Scotland.

The result of the observations made on the rocks of the Southern Uplands up to the period when they came under the notice of the Geological Survey of Scotland led to the conclusion that the lowest strata exhibited were referable to the Llandeilo age. That these Llandeilo rocks were succeeded by deposits containing fossils, as in the case of the Wrea limestone, indicating the horizon of the Bala or Caradoc rocks, was also known—and certain rocks which occur near the north-western margin of the area in the neighbourhood of Girvan in Ayrshire, have been referred by Sir Roderick Murchison to a still higher position in the Silurian series.

The labours of the Geological Survey of Scotland have not only confirmed these conclusions, but have added greatly to our knowledge of the nature of the Silurian rocks of the South of Scotland. They have also furnished subdivisions of these rocks, and a more ample account of their arrangement and fossil contents.

Every geologist familiar with the lower portions of the Silurian rocks of the Southern Uplands, the Llandeilo strata, had experienced great difficulty in recognising horizons, in this series, such as would enable him to divide these rocks into distinct portions. It is true that bands of anthracitic shale abounding in graptolites were, as regards their petrological nature, very distinct from the rocks in which they were intercalated. The great mass, however, of the Llandeilo beds of the Southern Uplands consist of rocks known in old petrological nomenclature as "greywackes"—a name which is still retained for want of a better—and as these rocks differed only in coarseness, and sometimes in colour, this circumstance rendered the division of the South of Scotland Silurian rocks into separate groups extremely difficult. And when it is added to this that contortions have greatly folded and denudations have largely planed off the edges of these rocks, the difficulty of making out distinct horizons among the Llandeilo strata of the South of Scotland becomes very apparent. It is only by a careful continuous, and long series of observations recorded in maps large enough to show all the contortions, the ins and outs of the strata, that these rocks could be brought into subdivisions enabling them to be recognised. Such have been the work of the officers of the Geological Survey of Scotland; and now we have in the explanatory notes to some of the sheets which have been published, the results of their work recorded, and the subdivision of these Llandeilo rocks indicated.

The explanation to Sheet 15, published in 1871, which includes, among other matters, a description of the Llandeilo rocks occurring in that portion of the Southern Uplands occupied by the north-west part of Dumfriesshire, the south-west portion of Lanarkshire, and the south-east portion of Ayrshire, contains the results of the labours of the Survey among these rocks. There do not appear, in any portion of the South of Scotland Silurian strata, any rocks which appertain to an age older than the Llandeilo; and these Llandeilo rocks are referable only to the Upper Llandeilo series, the Lower Llandeilo or Shelve rocks of Murchison, the Arenig rocks or Skiddaw slates of Sedgwick, being unknown in the district. This Upper Llandeilo series exhibits itself in the

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* Memoirs of the Geological Survey of Scotland, Sheets 1, 2, 3 and 15, &c. Explanations of, 1871, 1872, 1873.

THURSDAY, NOVEMBER 13, 1873

ON THE MEDICAL CURRICULUM

IN a recent number of this journal (NATURE, Oct. 2, 1873) we made some remarks on medical studies, which were intended more for students themselves than in any way to bear on the principles of medical education. To the latter subject special attention has just been directed by Prof. Huxley, who, as Lord Rector of the University of Aberdeen, has drawn up a series of propositions for the consideration of the Court at the next meeting in February or March, on which occasion he will deliver his inaugural address.

The following are the motions that the Lord Rector will propose :—

"I. That, in view of the amount and diversity of the knowledge which must be acquired by the student who aspires to become a properly qualified graduate in medicine; of the need recognised by all earnest teachers and students for the devotion of much time to practical discipline in the sciences of chemistry, anatomy, physiology, therapeutics, and pathology, which constitute the foundation of all rational medical practice; and of the relatively short period over which the medical curriculum extends—it is desirable to relieve that curriculum of everything which does not directly tend to prepare the student for the discharge of those highly responsible duties, his fitness for the performance of which is certified to the public by the diploma granted by the University.

"II. That it would be of great service to the student of medicine to have obtained, in the course of his preliminary education, a practical acquaintance with the methods and leading facts of the sciences comprehended by botany and natural history in the medical curriculum; but that, as the medical curriculum is at present arranged, the attendance of lectures upon, and the passing of examinations in, these subjects occupy time and energy which he has no right to withdraw from work which tends more directly to his proficiency in medicine.

"III. That it is desirable to revoke or alter ordinance No. 16, in so far as it requires a candidate for a degree in medicine to pass an examination in botany and zoology [illegible] part of the professional examination; and to provide, in lieu thereof, that the examination on these subjects shall, as far as possible, take place before the candidate has entered upon his medical curriculum.

"IV. That it is desirable to revoke or alter said ordinance No. 16, in so far as it requires candidates for the degree of doctor of medicine to have passed an examination in Greek, and that, in lieu thereof, either German or French be made a compulsory subject of examination for said degree, Greek remaining as one of the optional subjects."

In considering these points a review of the method by which the present position of the medical curriculum has been arrived at, will throw considerable light on the steps which ought to be taken for its improvement, and will show how subjects which have but an indirect bearing, or none at all, on medicine proper have been gradually made to form an element of the course of study, without any question having been asked as to whether their introduction does not bring its concomitant disadvantages.

The influence of Materia Medica seems to have been great in bringing about the present state of affairs. When Dr. Anthony Todd Thomson and Dr. Pereira, in their enthusiasm for their favourite subject, extended its limits so as to include a full account of the source and history of every one of the articles which were mentioned in the Pharmacopœia, and went so far as to give a full description of *Gallus bankiva*, together with all the steps in the development of its egg, simply because *Ovi vitellus* is an antidote against poisoning by corrosive sublimate, and is employed in the preparation of Mistura Spiritus Vini Gallici (egg flip), it is evident that as the sciences of zoology and botany became more profound, Materia Medica as a subject would proportionately expand. At last a time came when separate lectures had to be given on the above-mentioned kindred subjects, in order that those on Materia Medica might be more easily comprehended by the student; and, as might be expected, these independent lectures on zoology and botany, as those on chemistry had done before, became so complete in themselves, as to reduce the subject which had given rise to their introduction, to a simple formulary for the chemist, with references to the sources of the necessary scientific information. The introduction, however, of zoology and botany as separate independent elements of the curriculum, brought into the medical education a large mass of matter, which is very valuable no doubt in itself, but to the student entirely irrelevant; and as in the short pupilage of three or four years there is a much larger amount that ought to be learned than can be properly acquired in the time, it becomes a matter worth serious consideration, whether subjects which are not indispensable to a thorough training should be still taught and be required by the examining bodies. The question therefore resolves itself into the determination of whether the loss of time necessary for obtaining a superficial knowledge of a couple of sciences, is counteracted by the advantages of those sciences as a mental training and a basis for higher work? In an Introductory Lecture delivered some time ago at University College, Prof. Huxley throws the weight of his opinion in the scale against retaining the subjects which must be to him most dear, in the medical curriculum; and most will agree with him, notwithstanding the many difficulties in the way of an improved programme.

With regard to Prof. Huxley's fourth proposition, in which it is considered desirable to omit Greek from the preliminary examination, and substitute German or French in its place, the interest will not be so great to most, as that relating to the scientific qualifications that are necessary. The same conservative spirit which has prevented any reduction in the overloaded Biological portion of the curriculum, has, without question of any kind being asked, never even hinted at any change in the long-established and well-tried school-course, in which the at one time practically valuable and indispensable Greek and Latin are still retained, though of less importance at the present day. How many of our scientific men find that nothing deters them in every step of their work, more than a want of knowledge of the German language, now that the scientific activity of that country is so considerable and so rapidly increasing. There must be a change with the times, even in primary education, and we hardly think that in his introductory address to the King's College Medical Society on the 23rd of last month, Prof. Curnow put the case fairly when he disapproved of the substitution of German for Greek, because the one could be

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form of an anticlinal axis near the southern border of the Silurian area. This axis can be well seen in Roxburghshire and Dumfriesshire, having a north-east and south-west direction. It has also been recognised by the officers of the Geological Survey in Wigtonshire; and the rocks which it exhibits, which are the lowest in the Southern Uplands, have been designated by Prof. Geikie the "Ardwell group." This group is made up of "hard, well-bedded greywackes and grits, with bands of hard shale or slate. These rocks have a prevailing reddish or brownish hue, especially on weathered surfaces."

As seen in Dumfriesshire and Roxburghshire these low rocks have the same aspect and nature. They have afforded, both in Wigtonshire and Dumfriesshire, markings which have considerable resemblance to the fossil described by McCoy as *Protovirgularia*, and in Roxburghshire they have yielded crustacean tracks, but no other traces of organic remains have been obtained from them.

Above the Ardwell group the officers of the Geological Survey recognise a mass of strata to which they have given the name of the "Lower or Moffat Shale group." This group is composed of "flaggy greywacke and grey shales," which are distinguished by the occurrence in them of several bands of black carbonaceous shales. These strata are well developed in the neighbourhood of Moffat, Dumfriesshire, from whence they derive their name. The black carbonaceous shales are very persistent, having been traced by the officers of the Survey from near Melrose to the western shores of Wigtonshire, "a distance of more than 100 miles." Three bands of carbonaceous shales can frequently be made out, but occasionally they come together so as to form one thick band. These bands are very prolific in graptolites. They have, from their carbonaceous aspect, induced many persons, under the guidance of "practical miners," to expend large sums of money in search after coal, and some of the spots where they have been worked are known under the name of "coal heughs."

Although the Moffat group is well developed through the greater portion of the Southern Uplands, it is on the coast of Wigtonshire that the best sections of the series can be seen. Here they are recognised resting on the Ardwell group, having at their base "grey and reddish shales, and clays, with calcareous bands and nodules, and enclosed bands of black shale, the lowest members being hard and flaggy." The second member of the Moffat group, as seen on the Wigtonshire coast, consists of black shales with intercalated clays, like the fire-clays of the coal-measures. Calcareous nodules and lenticular bands are also associated with the black shales, the whole being so intensely plicated as to render an attempt to determine their thickness extremely difficult. Upon the black shales well-bedded greywacke and grits occur with occasional shaly partings. These are succeeded by black shales so much jumbled and jointed, that their thickness cannot be made out. The next sequence consists of grey flagstones, flaggy sandstones, and grits, in beds of varying thickness up to 3 or 4 ft., with abundant partings of grey shale. To these succeed a thick band of finely laminated grey shale, 3 or 4 ft. Black shales, bands 12 to 18 ft. in thickness, occur next, and the highest members of the group consist of fissile shales.

The Moffat group, as represented in Wigtonshire, has a thickness of about 1,000 ft., of which more than half consists of flaggy greywacke beds. The underlying series, the Ardwell group, probably attains to a much greater thickness.

The third member of the Upper Llandeilo rocks of the Southern Uplands of Scotland, like the second, derives its name from Dumfriesshire. It is well exhibited in the hill called Queensberry, and has been designated the Queensberry grit group. The characters of this third member, as they are seen in Wigtonshire, "consist of greywacke and grits in massive courses, with occasional bands of grey and greenish shales." Massiveness and regularity of bedding and jointing are the characters of this group. The sandstones are often coarse, and sometimes even coarse conglomerates appear, in which some of the embedded fragments are sometimes from 2 ft. to 3 ft. in diameter, a feature which distinguishes the Queensberry group from all the other members of the Upper Llandeilo rocks of the South of Scotland. Fossils appear to be absent from this group, no trace of them having been met with in the three parallel bands which traverse Wigtonshire.

In the Dumfriesshire portion of the Upper Llandeilo area of the South of Scotland, there have been recognised, above the Queensberry grit group, black shales with graptolites, the thickness of which have not yet been ascertained. To these black shales the name of Hartfell group has been given. As the typical area where these rocks occur is in the higher part of the Annandale district, the sheets of which have not yet been published, we have at present no account of this group from the Geological Survey.

The Hartfell group is succeeded by the Daer group, which is made up of hard blue and purplish greywacke, and grey shales. It derives its name from a stream flowing from the north side of Queensberry into the Clyde. Its strata are greatly folded, and no reliable estimate can be formed of the thickness of the Daer group.

The Hartfell shales of the Daer group seem to thin out towards the south-west. They have not been distinctly recognised in Wigtonshire, where the Dalveen group, which in Dumfriesshire succeeds the Daer group, is seen resting conformably upon the Queensberry grits.

In Dumfriesshire the Dalveen group consists of fine blue and grey greywacke, and shales having no features distinguishing them from other members of the upper Llandeilo rocks. Their estimated thickness is about 2,900 ft. They are well exposed in Dalveen Pass, Dumfriesshire, whence their name, and in Dinabid Linn they are seen passing under a coarse pebbly rock, "Haggis Rock."

In Wigtonshire the lower part of the Dalveen group is seen overlying the Queensberry rocks south of Corsewell Lighthouse. Here its lower portion is remarkably shaly, but thick masses of greywacke also occur. Among the shaley beds are some bands worked at Cairn Ryan for slates. These slates have long been known as affording graptolites; and another thin band of black shale also containing the same fossils appears in this group in Wigtonshire.

In Dumfriesshire above the Dalveen group a series of coarse and fine grits and greywacke, having red and green bands of flinty mudstone, conglomerate, and occasional breccia associated with them, occur—a persistent band of conglomerate containing quartz-rock pebbles, Lydian stone, and jasper characterise this group. The conglomerate, being locally known as "Haggis Rock," has furnished the name to the series, which is about 1,800 feet thick. The Haggis group in Dumfriesshire is seen striking across the river Afton, also, along the N.W. flanks of the Lowther hills, and elsewhere in this county. More to the north it can be recognised along the north-western margin of the Silurian area in Crawfordjohn, Lanarkshire. The Haggis rock is not persistent in its character. To the N.E. this conglomerate becomes much finer in grain, and passes "into a gritty greywacke." This group has hitherto yielded no fossils. In Wigtonshire the Haggis rock cannot be distinguished as a distinct series; its characteristic conglomerate being, as already seen, of local occurrence, it does not appear to manifest itself in the Silurians in the S.W. of Scotland.

(*To be continued.*)

LOCAL SCIENTIFIC SOCIETIES

IN very many ways has the general advance of intelligence, elevation of taste, and spread of education been shown during the present century, and more especially during the last thirty years; one of these ways is undoubtedly the increasingly rapid spread of Local Scientific Societies. What we mean by a "Local Scientific Society," as distinguished from the large Societies of London, is an association of individuals in a particular locality for the common study of one or more branches of science, by the reading of original papers, and what is perhaps of more importance, the actual investigation of the natural history—geology, zoology, botany, meteorology—and archæology of its district. Of the societies established within the last thirty years, nearly all are marked by these characteristics; such at all events is their professed object, and we are glad to say that, to judge from the special reports which we have received, and the numerous printed "Proceedings" of greater or less pretensions which are sent us from time to time, a very large proportion creditably carry out their programme.

In a number of the principal towns of England and Scotland associations exist, dating, some of them, from the end of last century, known as "Literary and Philosophical Societies," or by some similar title. These are generally comparatively wealthy, possessed of good buildings containing a library, museum, reading-rooms, lecture-hall, &c., with a large body of members belonging to the middle and upper classes. These, however, so far as their original objects are concerned, with one or two exceptions, scarcely come under the category of Local Scientific Societies, in the sense of the definition given above, though many of them, stimulated by the growing taste for Science, have recently added to their usual courses of lectures on literary subjects, others on subjects connected with Science, and have even organised classes for the study, under competent lecturers or teachers, of one or more branches of Science. In some instances, moreover, a few of the members of these respectable old associations have united to form societies of a kind which entitle them to be regarded as Local Scientific Societies, and even Field-Clubs. Still, all these older societies, as they existed previous to 1830, differed in many essential respects from the Local Societies and Field-Clubs which began to spring up about that time; even the well-known Literary and Philosophical Society of Manchester, quite on a par with some of the best London Societies, and which has produced original work of the highest value, has been all along confined to the learned and professional men of the city and neighbourhood, who have made use of the meetings of the Society for the purpose of making known the results of their independent scientific investigations.

So far as can be ascertained, the society just mentioned is the oldest provincial society which can be considered as in any way scientific, having been established in 1784, for the purpose of diffusing "literary and scientific intelligence, and of promoting the literary and scientific inquiries of learned men in the town and neighbourhood." "The results of its labours," Sir Walter Elliott says, in his valuable address to the Edinburgh Botanical Society, in 1870, on this subject, "were published in 'Memoirs,' the first volume of which appeared in 1785, at which time James Massey was president, and Thomas Barnes, D.D., and Thomas Henry, F.R.S., were Secretaries. Five volumes had appeared up to 1802. In 1805 a second series commenced under the Rev. John Walker, President, and John Hall and John Dalton, Secretaries, which had extended to five volumes more in 1860. A third series was commenced in 1862, and has reached volume xiii. The second series is enriched with many papers by Dalton, including the first development of the atomic theory." In 1858 a microscopical and natural history section was established; the latter, however, we regret to say, is since defunct.

The next society of this class in order of time was instituted at Perth in 1781, as the Perth Literary and Antiquarian Society; we need not say that, so far as eminence is concerned, it was never to be compared with the Manchester Society. It has never done scientific work of any value, though it possesses a handsome building, with a museum, devoted mostly to antiquities, but having a fine natural history collection as well, and a good library. Like many other societies of a similar kind, its building serves as a kind of meeting-place or club, where those members who have nothing to do can meet and have a gossip, and read the papers. This society has published only one volume of "Transactions" (in 1827), but so far as we know, they have now no transactions to record. A few years ago, as will be seen from our list in Vol. viii. p. 521, a Natural Science Society was established in the county, with Perth as its headquarters, which gives promise of being one of the best working Local Scientific Societies in the kingdom.

In 1801 a society of a similar kind was established in the sister kingdom, the Literary Society of Belfast, which has never done anything to call for note here. Previous to this, however, in 1793, the Newcastle-on-Tyne Literary and Philosophical Society was established, which, although it has published only one volume of memoirs, and is little more than the owner of an excellent public library, does good work by providing educational courses of lectures for instruction in mathematics, chemistry, and other branches of science as well as literature.

Up to 1830, about twenty other societies, more or less "Philosophical," which term seems then to have been thought a more dignified term than "Scientific," were instituted within the three kingdoms, including the Ashmolean Society of Oxford, and the Cambridge Philosophical Society. Of these, no less than six were in Yorkshire alone, a county, as we shall see, which continues to hold the foremost place, so far as number of scientific societies is concerned; the West Riding bristles with little Field Clubs. Among the best of the societies referred to is the Liverpool Literary and Philosophical Society, which, especially since its amalgamation in 1844 with the Natural Science Society, has done some excellent work, as can be seen from its voluminous "Proceedings," which contain papers that would do credit to any society. The Glasgow Philosophical Society is also one of high standing; and the Royal Geological Society of Cornwall, founded in 1814, which has done some good work in connection with the geology of the district. The Royal Institution of Cornwall is also one of the most creditable of these old societies, having been formed in 1818, for the advancement of knowledge of natural history, natural philosophy and antiquities, especially in their connection with Cornwall. Besides its valuable antiquarian work, it has pubished "The Cornish Fauna," a compendium of the natural history of the county.

The one of these older societies which in its object and work corresponds most nearly to our definition, is the Northumberland, Durham, and Newcastle Natural History Society, instituted at Newcastle-on-Tyne in 1829. Among its original members were Sir John and Sir Walter Trevelyan, and the late Albany Hancock, and both before and since its junction with the Tyneside Naturalists' Field Club, it has done much work of a kind similar to that which the recently established Field Clubs aim to do, having between 1831 and 1838 published two volumes containing valuable lists of the flora and fauna of Northumberland and Durham. This society, though somewhat crippled for want of funds, is still in a flourishing condition, and continues, in conjunction with the Tyneside Club, to publish in their Transactions, under the title of "Natural History Transactions of Northumberland and Durham," excellent lists of the fauna and flora, existing and fossil, of the district which it has adopted as its field for work. It possesses some splendid collections which the Newcastle College of Physical Science is generously allowed to use for purposes of study.

Had we space, others of these societies founded previous to 1830, as well as some of a more ambitious kind than the simple Field-Club, instituted since that time, could be named, which stimulated either by the example of the field-clubs, or more probably by the general advance of culture and the growing impressiveness of Science, have done much to foster a love for Science in their respective neighbourhoods and to investigate the natural history of their several districts. A large proportion of societies of this class are found in the south-west of England, in Devonshire and Cornwall: such are the Cornwall Polytechnic Society, the Devonshire and Cornwall Natural History Society, the Devonshire Association—a peripatetic Society founded in 1862 after the model of the British Association—the Royal Institution of South Wales (Swansea), and the Isle of Wight Philosophical and Literary Society. Others also we might mention at the other end of England, for an examination of our list shows that the activity of the country in this respect has been developed to the greatest extent in the north and south.

These societies, though differing in some essential respects from the simple Field-Club, yet in their own way do good and serviceable work by the establishment of museums, the encouragement of local exhibitions, the occasional publication of papers illustrative of the natural history and archæology of the district, and recently, what we deem of considerable importance, the institution of courses of lectures by eminent men of science, and the establishment of classes for the working and other classes who are engaged during the day. We would urge all of this class of association to bestir themselves to the performance of more thorough and more extended work in these directions, thereby not only doing a benefit to the members themselves, as well as to the cause of Science, but elevating the district in which they are located, and thus helping the country onward in the general march of improvement. By means especially of continuous series of lectures by eminent men of science and by well-organised systems of classes, the good that might be done by these institutions would, we believe, be inestimable; and now that the Science and Art Department offers such splendid facilities for the establishment of classes and museums in connection with any institution that chooses to take advantage of them, no local society of any pretensions need any longer be without the material of a comprehensive and high-class education for its members and those in its neighbourhood who are willing to be improved; only a lazy unwillingness to keep up with the rapid progress of the time can deprive a neighbourhood of these advantages. The Royal Cornwall Polytechnic Society, the first "Polytechnic" in the United Kingdom, is an example of what can be done in one way, by the establishment of lectures and classes, and by the institution of medals and money prizes for successful attempts to apply Science to industry. But a model which all literary and philosophic societies, *et hoc genus omne*, would do well to imitate, though they would find it difficult to rival, is the Birmingham and Midland Institute, an institute of which its originators may well be proud, and for the establishment of which they deserve the gratitude of the busy and important district in the midst of which it is planted. It scarcely comes within the scope of our subject, and we only mention it to show to the class of societies with which we are at present dealing, what they might hope to achieve if they only had the will and the generosity to bestir themselves and take the necessary steps. There is no reason why in every county town or other suitable place institutions of this kind should not be established, forming active centres of intellectual culture, and to which the smaller scientific societies of the surrounding districts might be affiliated without losing their independence and with very valuable results. We hope ere long to see this accomplished; and who are better fitted to take the initiative in the matter than those societies which pretend to represent the culture of the districts from which their members are drawn?

(*To be continued.*)

THORPE'S "QUANTITATIVE ANALYSIS"

Quantitative Chemical Analysis. By T. E. Thorpe, Ph.D., F.R.S.E., Professor of Chemistry, Andersonian University, Glasgow. (Longmans.)

WE welcome with pleasure a work which in the present state of our literature on Quantitative Chemical Analysis, may well be looked upon as a boon to the advanced chemical student. Fresenius's Quantitative Analysis has been so generally accepted by chemists as the standard book in this branch of Science, that we greatly regretted the unwarrantable liberties taken by the English editor in the late edition of our trusty author's work. The publishers, who did not, in justice to the accomplished author, recall that edition, may yet learn that the chemical public, at all events, know how to appreciate a good work on Quantitative Analysis. We confess to a feeling of relief, speaking as a teacher of chemical analysis, as we perused Mr. Thorpe's book; for although we have to differ from the author on some minor matters, we believe that this new work will speedily be found in the hands of every chemical student.

Our author has evidently felt what others have experienced before him, that Fresenius's Quantitative Analysis became with every new edition more and more unwieldy (we are speaking of the German editions), and that, at the commencement at least, a simpler guide to quantitative analysis might with advantage be placed in the hands of the student. As methods of analysis—especially volumetric methods—multiplied year after year, the teacher and the student looked to the master for some indications which methods should, under given circumstances, be adopted in preference to others. Mr. Thorpe has evidently been bent upon supplying this want. In the treatment of his subject he has followed the example set by Woehler in his "Practische Uebungen in der Chemischen Analyse," rather than that of Fresenius. It appears to us, however, that he has somewhat fallen into the other extreme, for, in the place of a series of carefully elaborated methods for the determination of each base and acid, he has contented himself with giving a few examples only of individual determinations, and has preferred to teach quantitative separations almost exclusively by describing, in language both terse and concise, a number of complex quantitative analyses, such as are likely to occur in practice. There is much to be said for this plan of teaching analysis, so to speak, *en bloc.* It involves, however, much repetition, or, at the very best, reference from one example to another, and leaves the student in considerable uncertainty whenever he has to break new ground. The aim of all quantitative teaching should be to enable the analyst to adopt or devise for himself correct methods of separation. The foundation for quantitative methods should, in fact, be laid by careful and accurate qualitative work. A good workable method may often be preferable to a more elaborate although more strictly accurate method.

In the endeavour to write as compactly as possible, the author has frequently over-estimated the mental powers and the chemical knowledge, say of second years' students, for whose use the work is apparently written, and has thus sacrificed clearness for briefness. We refer, for instance, to the methods given for the separation of iron, manganese, &c. in Spiegeleisen, condensed as it appears, from Fresenius, where the ammonium carbonate method occurs, but where it would be difficult for a student, without the teacher's assistance, to trace the chemical changes. There is too much of the *how* to do a thing, and too little of the *why* to do it throughout the work, to make it as useful to the beginner as it would otherwise be. Although the several methods for the separation of manganese from iron, &c., are to be found in different parts of the book, there are scarcely sufficient hints, why and under what circumstances and conditions the one method is to be used in preference of the other. The same applies to various other methods of separation. Well known and familiar chemical methods, again, are abandoned, occasionally, for new methods of at least questionable utility. We may mention, among such, the use of hydrochloric acid, as the starting-point in alkalimetry.

The same remark applies to the apparatus described and illustrated. The woodcut on p. 142 *ex. gr.*, illustrative of the method for taking the specific gravity of ammonia, looks startlingly elaborate. Much credit is due, however, to the author and his coadjutor, Mr. Dugald Clerk, for the care bestowed upon the preparation of the woodcuts. We consider them, for the most part, well selected and well executed. There is that pleasing evidence to the chemical eye, that the illustrations have originated in the laboratory, and that they depict apparatus which can be practically used, and are not merely put in to please and catch the eye. In fact, when we compare some high-priced books of the class, which it would not be difficult to enumerate, with the elegantly got-up and cheap volume of Mr. Thorpe, we can only congratulate him on the book he has produced.

If we may be allowed to tender advice, we should say:—Condense the part on the operations of weighing; enlarge the number of examples of simple gravimetric analysis, so as to include the more important acids and bases; draw a line between determinations usually required in analyses for practical or commercial purposes, and the more elaborate complete analysis of the same bodies; and last, but not least, explain more fully, why and when one method answers better than another—if only in compassion for the weaker analyst.

We cordially recommend the book, and hope to see these suggestions adopted in the next edition, for which in all likelihood we shall not have to wait long.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Management of the British Museum

I BEG to protest against the remarks upon the management of the British Museum contained in your article of November 6. The general question whether a public institution of the sort is best governed by a public official or by a body of Trustees, may very likely admit of much discussion, but the decision should not be prejudiced by totally ignoring the noble work which has been and is being done by the Museum. No scientific man surely can be ignorant that the British Museum exists not so much for the momentary amusement of gaping crowds of country people, who do not understand a single object on which they gaze, as for the promotion of scientific discovery, and the advancement of literary and historical inquiry. We are told about the indifference of the Museum Trustees to the best interests of science, but we are not reminded frequently enough that it is almost impossible to carry out any scientific or literary inquiry in a complete manner, without resorting to the great national museum. There are doubtless many things which the Trustees have not done, but is it a slight matter that they have given us, on the whole, by far the most extensive and complete body of collections anywhere brought together in the world? The library and reading-room alone are enough to do honour to their management, and it is almost impossible to fathom the degree in which this library assists every kind of inquiry. When we are least aware, we are often enjoying the fruits of investigation in that library; the late Prof. Boole, for instance, spent the last few months of his life in the Museum, pursuing an exhaustive inquiry into previous writings on the subject of Differential Equations.

As regards the other collections, I presume that no one will call in question their enormous extent; and the fact that they are not adequately lodged and displayed as yet, is due to their very vastness, and to the fact that Government would not, until lately, afford the money for the new buildings. As regards the real interests of original inquiry, too, comparatively little harm is done by the want of room for exhibition, since *bonâ fide* scientific students can always obtain access to the collections.

I am far from denying that the officials who have conducted the South Kensington Museum have, by an enormous expenditure of public money, collected together a great quantity of beautiful objects of art, and have thus not only afforded opportunities for art study, but have made this museum a very agreeable and fashionable lounge. But I must protest against the notion, apparently countenanced in NATURE, that the scientific value and work of a national museum is to be measured by the number of millions of persons who saunter through the galleries. No doubt the utility of a museum in affording popular instruction and elevated amusement to large masses of people is very considerable, but this popular work is altogether of a different order from the strictly scientific object of collecting together all the products of intellect and of Nature. It is an unavoidable misfortune of the best and highest work in science that it is quite unobtrusive. The public is struck by the thousands who crowd the decorated galleries of South Kensington. There is nothing to attract public attention in the two or three hundred bookworms patiently plodding through the books in the Museum library, or the few students turning over the drawers of the zoological, botanical, mineralogical, numismatic, and other collections. But in NATURE, which has so powerfully advocated the necessity of promoting original research in this country, I should expect, more than anywhere else, to find a due appreciation of the noble work which is being carried out by the British Museum trustees, and by the staff of eminent scientific and literary men who are employed under their direction in promoting almost every branch of literature and science. We have heard many complaints of the apathy displayed by Government in the promotion of science. The existence of the British Museum is the best answer to that complaint. As regards those branches of science which demand the use of large collections, it may be regarded as the great national laboratory; and if scientific men do not make adequate use of it, that is their fault and not that of the trustees.

W. STANLEY JEVONS

[Our opinion of the immense importance to research of the collections of the British Museum is quite in accordance with the above letter of our esteemed correspondent, and if he will read the article again he will see nothing in it to indicate any difference of opinion. Indeed we regard the positions of the scientific men in the British Museum as positions of endowed research, and positions, moreover, which have amply justified it, miserable as the amount is in many cases. Our objection is to the existence of trustees not represented by a Minister, and to the action of the trustees, who have not expanded the area of the utility of the collections, and who have cared so little for the men of science working under them and the collections themselves that the former are underpaid and the latter are much less useful than they might be. Mr. Jevons concedes the whole point when he refers to the money so properly spent at South Kensington; for had the British Museum been under the same Minister, money would have been spent there too. The money must be spent unless we are to sink to the level of—well, let us say Morocco; and it is to prevent this that the proposed transfer has been suggested.—ED.]

On the Equilibrium of Temperature of a Gaseous Column subject to Gravity

IN NATURE, vol. viii. p. 486, Mr. Guthrie asks the question, "Is thre no possibility of testing the nature of thermal equilibrium of a column of still air?" I think to this question an answer may be given, which, though indirect and imperfect, will perhaps decide the controversy on the above subject.

If gravity causes in the temperature of a gaseous column the difference, which Mr. Guthrie thinks it does, that difference must be in proportion to the height of the column, and in inverse proportion to the specific heat of the gas. Hence it follows that, if two equal columns of different gases, both under the same thermal influence, are joined at their lower parts by a thermo-electric pile, the side of this pile, which is surrounded by the gas with the highest specific heat, must be constantly cooler than the other side. The result of my experiments respecting this, is the confirmation of Mr. Guthrie's opinion. The description of these experiments, and a theoretical treatise on the subject, have been in the hands of Prof. Poggendorff since the beginning of last June, and will be published in an early number of his *Annalen*.

I hope that my experiments will induce others to try them in the same or in another manner, in order to bring the question concerning the influence of gravity on the thermal equilibrium to a final decision. Should it prove in favour of Mr. Guthrie's theory, as I believe it will, this theory, represented till now only by a very small minority, although it was broached twenty years ago by Waterston,* will give rise to results† which may perhaps clear up many of our ideas about Kosmos.

The argument which Prof. Clerk-Maxwell has brought against Mr. Guthrie in NATURE, vol. viii. p. 85, does not appear to me to be generally correct. He says:—In a given horizontal stratum of a gaseous column subject to gravity, a greater number of molecules come from below than from above to strike those in the stratum, because the density of the gas is greater below than above. Certainly the number of molecules, which enter into such a stratum during a certain time, depends upon the density of the gas, but besides this, it depends upon the probability of entering into it, which exists for each molecule. Now, this probability is not only dependent upon the distance of a molecule from the stratum, upon its velocity, its direction and its encounters with other molecules, but also upon the very fact of its being above or below the stratum.

Gravity continually tends to diminish the distance between any horizontal stratum and each molecule which is above the stratum, and continually tends to increase the distance between the stratum and each molecule which is below. Hence it follows that the probability of entering into the stratum will be greater for a molecule which is above than for one below, if, in the case of both, all other circumstances are equal. For example, consider two molecules, which in a given moment move with the same velocity and in the same direction on the two sides of the stratum; if this direction is horizontal like the stratum, and if in the given moment the distances of the molecules from the stratum are both very small, in the next moment the molecule above the stratum will have entered into it, while that one below will have removed from it.

In the case of the density being greater below the stratum than above, more molecules would enter it from below, if gravity did not exist. But under the influence of gravity, the effects of the difference in density can be balanced by those of the above-mentioned difference in the probability, which exists for each molecule of entering into the stratum during a certain time. I even consider this last difference to be the dynamical cause of the difference in density.

Westend, near Berlin, Oct. 20 G. HANSEMANN

Periodicity of Rainfall

As far as my own figures are concerned, the reply to Mr. Meldrum's question is very easily afforded. I agree with him that it is undesirable to use averages deduced from groups of stations variable both in the number and locality of their components. The observations which I quoted were those of a single station, Halton, St. Philip, Barbadoes.

With respect to the general question, I regret being unable to share Mr. Meldrum's evident enthusiasm, and that a very different opinion has been published in the *Zeitschrift*, by Dr. Jelinek, one of the most eminent meteorologists of the present day. It may be convenient to some readers to be informed that an abstract of Dr. Jelinek's article is given in "British Rainfall, 1872," together with a general *résumé* of the state of the question up to the date of its publication.

Camden Square, Nov. 1 G. J. SYMONS

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* In "On Dynamical Sequences of Kosmos."

† I have expounded some of these results in an abstract mechanical form in "Die Atome und ihre Bewegungen" (Cöln Lengfeld'sche Buchhandlung, 1871).

THE COMMON FROG*

IV.

HAVING now passed in review the greatest differences presented by the nearest allies of our common frog (the members namely of its own order), certain facts of interest present themselves respecting the geographical distribution of the group. These facts are interesting, because they point not only to the exceptional nature of the faunas of South America and of Australia, but also to a certain zoological affinity between those two regions of the earth, distinct as they are from one another. Thus, as has been mentioned, it is only in Australia and South America that the typical genus *Rana* is absolutely wanting. One genus of Tree-frogs, *Pelodryas*, is confined to Australia, but is closely resembled by another genus, *Phyllomedusa*, which is restricted to South America, and differs from the former only by the absence of a web between the toes. It should be recollected that the primary subdivisions of a zoological order are termed *families*. One whole family, called *Cystignathidæ*, is (with the exception of two species) confined to Australia and America.

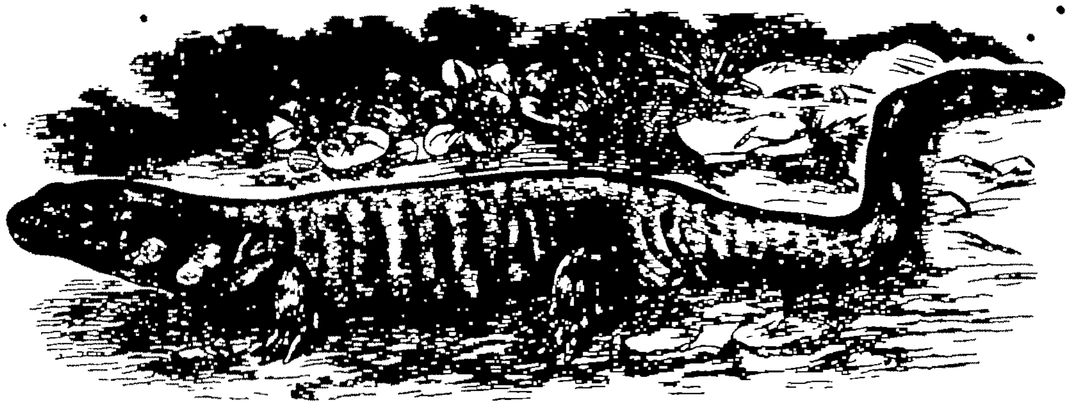
FIG. 16.—An American Eft of the genus *Amblystoma*.

The typical Tree-frogs (*Hyla*) abound in South America and are also found in Australia, but not in India or in Africa south of the Sahara. On the other hand another genus of Tree-frogs (*Polypedates*), is found in India, Japan, and Madagascar, but not in either Australia or America.

The typical Toads (*Bufo*) have, however, their headquarters in South America, yet are wanting in Australia, though they are found everywhere else where the order exists at all.

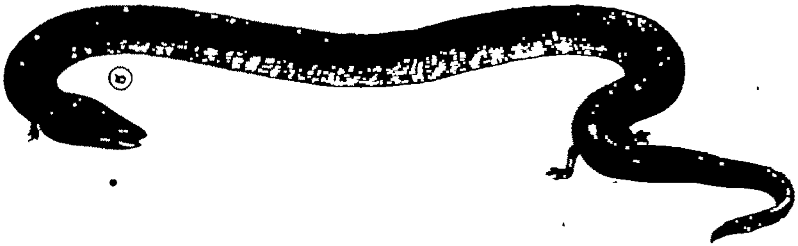
FIG. 17.—The *Amphiuma*.

The earth's surface, considered as to its population of the frog and toad order, may be divided into three great regions. The first of these is composed of Europe, Northern Asia (with Japan and Chusan), North America, and Africa north of the Sahara. The second region consists of Africa south of the Sahara, Madagascar, India, and the Indian Archipelago. The third region is made up of South America and Australia, and the resemblance between these two parts of the earth's surface as to their frogs and toads is paralleled by that as to their mammalian faunas, since marsupial mammals (or pouched-beasts of the opossum kind), are strictly confined to Australia (and its islands) and America.

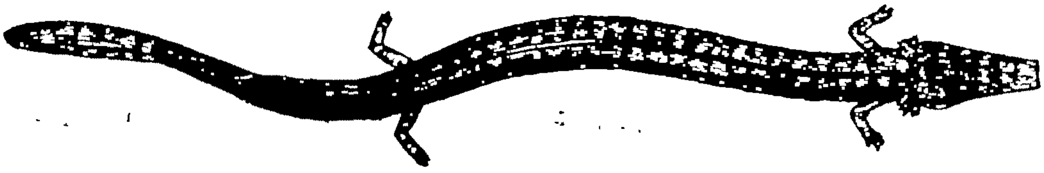
FIG. 18.—The *Proteus*.

No Frog or Toad has yet been found in New Zealand.

Africa, considering its size and climate, is poor in species of *Anoura*.

We should be prepared for the fact that in South America Tree-frogs abound, since all kinds ot animals in that region assume an arboreal habit.

Monkeys are tree-livers all the world over, but nowhere are all the indigenous species so thoroughly arboreal as in tropical America. There alone do we find monkeys with a prehensile tail capable of serving as a fifth hand, and so affording greater security and facility to locomotion amidst the branches. Only there also do we find beasts so exclusively constructed to pass the whole of their lives in trees that they can move along the ground only with difficulty—such is the case with the sloths. Porcupines, which in the old world have short tails, in the new world have long and prehensile ones. An animal allied to the Badger—the Kinkajou (*Cercoleptes caudivolvulus*)—similarly acquires in South America a long and prehensile caudal appendage. Even the Fowl and Peacock Order of Birds becomes in South America more strictly arboreal than elsewhere (being represented by the Curassows), and the very geese find there a congener (*Palamedia*) specially adapted to dwell in trees and destitute (like the frog *Phyllomedusa* before mentioned) of a web-like membrane between the toes.

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* Continued from p. 13.

We have now advanced a further stage in seeking a reply to the question, "What is a Frog?" We have now viewed it in the light to be derived from a consideration of the more noteworthy forms of the frog's order.

We may next inquire what are its next nearest allies? What other animals of the class Batrachia constitute an order which approaches nearest to the frog's order *Anoura*?

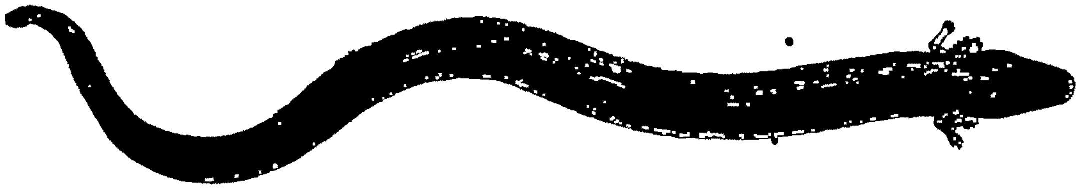

FIG 19 —The *Siren*

Almost every pond in England which harbours frogs, harbours also those little four-legged, long-tailed, soft skinned creatures termed *Efts* or *Newts* (of the genus *Triton*) familiar to every schoolboy.

These Newts which are thus by circumstances placed actually in juxtaposition with the frog are also zoologically his nearest allies outside his own (frog and toad) order. Like the frog they undergo a metamorphosis, at first appearing as Eft-tadpoles (with elongated external gills, but devoid of limbs), subsequently losing the gills and acquiring limbs. Efts, as is manifest, are widely and strangely different in form from frogs and toads.

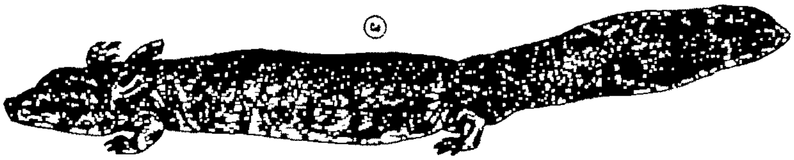

FIG 20 —*Menobranchus.*

Thus is justified the assertion before made as to the far less exceptional form of the human body than that of the frog. For when, amongst Mammalia, we go outside that *order* to which Man belongs, we find in his *class* other creatures (insect-eating, flesh-eating, and of the squirrel kind) which more or less closely resemble some of the lower members of man's order. When, however, amongst Batrachia, we go outside that *order* to which the frog belongs, we find in his *class* no creatures whatever which present anything like such an approximation to any members of the frog's order as is presented by the mammals above referred to certain members of man's order.

The Efts (or Newts) with their allies—hereinafter noticed—constitute the second order *Urodela* of the class Batrachia.

This order is very unlike the first and already described order (*Anoura*), in that it is composed of creatures which in many respects are strangely divergent; and though most of the species more or less resemble our own Efts (or Newts) in shape, yet the *Urodela* are very far from constituting such a homogeneous group as are the *Anoura*.

It will be well now to review some of the more striking forms contained in the order.

The Land Eft (*Salamandra*), though common in Holland and France (as well as the rest of Europe), is unknown in this country.

Genera allied to the European genera *Triton* and *Salamandra*, and to the American genus *Amblystoma*, may have the body and tail more and more elongated and the legs reduced, as in *Spelerpes*, *Chioglopa*, and *Œdipina*, till they attain the condition of *Batrachoceps*. The greatest excess of this development, however, is found in the North American genus *Amphiuma*, the minute limbs of which have either three or two toes, according to the species. These creatures are called by the negroes "Congo Snake," and are quite erroneously regarded as venomous.

The largest existing Urodele—the gigantic Salamander (*Cryptobranchus*)—is found in Japan, where it attains a length of 5 or 6 feet. A closely allied species inhabits China, and during the tertiary period one also inhabited Europe, the fossil skeleton of which being strangely supposed to be that of an antediluvian man received the curious appellation, "Homo diluvii testis."

In *Cryptobranchus* (as in all the Urodela yet enumerated except *Amphiuma*), though the young have gill openings and external gills, the adults are devoid of both.

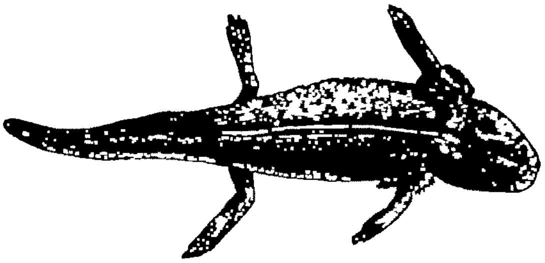

FIG 21.—The Axolotl.

In a North American genus, however (*Menopoma*), which, though smaller in size, closely resembles Cryptobranchus in figure, there is a permanent gill opening, though the gills themselves disappear in the adult, and the same is the case with *Amphiuma*. Thus in these animals the metamorphosis is less complete.

In the subterranean caverns of Southern Austria (Carniola and Istria) is found the *Proteus*. This is an elongated Urodele, with slender limbs, and but two toes to each hind foot. Passing its whole life in perpetual darkness, it is blind and colourless, except the external gills, which are red. This animal retains during the whole of life not only the gill aperture on each side, but also the external plumose gills which are transitory in the Anoura and in all the Urodela hitherto mentioned. Here then we first meet with an animal which may be said to be a permanent and persistent Tadpole, yet rather like an Eft-tadpole than like that of the Frog.

A North American Urodele, misnamed (for it is silent enough) *Siren*, also, presents us with permanent external gills, and it offers another interesting resemblance to the tadpole of the frog in that it is furnished throughout life with a horny beak. It has also another remarkable character in which it stands alone in its class. Hitherto every relative of the frog has had, like it, four limbs in the adult condition. In the Siren, however, we for the first time make acquaintance with a creature belonging to the class (though not to the *order*) of frogs and toads, which is devoid altogether of hinder (or pelvic) limbs, being in this respect like the whales and porcupines amongst beasts, and like the little lizard, *Chirotes*, amongst reptiles.

Another North American Urodele, *Menobranchus*, possesses throughout the whole of life both gill openings and external gills. But it is furnished with four limbs, and in other respects more or less resembles in appearance, as it does in size, the genus *Menopoma* before noticed.

Finally there is a genus of this order (*Urodela*) which has of late presented circumstances of peculiar interest. This is the Axolotl of Mexico, which was long considered by Cuvier to be a large Eft-tadpole, possessing as it does permanent gills and gill-openings, with some other characters common to the Eft-tadpole stage of existence. At length, however, its mature condition was considered to be established by the discovery that it possesses perfect powers of reproducing its kind.

For some years, individuals of this species have been preserved in the Jardin des Plantes at Paris, and a few years ago one individual amongst others there kept was observed, to the astonishment of its guardian, to have transformed itself into a creature of quite another genus—the genus *Amblystoma*, one rich in American species. Since then several other species have transformed themselves, but without affording any clue as to the conditions which determine this change—a change remarkable indeed, resulting as it does not merely in the loss of gills and the closing up of the gill-openings, but in remarkable changes with respect to the skull, the dentition, and other important structures.

There is, moreover, another and very singular fact connected with this transformation. It is that no one of the individuals transformed (although we must suppose that by such transformation it has attained its highest development and perfection) has ever yet reproduced its kind, and this in spite of every effort made to promote reproduction by experiments as to diet and as to putting together males and females both transformed, also transformed males with females untransformed, and males untransformed with females transformed. Indeed, the sexual organs seem even to become atrophied in these transformed individuals. Moreover, all this time the untransformed individuals have gone on bringing forth young with the utmost fecundity, no care or trouble on the part of their guardians being required to effect it.

A fact more noteworthy could hardly be imagined in support of the view of specific genesis put forward recently.* Here we have a rapid and extreme transformation taking place according to an unknown internal law of the species which transforms itself. No one, moreover, has been able to detect the conditions which determine such transformation (though it takes place under the eyes, and in the midst of the experiments of its observers). This latter fact affords abundant evidence how obscure and recondite may be the conditions which determine the transformations of specific genesis, and how utterly futile are observations as to an apparent homogeneity of readily appreciable conditions. They are so since it seems to be just such recondite ones which really determine the changes just referred to, and probably, therefore, other changes analogous to them.

It may be a question whether the genus *Menobranchus* may not also be a persistent larval* form, and one which now never attains its once adult form. If so, it is most probable that its lost state was similar to that of the exclusively American genus *Spelerpes*, the larva of which *Menobranchus* much resembles. With respect to *Proteus* and *Siren* no conjecture of the kind can yet be made.

Individuals belonging to the common English species (*Triton cristatus*) occasionally retain some of the external characters of immaturity, in spite of having attained reproductive capability; and a European species (*Triton alpestris*) often matures the generative elements while still, as to external appearance, more or less in its tadpole stage of existence. The adult condition, however, is normally and generally attained by it.

The geographical distribution of the *Urodela* is very remarkable. North America is the head-quarters of the order, and, with rare and trifling exceptions, the whole are confined to the Northern hemisphere. The exceptions are certain forms which extend down the Andes into South America, and one or two species of *Amblystoma*, which similarly descend along the highlands of South Eastern Asia. Urodeles are absolutely wanting in Hindostan, Africa south of the Sahara, the Indian Archipelago, Australia, and New Zealand. As might be expected, that part of Asia which is nearest to North America, namely China and Japan, is the region of the old world most richly peopled by species of *Urodela*. Altogether the world's surface may be divided according to its Urodele population into three regions. The first will comprise Europe, Africa north of the Sahara, and North Western Asia. The second will include Japan and Eastern Asia. The third will be formed by North America, with a slight extension southwards into South America—a division which by no means coincides with that indicated by the *Anoura*.

The above two orders (*Anoura* and *Urodela*) comprise all the animals most nearly allied to the common frog, of all those outside its own order. There is, however, another small ordinal group of animals which remains to be here noted, because of all existing creatures they come nearest to the frog, after the *Urodela*.

(*To be continued.*)

INAUGURATION OF THE LINNEAN SOCIETY'S NEW ROOMS

OPENING ADDRESS BY THE PRESIDENT

IT is now seventeen years since the Government first recognised the claims of our Society to encouragement and assistance on the part of the State, as one which devoted itself to scientific pursuits unremunerative to its members, but tending directly or indirectly to public benefit; and since then a sense of the justness of such claims on the part of pure natural science has become gradually more general. We are no longer in the days when a Peter Pindar could turn the Royal Society and its president into ridicule as boiling fleas to ascertain whether they turned red like lobsters. The *Times*, instead of a short leader dismissing the British Association meetings in a similar strain of banter, devotes daily, during the time of its session, half a dozen columns to the details of its proceedings. And our own department in natural science is now admitted to be one of the most important branches of general science, specially important in its relation to our material prosperity. Our food and raiment, the essentials of life, are derived exclusively from the animal and vegetable kingdoms, and biological products contribute largely to many of our luxuries, whilst on the other hand some of the greatest calamities with which we are afflicted are due to the rapid development of animal or vegetable life. Many are the associations, under Government as well as individual patronage, devoted to the improvement and increase of useful animals and plants; and of late attention has been also devoted to the arrest of the ravages of the noxious ones, the balance of natural selection being disturbed by the interference of agriculture and animal education. The due study of the means of restoring this balance, of turning it more and more in our favour, of calling in to our aid more and more of the hitherto neglected available species, or of the hitherto latent properties of those already in use, of checking the progress of blights and murrains, requires a thorough knowledge of the animals and plants themselves, and that thorough knowledge can only be obtained by that scientific study not only of particular animals and plants supposed *a priori* to be useful or noxious, but of *all* animals and plants, which it is the special province of our Society to promote. And in this respect I think it will be generally admitted that we have not been neglectful of our duty, and that we have done our part in rendering effective the support we have of late years received from Government as well as from individuals, and in establishing a sound claim for its increased continuance. Besides the aid afforded to scientific researches by our largely augmented library, the great value of the papers published in the recent volumes of our Transactions and Journal has been acknowledged abroad as well as at home. It is in our Society, for instance, that the great Darwinian theories were first promulgated; and it must be recollected that the five or six hundred copies of our publications regularly sent out, place the researches they exhibit at once at the disposal of the leading followers of the science in all parts of the world. It is true that these great additions to our efficiency are not entirely due to Government patronage, but are the direct results of the reforms introduced by Dr. Hooker in 1855. Those reforms, however, would have lost much of their effect had we remained confined to our old quarters in Soho Square. Cramped for space in those obscure and dingy rooms, it required a strong devotion to science to induce an adequate attendance at our meetings; and saddled with a heavy rent, we could neither purchase books for our library nor find room on our shelves for those presented to us.

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* See Genesis of Species, chap. xi.

* The young of the Frog or Eft is called a larva.

In the spring of 1856, however, an opening was made for our obtaining rooms in Burlington House. I was then on the Council, and joined heartily in the conviction of the importance of availing ourselves of the opportunity, notwithstanding the heavy expense it might entail, which I felt confident we could cover by a subscription amongst our fellows. Our President undertook the preliminary negotiations, and at the meeting of our Council on June 11 a letter was officially communicated to us addressed by the Secretary of the Treasury to the President of the Royal Society, allowing the temporary location in Burlington House of the Linnean and Chemical Societies with the Royal Society, upon certain conditions; those which affected us being, that the Royal Society should be put in possession of the main building of Burlington House on the understanding that they would, in communication with the Linnean and Chemical Societies, assign suitable accommodation therein for those bodies, and that the Fellows of the three societies should have mutual access to their three libraries for purposes of reference. Our Society, at a special general meeting held on the 17th of the same month, authorised the Council to take the necessary steps for carrying out the proposal of the Government, and in the following February 1857 the Royal Society assigned to us the rooms which we have since occupied under the above conditions. A subscription was organised which ultimately amounted to nearly 1,100*l*., sufficient to defray all expenses of parting with our old rooms and fitting up the new ones, with a very small surplus, which was carried to the general account. In the same month of February I was associated with our then active and zealous President and Secretary, and with Mr. Wilson Saunders as a Removal Committee, and on Tuesday June 2 the Society was enabled for the first time to meet in their new rooms.

Our position, however, although so great an improvement upon Soho Square, was not yet quite satisfactory. It was provisional only, and under the wing, as it were, of the Royal Society, and liable at any time to be exchanged for a worse or a better one as the case might turn out. This uncertainty is now removed. The Government, rightly understanding the relations which ought to prevail with the scientific societies judged to be deserving of their support, obtained from Parliament adequate means for providing ample accommodation to the six societies here located, without reserving any right of interference with or control over their scientific operations. Thus our new quarters have assumed a permanent and independent character, the rooms have been built and fitted up expressly for our Society, and, having followed out all the arrangements, I feel bound to acknowledge the effective manner in which the liberal intentions of Government have been promoted and carried out in detail by the architects, Mr. Barry and the late Mr. Bankes. When the plans for the new building were first being prepared, some six or seven years since, we were applied to for particulars of the accommodation we should require for our library and meetings, for the transaction of the business of the Society and for the residence of our librarian and porter. We were not consulted, it is true, about the general arrangements in relation to the other societies, and we have to regret the cessation of that close juxtaposition and intimate intercourse with the Royal Society which was so agreeable to us, but in all other respects our requisitions were fully complied with in the plans prepared and sent to us for approval, and the only alteration since made has been the curtailment of a portion of the basement premises in favour of the post-office, which rather inconveniently limits the stowage room for our stock of Transactions. With this sole exception we have the space we asked for, and the bookshelves and such other fittings as have been provided by Government have been worked out in the most satisfactory manner.

Our removal here has necessarily been attended with considerable expense, the precise amount of which cannot yet be calculated, but it will probably exceed 600*l*. The Council have, however, not thought it necessary to call for any special subscription. The investments made during the past year have been partially with a view to the present occasion, and the gradually increasing sale of our publications and general appreciation of the value of our labours has been so far adding to our receipts that we closed last session with a much larger balance in hand than usual, and we hope to clear ourselves of the liabilities we are incurring, without reducing our invested funds much below 2000*l*. At the same time, we must not conceal from ourselves that we shall be called upon for a considerable increase in our expenditure. Our enlarged accommodation, combined with high prices, will add much to our household expenses. We are threatened with a repeal of the Act which exempts us from parochial rates. Nearly the whole of our library having within the last three weeks passed through my hands, I have become convinced that it will require a large outlay in binding, as well as in filling up gaps to render it really efficient. And, above all, we must bear in mind that the chief means we have of promoting the scientific objects for which we are associated, the only way in which we can render them available to our numerous Fellows resident in our colonies, is through our publications, and heavy as have been of late years our printer's and artists' bills, they will and ought to become heavier and heavier still. To render fully available the assistance we have received from Government, we require continued and increased support from our Fellows, and from the scientific public. We reckon already among our Fellows the great majority of those who have acquired a name in zoology, or botany, and I sincerely hope that all men of means who take a sincere interest in biological pursuits will think it a pleasure as well as a duty to contribute directly or indirectly to the support of the Linnean Society of London.

With regard to future arrangements in the new phases of life into which the Society has entered, the Council has kept in view three great objects, the endeavour to render our Meetings attractive, the extended usefulness of our library, and the steady maintenance of our publications. On meeting-nights the library will be open at 7 o'clock, the chair will be taken in the meeting-room at 8 o'clock, as at present, and after the meeting the Fellows will adjourn to tea in the Council Room upstairs, opposite to, and in direct communication with the library. The extended shelf-room in the library has enabled a classification of the books which will render those most frequently consulted much more readily accessible than heretofore; and as evidence that there is no relaxation in our publishing department, I have to announce that besides the two numbers of our Journal, one in Zoology, and the other in Botany, which have been sent out since our last meeting, two new parts of our Transactions are in the course of delivery, the concluding one of Volume XXVIII., and the second of Col. Grant's Volume XXIX. The first part of Volume XXX. is in the printer's hands.

INAUGURATION OF THE CHEMICAL SOCIETY'S NEW ROOMS

On Thursday night last the Chemical Society met for the first time in the new apartments assigned to it in the right-hand front wing of Burlington House. The event was a notable one, and it is not often that such an occasion happens to the president of a hard-working body of scientific men as last Thursday fell to the lot of Dr. Odling when he rose to welcome the fellows to their new home, and he might well feel it his duty to break for once the tradition which imposes silence on the president on the first night of the session.

Dr. Odling accordingly rose and proceeded to bid them welcome to the new rooms, and then to give in a few words a general statement of what had been done in relation to the taking possession of them by the society. This it seems had been by no means an easy matter, as but a few days back the society was still in its old quarters without a book of its library moved, and the present apartments were in a damp and generally unfinished state.

Thanks, however, to the exertions of the Council and especially of the Junior Secretary (Dr. Russell), who were most kindly met and aided in their endeavours by Mr. Barry (the architect) and the Clerk of the Works; the new rooms were got into a habitable condition, the books in great part placed in their cases, and the meeting-room provided with seats in time for the first meeting of the session.

The rooms in question at present in use consist of the library, a noble room on the second floor, well capable of holding the books of the society for some time to come. That for meetings, below the library and overlooking Piccadilly, is capable of seating nearly twice the number of listeners that could be provided for in the old quarters. The seats, however, are somewhat crowded, and though the room is provided with double windows there is a considerable noise from the street. The president, however, held out hopes of a wooden or asphalt pavement being before long laid down in front of the building, and we hope a point of such importance will not long be neglected by the authorities. The most noticeable point, however, is a laboratory, placed on the right-hand side of the meeting-room and opening into it with double doors immediately behind the lecture-table. This, though at present not quite ready for use, is supplied with every fitting of a good laboratory, and will shortly be provided with the necessary apparatus and re-agents. According to the president, "whatever may be its subsequent use, it is intended at present to place it at the disposal of those authors who may wish to illustrate their papers with experiments." We do not know whether the words of the president imply an intention on the part of the society to aid research by granting the use of its laboratory in such cases as it may think deserving, but in any case the society deserves the thanks of every scientific man for so admirable an innovation as a room for the preparation of experiments.

Dr. Odling in his speech alluded to the "childish pleasure, childish in its earnestness and simplicity," with which a chemist looks upon a new experiment. We quite agree with him as to the fact of its existence, but we think that this desire to see answers a far higher purpose than that of mere pleasure. The science of the chemist is essentially a science in which, to quote a popular phrase, "seeing is believing," and nothing can be more wearisome than the constant repetition of the description of reactions, or the recounting of qualitative or quantitative results unenlivened by a single experiment. Such descriptions quite fail to lay hold upon the mind, except at the expense of a wearisome strain, and the consequence is that many a valuable paper loses half or all its effect when read (which should be to raise discussion), simply because in an attempt to describe facts the author loses sight of the necessity of succinctly generalising therefrom.

In the meantime what have the other societies affected by the changes in Piccadilly been doing to provide for the experimental illustration of papers? and especially what has the Royal Society done in the direction to which we have alluded? We are informed on the best authority —nothing! The rooms of the latter consist as did the temporary ones, simply of those requisite for the accomodation of the library and for the *reading* of papers. Now is the Chemical Society right? If so the Royal Society is wrong. It has not done all when it has provided comfortable reading-rooms for its members, and a place where its secretaries can read the papers to a few silent Fellows who are sparsely scattered over the benches. The reading and publication of papers is not all that a great and wealthy society can or ought to do for the advancement of science. Why should its laboratories not exist as well as its library?

There is no reason why the meetings of the societies instead of being, as some of them now are, dull reunions only attended by the Fellows as a matter of duty, should not be made more useful to men of science. What could be better than to see them attended by the more advanced of the younger students of science, as the meetings of the Chemical Society now very often are, who might there see how the better known workers demonstrate their discoveries, and how their papers are examined and discussed. Unless some attempt is made to give the other societies a greater grasp over the several classes of workers to which they more directly appeal, they will infallibly lose the guiding power they have hitherto had, and the advantages conferred by their organisation in the propagation of scientific knowledge will be lost. It behoves the Royal Society in particular to show the way to the others in following in the steps taken with such signal success by the chemists. If it does not do so, but allows itself to be left behind, it must soon see many of the most important papers sent to the Chemical or to such of the other societies as may choose to provide the means of properly illustrating them.

It may be urged that if papers are to be experimentally illustrated, all cannot possibly be read. We can only say so much the better. Why should not a society's council exercise a wise discretion, and relegate some classes of papers at once to the "Journal," the proper place for many a mass of numerical data now perforce read, but of which discussion is impossible?

F. C. S.

NOTES

We regret to announce the death, on the 10th inst., of Mr. B. F. Duppa, F.R.S., well known for his numerous and important researches in organic chemistry. He was educated at Cambridge, and was afterwards, in the year 1857, a pupil in the Royal College of Chemistry. Within a period of eleven years he published, partly alone and partly in conjunction with Mr. W. H. Perkin and Dr. Frankland, no less than twenty papers, most of which appeared in the Transactions and Proceedings of the Royal Society. The most important of these researches related to the action of bromine and iodine on acetic acid, the artificial production of tartaric acid, the formation of organic compounds containing mercury, and the synthetical production of numerous acids of the fatty and acrylic series. Mr. Duppa was elected a Fellow of the Royal Society in 1867. Being a man of independent means, he never applied for, nor held, any scientific appointment, but formed one of that small band of enthusiastic and disinterested amateur workers of whom England may justly feel proud, and to whom she is so much indebted for a very large proportion of the contributions which she has made to the progress of science.

Mr. Mitchell, of Old Bond Street, is, we believe, about to publish a portrait of the late Dr. Bence Jones, engraved by Holl from the beautiful drawing by Mr. George Richmond, R.A.

The following awards have been made by the French Geographical Society:—2,000 francs to M. Dournaux-Dupéré, who has just set out for Timbuctoo; this gentleman has also received a similar sum from the Minister of Public Instruction; 2,000 fr. to M. Francis Garnier, to aid him in his explorations along the Blue River in China, and which have Yun-nan and Tibet for their objects; 1,500 fr. to MM. Marche and Compiègne, who have already proceeded a considerable distance along the course of the Ogowe with the design of penetrating as far as the great African lakes, and joining Livingstone.

The subject for the Le Bas Prize (Cambridge) for the present year is "The Respective Functions of Science and Literature in Education." Candidates must be graduates of the University of not more than three years' standing from their first degree when the essays are sent in, which date is fixed before the end of the Easter Term, 1874. The essays must each bear some motto, and be accompanied by a sealed paper bearing the same motto, and enclosing the name of the candidate and that of his college. The successful candidate is required to publish the essay at his own expense.

Messrs. Trubner and Co. will publish, in about ten days, Mr. George Henry Lewes' new work, entitled "Problems of Life and Mind."

With reference to the paragraph in last week's Nature on the discovery of the conversion of spherical into plane motion, Prof. Sylvester writes: "I feel it an act of simple justice to another to say that I should never have hit upon the instrument which effects this, had it not been for the previous, wholly original and unexpected, discovery made nine years ago, by M. Peaucillier, of the conversion of circular into rectilinear motion, with which I was recently made acquainted by M. Tchebicheff, and which seems to have been little noticed in the discoverer's own country, and to have remained wholly unknown in this. M. Peaucillier has succeeded by the most simple means in solving a kinematical-problem which had baffled the attempts of all mechanicians, from our James Watts downwards, to accomplish, and a simple Captain of Engineers in the French army has actually accomplished by a stroke of inspiration the mathematical solution of a question which many of the most profound and sagacious mathematicians of the age have been long labouring, but necessarily (as it is now obvious) in vain, to prove to admit of none. The conversion of circular into rectilinear motion before M. Peaucillier's discovery was gradually growing to be classed in the same category of questions as the quadrature of the circle, and by a great number of mathematicians was actually deemed to be equally impossible in the nature of things. A working model of Peaucillier's machine constructed by my friend M. Garcia, the brother of Malebran and the inventor of the laryngoscope, is in my possession at the Athenæum Club, and several copies of it have been already made by its admirers, which term comprises all who have seen it. The wonderfully fertile kinematic and mathematical results which I have succeeded in educing from the simple conception involved in this machine may form the subject of another communication to Nature."

Prof. Jelinek, of Vienna, writes us that the death of Prof. Donati is the only unhappy event connected with the Meteorological Congress of Vienna, which in all other respects has proved successful. The fact of all countries of Europe (France excepted) and the United States of North America being represented at the Congress, and the conciliatory spirit in which all the proceedings were held, the general desire to arrive at an uniform system of observation and publication make us hope, he thinks, that further decisive steps in this direction will be taken. The Congress has expressed the wish, that another Congress of Meteorologists shall meet in three years, and it has appointed a permanent Committee under Prof. Ruys Ballot of Utrecht, as President, and with Prof. Bruhns of Leipzig, Cantoni of Pavia, Jelinek of Vienna, Mohn of Christiana, Director Scott of London, and Director Wild of St. Petersburg, as members to prepare the solution of certain questions especially relative to the best form of publishing meteorological observations and to the extension of the existing system of meteorological observations. The permanent Committee has been also charged with the preparatory steps towards the convocation of a second Maritime Conference (the first having been held at Brussels in 1853). There will be three editions of the proceedings of the Congress. The one German, the other French, the third under the care of Mr. Robert Scott, in English.

Rather an unusual incident has recently occurred in the Belgian Academy of Sciences, about which, according to the two gentlemen most concerned, erroneous statements have been made in the Belgian papers and *La Revue Scientifique*. The common statement is that at the *séance* of June 7 last M. E. van Beneden, son of the well-known Professor of Zoology at the Catholic University of Louvain, and himself Professor of Zoology at Liége, by appointment of the present Catholic Ministry, read a paper on the results of a voyage which he had recently made to Brazil and La Plata. Speaking of the difficulty of obtaining a dolphin on account of the superstitions of the Brazilian fishermen, he is reported to have referred to the ancient belief in Europe that dolphins were in the habit of bringing dead bodies on shore, and to have said, "The *fable* of Jonah is an embodiment of this belief." Thereupon, it is said, M. Gilbert, Professor of Mathematics, and M. Henry, Professor of Chemistry at Louvain, in a letter to M. Quetelet, the secretary, protested against the expression being allowed to pass uncensured, as it was a violation of their religious convictions, and an infringement of the traditional law of the Academy, that nothing be said to hurt the religious convictions of any member. At the next meeting of the Academy, October 11, M. Gilbert insisted on this note being read, but by the vote of the Academy the order of the day was at once proceeded with. Thereupon the two aggrieved professors felt called upon to resign their connection with the Academy. The real facts of the case are stated by MM. Gilbert and Henry in a long communication to the last number of the *Revue Scientifique*, from which it appears that the reference to "the fable of Jonah" was not in the paper at all as originally read, but was added in a note to the paper when subsequently printed in the *Bulletin* of the Academy. No doubt the two professors have a greater grievance than the irate Bishop Dupanloup had in the admission to the French Academy of M. Littré; and no doubt it is well in all scientific discussions in a mixed society to steer clear of "the religious difficulty" entirely, but after all it must seem to an outsider as if all this pother about "the fable of Jonah" were a case of "much ado about nothing."

A MEETING of the local executive of the British Association was held on Monday, at Bradford, and the financial account, which was submitted, showed the total expenses of the late meeting in that town to amount to about 3,300*l*. The guarantee fund subscribed amounted to 5,200*l*.

AT a recent meeting of the Manchester Scientific Students' Association at the Royal Institution, Mr. George C. Yates, F.S.A., exhibited a unique specimen of a Neolithic Flint Celt, or axe, which he had obtained at Holyhead a few weeks ago. The specimen, we believe, has been thoroughly authenticated, and Mr. Yates has consented to deposit it in the British Museum.

A SERIES of Birkbeck Scientific Lectures for the People was commenced last week at Leeds, by Mr. J. Norman Lockyer, F.R.S., to be continued by Dr. Carpenter, Mr. Miall, and Prof. Martin Duncan, till Christmas. We believe that the action of the Trustees in thus aiding the spread of scientific knowledge throughout the country will be attended with the best results.

ON Tuesday last a deputation of the Harrow Vestry, representing the residents, tradesmen, and other classes of the parish, had a second interview with the Governors of Harrow School, for the purpose of lodging and explaining thirty-six objections in detail to the proposed statutes for the government of the school. One point most justly insisted on by the deputation is the fact that John Lyon, the founder of the school, intended it mainly for the benefit of the parishioners of Harrow, whereas the Governors, like the Governors of others of our public schools, notoriously throw every possible difficulty in the way of children of common parishioners reaping the benefit of the fund generously left for their education. The Governors try to silence the complainants with a pittance of 250*l*. a year to found a subordinate school. We hope the Harrow Vestry will not cease to agitate the matter, until they obtain all that rightly belongs to them.

WE have received a revised list of those who obtained Queen's Medals at the Science and Art Examinations, May 1873.

A CORRESPONDENT at Cannes, France, informs us that on November 4, about 6 P.M., a beautiful and distinct, though faint, lunar rainbow was seen there, which lasted a quarter of an hour, and then suddenly disappeared just as the first drops of rain were felt.

THE forthcoming number of Petermann's *Mittheilungen* will contain an article by Messrs. E. Behm and F. Hanemann on the most recent discoveries in South-east Australia, accompanied by a map in which these discoveries are embodied.

MESSRS. W. AND A. K. JOHNSTON have published a very useful war-map of the Gold Coast of Ashantee and neighbouring countries, with a sketch-map of Guinea and a small map of the whole of Africa, all carefully disposed on one large sheet.

FOR several winters past courses of lectures, intended mainly for the industrial classes, have been given on scientific subjects in the Edinburgh Museum of Science and Art, by the professors of the University and other gentlemen eminent in their particular departments. The charge for a course of six lectures, the number given on each subject, is only sixpence, and we believe the results have been extremely satisfactory. The following is the programme for the present winter:—"Chemistry of the Common Metals," by Prof. A. Crum Brown, M.D.; "Physiology and Public Health," by Dr. John G. M'Kendrick, F.R.S.E.; "Cosmical Astronomy," by Prof. Tait; "The Carboniferous Formation of Scotland," by Mr. James Geikie, F.R.S.E.; "Weather and Climate," by Mr. Alex. Buchan, F.R.S.E.; "The History of Commerce," by Prof. W. B. Hodgson, LL.D.

THE same journal has the following details concerning the Italian Association of Men of Science:—Inaugurated in 1837 by the Grand Duke of Tuscany (twenty-five years before France had followed the parent movement in England), it fell under the ban of Pope and Bourbon alike, who saw in it the foster-mother of revolution. In spite of police restrictions and other proofs of the dislike with which it was viewed, its meetings gained in attractiveness every year till, in 1846, favoured by the early liberalism of Pio Nono and Charles Albert's ill-will to Austria, it celebrated the centenary of Balilla's throwing off the German yoke in the Ligurian capital. Thanks to Piedmont, it outlived the reaction of 1848; and in 1859–60 it shared in the national jubilee it had assisted in consummating. Rome, proclaimed as the capital in 1861, was to be the scene of its reunion in 1862; but the Vatican, countenanced by Austria and France, frustrated the attempt. The storming of the Porta Pia in 1870 rendered possible the long-cherished design, and, under the appropriate presidency of the venerable Count Mamiani, formerly Prime Minister of Pio Nono during his short constitutional reign, it met on the 20th ult. in the capital. One hundred and fifty was the muster of members—not a numerous one, but counting the most distinguished statesmen and *savans* in the kingdom. Donati had but lately fallen a victim to cholera, but his science was adequately represented by the Padre Secchi, who still clings to the Society of Jesus.

WE have received from Mr. D. Mackintosh a reprint of his article from the *Quarterly Journal of the Geological Society*, "On the more remarkable Boulders of the North-west of England and the Welsh Borders."

THE additions to the Zoological Society's collection during the past week include a Crab-eating Opossum (*Didelphys cancrivora*) from the West Indies, presented by Mr. G. H. Hawtayne; a Common Parodoxure (*Paradoxurus typus*) from India, presented by Mr. C. Maurer; an Indian Jackal (*Canis aureus*) from Penang, presented by Mr. F. H. Fredericks; three Robbin Island Snakes (*Coronella phocarum*) presented by Rev. G. H. R. Fisk; a Little Grebe (*Podiceps minor*), British, presented by Mr. H. P. Hensman; a Black Wallaby (*Halmaturus Ualabatus*) from N. S. Wales, purchased; a Gazelle (*Gazella dorcas*) from Egypt, deposited; an Axis Deer (*Cervus axis*) and a Molucca Deer (*C. moluccensis*), born in the Gardens.

PHYSICAL GEOGRAPHY AND TERRESTRIAL MOLLUSCA OF THE BAHAMA ISLANDS

A PAPER on this subject has recently been communicated to the Lyceum of Natural History, New York, by Mr. Thomas Bland.

The northern end of the Bahama group lies opposite southern Florida, and from this point the islands stretch off in a double series, nearly parallel to the trend of Cuba and San Domingo, and terminate, properly, in the Turk's Island Bank, on which are the last and most easterly of the chain, which extends about 600 miles, from within 70 miles of the coast of Florida to within 100 miles of that of San Domingo.

Several banks are distinguishable, and the islands are generally on the windward sides of these, never exceeding 200ft. in height, and being almost universally environed with reefs or shelves of rock, which extend often to a considerable distance and usually terminate abruptly.

The geological formation appears to resemble that of Bermuda; their form and surface condition being largely due to prevailing winds and currents, but also owing much, probably, to the configuration of the land on which the coral reefs were built up.

Lieutenant Nelson speaks of the Bahamas as the Gulf Stream Delta; thrown down where the stream receives a check from the Atlantic on emerging from the Gulf of Mexico.

In a communication to NATURE, vol. vi. p. 262, Mr. Jones furnished evidence of the subsidence of the Bermudas. In excavations made for the great dock *e.g.*, there was found, at 46 ft. below low-water mark, a layer of red earth, containing remains of cedar trees, and resting on a bed of compact calcareous sandstone.

Mr. Bland examines the evidence afforded (as to subsidence), by the distribution of land shells on the Bahama Islands. The total number of species known is about 80.

Judging from both operculates and inoperculates, the land-shell fauna of the Bahamas is essentially West Indian, and that of the Great Bank (especially), closely allied to the Cuban fauna. Mr. Bland gives a list of inoperculate species common to the Bahamas, the adjacent continent, Bermuda, and certain of the West Indian Islands; which shows in a marked manner the alliance referred to.

The banks and islands of the Bahama chain diminish in size to the south-east, indicating greater subsidence in that direction. Similarly, the submerged Virgin Island bank, Sombrero and the Anguilla bank, terminate the parallel West Indies chain eastward from Cuba; and in Anguilla have been found remains of large extinct mammalia which must have inhabited at one time a much more extensive area.

The author criticises Dr. Cleve's geological grouping of the islands north of Guadeloupe (in two groups, one comprising Bahamas, of post-pliocene date, another of the tertiary Eocene, Miocene, and Pliocene), and points out that the land shell fauna of Saba, St. Eustatius, St. Kitts and Nevis, of Redonda and Montserrat, and of Barbadoes and Antigua, is, in common with most of the islands to the south, to and inclusive of Trinidad, distinct from the fauna of the islands between and inclusive of the Bahamas and Cuba, and the Anguilla bank, on which are Anguilla, St. Martin and St. Bartholomew. This well-defined line of separation must be considered in connection with the past and present geological history of the islands.

Dana traces parallel bands of greater or less subsidence in the Pacific Ocean, and analogous conditions in the Atlantic; the subsidence was probably, he says, "much greater between Florida and Cuba than in the Peninsula of Florida itself, and greater along the Carribbean Sea parallel with Cuba, as well as along the Bahama reefs than in Cuba." Recent soundings, cited by Mr. Bland, confirm this view.

SCIENTIFIC SERIALS

Ocean Highways, November.—In an article on "The Results of the Arctic Campaign, 1873," it is shown that the right direction for Arctic Exploration has been unmistakeably indicated, further proofs have been afforded of the practicability of attaining an advanced position by following that direction, and additional evidence has been accumulated against the route advocated by "unpractised theorists." These conclusions are rightly drawn from the eminently successful results obtained from the *Polaris* expedition and from Captain Markham's fruitful cruise in the *Arctic*, as contrasted with the comparatively unsuccessful attempts made in the Spitzbergen direction by the Swedish Expedition and that of Mr. Leigh Smith. "The learned societies will be able to make their appeal to the Government with even stronger and more cogent arguments than were at their disposal in the end of last year; while in the present Prime Minister and Chancellor of the Exchequer they have an old and staunch supporter of Arctic expeditions, and one who has studied their history and appreciated their uses." There is a carefully constructed map illustrative of Captain Markham's voyage in the *Arctic*. Other articles are, "On the Distribution of Coal in China," by Baron von Richthofen; "South American Progress" (Argentine Republic), by F. J. Rickard; "Highways and Byeways of Naval History," the first of a series of articles by Mr. R. Lendall.

Gasetta Chimica Italiana, Fascicolo V. and VI.—The number commences with a paper on Santonin, by S. Cannizzaro and F. Sestini. Santoninic acid is described; it is derived from santonin by the addition of one molecule of water to one of santonin. The addition is effected by acting on santonin by means of a warm aqueous alkaline solution. The formula of the acid is $C_{15}H_{20}O_4 = C_{15}H_{18}O_3 + H_2O$. The properties of the acids and its salts are described, and the action of nascent hydrogen on santonin is then considered.—New researches on benzylated phenol, by E. Paternò and M. Fileti.—On the chemical analysis of some wines grown in the Veronese province, by Prof. G. Dal Sie. The wines in question seem to be somewhat strong, the percentage (volume) of alcohol ranging from 9·4 to 16·4. Very voluminous tables of analyses are given.—A paper on the dry distillation of calcic formate, by A. Lieben and E. Paternò concludes the original portion of the number, which concludes with 155 pages of abstracts from foreign journals.

Annalen der Chemie und Pharmacie, Band 168, Heft 2 and 3, August 30.—The number commences with two papers from Prof. Beilstein's laboratory. The first by W. Hemelian is on a new method of preparing the sulpho acids; the method in question is a modification of that of Strecker. Dr. E. Wroblevsky communicates a paper on certain haloid derivatives of toluol; he describes a number of the meta-brom-toluol compounds, and also deals with the para-brom-toluols and the tri-brom-toluols.—The other papers are: On selenic acid and its salts, by Dr. v. Gerichten. He finds that the seleniates are all isomorphous with the corresponding sulphates, and the double salts also agree with the double sulphates.—On the action of tri-sulpho-carbonate and sulpho-carbaminate of ammonium on aldehyde and acetone, by E. Mulder. A number of the compounds resulting from these reactions are described.—On a new mode of forming ortho-tolulilic acid, by R. Fittig and William Ramsay. On meta-toluic acid, by C. Boettinger and W. Ramsay.—On ethyl and di-ethyl-allyamine, by A. Rinne. Ethyl-allylamine is isomeric with methyl crotonylamine, the two bodies having the formulæ—

$$N\left\{\begin{matrix}C_3H_5\\C_2H_5\\H\end{matrix}\right. \text{ and } N\left\{\begin{matrix}C_4H_7\\CH_3\\H\end{matrix}\right. \text{ respectively.}$$

The author describes several of the salts of the former. Di-ethyl-allylamine $N\left\{\begin{matrix}C_3H_5\\C_2H_5\\C_2H_5\end{matrix}\right.$ is produced by the action of ethyl iodide on allylamine. The author describes it and its hydrochlorate and platino-chloride.—Researches on the isomers of cresol with regard to their occurrence in coal tar, by M. S. Southworth.—Researches on sorbic acid by E. Kachel and R. Fittig.—The number concludes with a very lengthy paper on the actions occurring in the inner non-luminous flame of the Bunsen burner, by R. Blochmann. The author has collected and examined the gases from various parts of the flame, and the memoir is illustrated with two plates showing the apparatus used, and the flames given by the burner under various treatments, and a diagram showing the percentages of CO_2 and H_2O, given by flames when burning, at various heights above the burner up to 120 millimetres.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, Nov. 4.—Prof. Newton, F.R.S., vice-president, in the chair. The Secretary read a report on the additions that had been made to the Society's menagerie during the months of June, July, August, and September. Mr. G. Dawson Rowley exhibited a singular malformed variety of the Domestic Duck, and the Secretary a collection of fishes (containing six examples of *Ceratodus forsteri*) made by Mr.

Ramsay, in Queensland.—A communication was read from Mr. J. B. Perrin, containing an account of the Myology of the Hoatzin (*Opisthocomus cristatus*).—A communication was read from Capt. R. Beavan, Bengal Staff Corps, containing a list of fishes met with in the River Nerbudda, in India.—A second communication from Capt. Beavan contained some remarks on certain difficulties involved in the acceptance of the Darwinian theory of evolution.—A communication was read from Mr. Montague R. Butler, containing descriptions of several new species of Diurnal Lepidoptera.—A communication was read from Mr. R. Swinhoe, H.B.M. Consul at Chefoo, on the Song-Jay of Northern China, with further notes on Chinese ornithology.—Mr. P. L. Sclater, F.R.S., exhibited and pointed out the characters of fourteen new species of birds collected by Signor Luigi Maria D'Albertis during his recent expedition into the interior of New Guinea.—A communication was read from Prof. J. V. Barboza du Bocage, on the Ground Hornbill of Southern Africa—*Buceros carunculatus cafer* of Schlegel.—A second communication from Prof. Barboza du Bocage contained a note on the habitat of *Euprepes coctei*, Dum. et Bibr.—A communication was read from Surgeon-Major Francis Day, containing descriptions of new or little known Indian fishes.—Mr. R. B. Sharpe, read a paper describing the contents of a collection of birds recently received from Mombas in Eastern Africa.—A second paper by Mr. R. B. Sharpe contained a list of a collection of birds from the River Congo.—Mr. G. B. Sowerby, jun., communicated the descriptions of eleven new species of shells.—A communication was read from Dr. J. E. Gray, F.R.S., on the skulls and alveolar surfaces of Land Tortoises, *Testudinata*.

Linnean Society, Nov. 6.—Mr. G. Bentham, president, in the chair.—Before the commencement of proceedings, this being the first occasion of the meeting of the society in its new rooms in Burlington House, the president gave an address on the present relation of Government towards the learned societies, which will be found elsewhere.—A resolution was then proposed by Dr. Hooker, seconded by Mr. Gwyn Jeffreys, and carried unanimously, recognising the obligations of the Linnean Society towards the Government for the handsome accommodation now for the first time provided independently for it.—On *Hydnora americana*, by Dr. J. D. Hooker. In his monograph of the Rafflesiaceæ in De Candolle's "Prodromus," Dr. Hooker had thrown some doubt on the correctness of De Bary's description *Hydnora*, and on the close affinity which he traced between it and *Prosopanche*. Further investigation has, however, amply confirmed the accuracy of De Bary's description. A very great difficulty is presented, from the point of view of the theory of evolution, in the occurrence of two species of this genus, one in South Africa and one in South America, so closely resembling one another in every point of their structure, and both root-parasites, that it is impossible to look upon them otherwise than as very nearly related. The only possible connection between them would appear to be through *Cytinus*, another nearly allied genus of root-parasites, species of which are natives both of South Africa and of South and North America.

Chemical Society, Nov. 6.—Dr. Odling, F.R.S., president, in the chair.—The president delivered a short address, to which we refer elsewhere, congratulating the Fellows on taking possession of their new rooms in Burlington House. A paper was then read by Mr. David Howard on the optical properties of some modifications of the cinchona alkaloids, being an elaborate investigation of the variations in the rotatory powers of this class of bodies when examined by the polarimeter. The other communications were—a preliminary notice on the oils of wormwood and citronella, by Dr. C. R. A. Wright; on the estimation of nitrates in potable waters, by Mr. W. F. Donkin; and a note on the action of iodine trichloride upon carbon disulphide, by Mr. J. B. Hannay.

Royal Microscopical Society, Nov. 5.—Chas. Brooke, F.R.S., president, in the chair. A paper by the Rev. W. H. Dallinger was read, describing some further researches made by himself and Dr. Drysdale on the development of certain monads, in the course of which they had been able to trace the life-history of a species, although in their earliest stages these organisms were so minute as to require an objective of $\frac{1}{50}$ in. for their observation. A number of beautifully executed drawings accompanied the paper.—Mr. Alfred Sanders read a paper on the art of photographing microscopic objects, in which he described a simple and successful process of manipulation, and showed how the most satisfactory results might be obtained without the aid of expensive and complicated apparatus.—A paper was also read by Mr. S. J. McIntire, entitled "Some Notes on Acarellus," in which he minutely described a species found parasitic upon Obisium, and which he believed to be identical with Hypopus, described by Dujardin. Specimens both mounted and alive were exhibited under the Society's microscopes.—Some photographs of *Navicula lyra* and *Amphipleura pellucida*, taken by Dr. J. J. Woodward, were also exhibited.

PARIS

Academy of Sciences, November 3.—M. de Quatrefages, president, in the chair.—The following papers were read:—An analysis and criticism of an "Essay on the Constitution and Origin of the Solar System, by M. Roche," by M. Faye.—On the mutual action of voltaic currents by M. Bertrand.—On the verification of Baume's hydrometer, by MM. Berthelot, Coulier, and d'Almeida.—On certain calorimetric values and problems, by M. Berthelot.—Observations of the solar protuberances during the last six solar rotations (April 23 to October 2, 1873) with some consequences affecting the theory of the spots, by Father Secchi. In this paper Secchi continued his observations, portions of which appeared in the first half of the year. The author again asserted that the spots are the product of eruptions, and observed that some metals were more opaque than others, *e.g.*, a sodium eruption gave a very black spot. He admitted, however, that some spots existed without eruptions.—Researches on the thermic effects accompanying the compression of liquids, by MM. Favre and Laurent.—MM. Morin and Phillips presented a report on M. Graeff's paper on the *régime* of rivers and the effects of a multiple system of reservoirs.—Memoir on experimental teratology, by M. C. Dareste.—On a map of the world on a gnomonic projection, &c., by M. B. de Chancourtois.—The following papers were presented to the Academy:—Observations on M. Dubois' paper on the influence of refraction at the moment of contact of Venus with the Sun's limb, by M. Oudemans.—On a new volatile saccharine matter extracted from Madagascar rubber, by M. Aimé Girard.—On the cooling effects produced by the joint actions of capillarity and evaporation: Evaporation of carbonic disulphide on porous paper, by M. C. Decharme.—Origin and formation of the dental follicule in the mammiferæ, by MM. Magitot and Legros.—On capillary embolism and hemorrhagic infarctus, by M. Bouchut.—Observations on M. Pellarin's note on choleraic dejections as agents in the propagation of that disease, by M. H. Blanc.—On the different practical problems of aërial navigation, by M. W. de Fonvielle.—On the formation of swellings on the rootlets of the vine, by M. Max. Cornu.—Observations on M. Guérin Méneville's suggestion that the *Phylloxera* is a result of the vine disease.—Note on the best dimensions for electro-magnets, by M. Th. du Moncel.—On a process for the preparation of active amylic alcohol, by M. J. A. Le Bel.—On the influence which certain gases exercise on the preservation of eggs, and on the influence of certain substances in the preservation of eggs, by Mr. C. Calvert.—On the metamorphism and physiological changeability of certain microphytes under the influence of media and on the relation of these phenomena to th initial cause of fermentation, &c., by M. J. Duval.—On the action of the respiratory apparatus after the opening of the thoracic cavity, by MM. Carlet and Strauss.—On the different properties and structures of the red and white muscle in rabbits and in rays, by M. Ranvier.—On scurvy and its treatment, by M. Champouillon.—On telluric intoxication, by M. L. Colin.—On the calcareous spar of the green marles of Chennevière, by M. Stan. Meunier.

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THURSDAY, NOVEMBER 20, 1873

THE ARCTIC EXPEDITION OF 1874

THE prospect of the Government being convinced of the propriety of despatching an Arctic Expedition, really seems to be brightening. We expressed some apprehension, when the Royal Geographical Society addressed the late Chancellor of the Exchequer on the subject last year, that sufficient pains were not taken to have all branches of Science represented in the Deputation, and that, consequently, the importance of the results of Arctic Research had not been completely explained. There is no cause for any such doubt on the present occasion. The matter has been most carefully and maturely considered by a joint committee appointed by the Councils of the Royal and the Royal Geographical Societies, and consisting of representatives of various departments of Science as well as of the most eminent Arctic authorities.

A memorandum has been drawn up, and submitted to the Council of the Royal Society, in which the scientific results to be obtained from the examination of the unknown area round the North Pole are set forth; the different sections having been prepared by men who are in the first rank as authorities in their particular departments of study—namely, geography, hydrography, geodesy, physics, meteorology, geology, botany, zoology, and anthropology. The memorandum also includes a carefully prepared statement, drawn up by distinguished Arctic authorities of the practical aspects of the question, the composition of such an expedition, the precautions that should be taken, and the best route.

The Royal Society is a body which, from its high position and from its strong sense of responsibility, never takes action without very careful and mature previous consideration. When this body once adopts a course on any question, the public can always feel satisfied that it has first received the closest attention, in all its bearings, from men of the highest attainments. The memorandum of the Committee has been before the Council, and we are able to announce that the value of the scientific results to be derived from Arctic exploration has been recognised, and that the Royal Society is prepared to represent to the Government the desirability of undertaking the discovery of the unknown region.

With the object of inducing the Government to undertake a North Polar Expedition, the Council of the Royal Society has appointed a deputation to represent their views, consisting of Dr. Hooker, the President-elect, Prof. Huxley, Prof. Allman, Mr. Prestwich, Mr. Busk, Mr. Sclater, and General Strachey.

The British Association has also appointed a Committee with the same object.

The Royal Geographical Society will be represented by its President, Sir Bartle Frere, Sir Henry Rawlinson, the veteran Arctic explorer, Sir George Back, and Admirals Collinson, Ommanney, and Sherard Osborn.

The Dundee Chamber of Commerce is also deeply impressed with the practical importance of discovery in the unknown area, and has drawn up a memorial to be presented to the Prime Minister, through the member, Sir John Ogilvy. Dundee is not only the principal whaling port of Great Britain, but is also the centre of a great and thriving industry, namely, the manufacture of jute, the growth of which employs millions of ryots in Bengal. Now, in the process of preparing the jute fibre, the use of animal oil is essential, so that the business of chasing whales and narwhals in the Arctic seas is of the utmost importance to the cultivators of the Gangetic delta. One industry supports the other, and India, as well as Great Britain, has an interest in Arctic discovery. The Chamber of Commerce, considering the vast interests at stake, holds it to be most important that the unknown polar region should be explored, in order that a more complete knowledge may be acquired of the haunts, migrations, numbers, and habits of the various oil-yielding animals. The Chamber also feels the advantages derived from Arctic expeditions by the best among the experienced mates and harpooners who obtain employment, and indirectly by the whole seafaring population of the west coast of Scotland. Nor are the bold seamen and enterprising manufacturers of the northern ports, any more than the naval officers and men of science, indifferent to the old renown of their country, and to the immense advantages which are derived from voyages of discovery.

The events of the last year have strengthened the arguments in favour of an Arctic Expedition. We believe that the despatch of a naval officer to Baffin's Bay last spring was due to the forethought of Admiral Sherard Osborn. The choice was undoubtedly a fortunate one, for Captain Markham entered heart and soul into the spirit of the service on which he was employed. He studied the new system of ice navigation, and of handling powerful steamers in the ice with minute attention. He had the rescued crew of the *Polaris* on board for several months, and learned from Dr. Bessels and Mr. Chester all the particulars of their extraordinarily successful voyage. Nothing escaped him, and on his return he submitted a full and most valuable report. Thus the fact that a ship can pass up Smith Sound to 82° 16′ N. without check of any description, unknown before, is now established, as well as the constant movement and drift of the ice in the strait leading to the unknown region. The revolution in ice navigation, caused by the use of powerful steamers, is also more fully understood and appreciated through the report of Captain Markham.

The deputation which is about to seek an interview with Mr. Gladstone and Mr. Goschen, is thus strengthened with fresh arguments and with a more exact and complete statement of the objects of Arctic research. It will represent interests which cannot be neglected, and bodies whose individual opinions must needs carry great weight. There will be the Royal Society, the recognised adviser of the Government on all matters relating to Science; the Royal Geographical Society, the British Association, and the Dundee Chamber of Commerce representing the interests of a great industry and of the sea-faring population of Scotland. The navy will also be fully represented, and the leading Arctic authorities will be present, acting in perfect unanimity as regards the route to be taken and the work to be done.

We believe that such a deputation must have considerable influence on the decision of the Government, and that there is every prospect of sanction being given to the fitting out of a naval Arctic expedition in 1874. Mr. Goschen is, we have reason to think, now conversant with the subject, and, as the Minister whose duty it is to advance and foster the interests of the British navy, it is impossible that he can fail to see the advantages of Arctic service. He is supported, at the Admiralty, by Sir Alexander Milne, who has ever been friendly to such enterprises, and sensible of the excellent school for naval men afforded by voyages of discovery; and by Admiral Richards, the hydrographer, whose sound judgment and great Arctic experience render his advice most valuable.

The Prime Minister, with whom the decision will rest, is a statesman who well knows the general, as well as the scientific uses of Arctic enterprise. He formed one of that Ministry which despatched the last scientific expedition to the Arctic Regions; and, as a member of the Select Committee of the House of Commons on Sir John Ross's case, he signed a report expressing his approval of Arctic voyages in the strongest terms—"A public service is rendered to a maritime country, especially in times of peace, by deeds of daring, enterprise, and patient endurance of hardship, which excite the public sympathy and enlist the general feeling in favour of maritime adventure." Such were, and we trust still are, the views of Mr. Gladstone with reference to the general uses of Arctic voyages of discovery. When to these general impressions are added a knowledge of the important scientific and practical results to be attained, the assurance that there is no undue risk, that the cost will be comparatively slight, and the good both to the navy and to mercantile interests incalculable, we cannot bring ourselves to believe that the decision of Mr. Gladstone will not be favourable to a renewal of Arctic research.

LOCAL SCIENTIFIC SOCIETIES*

II.

ALTOGETHER, so far as we have been able to ascertain,† the number of existing local societies ‡ which have for their main, or only as a part of their object the culture of Science, that were established in the years between 1781 and 1830, are only 22. We shall see that the increase since 1830 has been enormous, though the large majority of those established during the last forty-three years are of a much more simple kind, so far as organisation is concerned, than those established during the former period, have to a great extent a different object in view or rather accomplish the intellectual improvement of the members after a different fashion, and are, we think, thoroughly characteristic of the scientifically inquisitive and increasingly intelligent period during which they have been established. Not many "Literary and Philosophical Societies" have been established during the latter period, most of them being professedly devoted to study and research in Science, especially in natural history, in all or one of its branches, and a large majority of them being Field Clubs, as those associations are called, the whole or part of whose programme is to investigate the natural history (including botany, zoology, and geology) of particular districts, in combination sometimes with their archæology. Indeed, the last forty years might well be designated the era of field clubs.

We have already mentioned the Northumberland, Durham, and Newcastle Natural History Society, established in 1829, which, although it has done some excellent field-club work, was not professedly established for this purpose. There can be no doubt that the first genuine field-club was the Berwickshire Naturalists' Club, founded September 21, 1831, though Sir Walter Elliot traces the true origin of field-clubs to an association of students, formed in 1823 at the University of Edinburgh, under the name of the Plinian Society, for the advancement of the "study of natural history, antiquities, and the physical sciences in general." They met weekly in the evening during the session, from November to July, for reading papers and discussions; and also, as the season advanced, made occasional excursions into the neighbouring country. The chief promoters of the scheme were three brothers named Baird, from Berwickshire; but John, the eldest, must be considered the founder. He drew up an elaborate code of laws in eighteen chapters, and, as the first president, made a statement of the proposed plan and objects of the society at their inaugural meeting on the 14th January 1823. Among the original members occur the names of James Hardie, J. Grant Malcolmson (both Indian geologists), and Dr. John Coldstream; and, at a later period, those of Charles Darwin * (of Shrewsbury, 1826), John Hutton Balfour (1827), and Hugh Falconer (1828), with others who have since become distinguished in the scientific and literary world. The latest notice of the society is the session of 1829-30, up to which time the Bairds, although they had left the University, appear as occasional contributors.

No doubt this Edinburgh Association had considerable influence in originating the Berwickshire Club, for two of the Bairds became parish ministers in Berwickshire, and it was they, along with their brother, the late Dr. William Baird, of the British Museum, Dr. Johnstone, Dr. Embleton, and four or five others, who met at Coldingham on the date above given, and drew up the plan of the Berwickshire Naturalists' Club, "a term," Sir W. Elliot remarks, "now first extended to a scientific body." Its object was declared to be the "investigation of the natural history of Berwickshire and its vicinage;" in reality its field extends over the whole of Berwickshire, Roxburghshire, and the north-east part of Northumberland, to the limits of the Tyneside Club's district. The rules of the club, as all rules should be, are short, providing that the club should hold no property, require no admission fee, and should meet five times in the year at a place and hour to be communicated to each member by the secretary. Thus the Berwickshire Club is a field-club pure and simple, having, unlike many other similar clubs, no winter meetings for the reading of papers, whatever papers are read being read after dinner on the days when excursions are made. At the first anniversary it numbered 27 members, and in 1870, when Sir Walter Elliot gave his address, there were 249 members on the roll, including a few ladies, and "two corresponding members, the last description having been added in 1868 to admit intelligent working-men," though why this invidious distinction should be maintained in a body solely devoted to scientific research, we fail to see; surely Science at least is a common ground on which all classes can meet without a shadow of bitter class-feeling to mar the geniality of intercourse. The more that the higher tastes and recreations are common to all classes, the less room will there be for misunderstanding and bitterness. If a working-man can pay the subscription—and the field-club subscription is usually small, and working-men's wages are now unusually high—by all means let them be received on a common footing with the other members. Many of our best field-clubs are composed almost entirely of working-men, and every encouragement should be given to this class to join such clubs, for, morally and intellectually, we think they will reap more benefit from such associations than any other class.

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* Continued from vol. viii. p. 524.

† We regret to say that none of the Edinburgh Societies have seen meet to forward us information.

‡ We do not include in this article the great London Societies, as the Royal, the Linnean, the Astronomical, &c.

* The first paper contributed by him, entitled "On the Ova of the *Flustra*," in which he announces that he has discovered organs of motion, and, secondly, that the small black body hitherto mistaken for the young of *Fucus loreus* is in reality the ovum of *Pontobdella muricata*, exhibits his early habits of minute investigation.

The Berwickshire Club continues to be one of the most efficient and productive in the country, the fruits of its excursions being contained in six goodly volumes, containing many valuable papers on the natural history and archæology of its large district, and extensive and carefully compiled lists of the existing and extinct fauna and flora. As the Berwickshire Club is the model after which, to some extent, all succeeding field-clubs have been formed, we shall here give from Sir Walter Elliot's address, its simple and inexpensive method of conducting its field-days:—"Arrangements are made with the railway companies for the issue of tickets on favourable terms. The members assemble at breakfast at 9.30, after which the programme of the day is explained, and any objects of interest procured since the last meeting are exhibited and described. At 11 the party proceeds on foot or by conveyance to the points indicated, breaking into sections for botanical, geological, or antiquarian research, and either meeting again at some convenient spot, or returning independently to dinner at 4 o'clock. The members present rarely exceed from 30 to 50, often fewer. Of course the hive contains a considerable proportion of drones who rarely appear, ladies never. The distances are so great, the excursions so thoroughly directed to investigation, that few but those intent on work attend. After a frugal repast, the staple of which is a fine salmon invariably sent from Berwick, papers are read and discussed, and the members disperse according to the exigencies of their trains. The whole expenses of the day vary from four to five shillings per head."

In the decade between 1830 and 1840, other sixteen local societies were formed, many of which, though not professedly field-clubs, have done, through individual members, good field-club work, as is testified by their publications, and have otherwise done much to promote the cause of Science in the neighbourhood. It was during this period that the Cornwall Polytechnic Society (already mentioned), the Penzance Natural History and Antiquarian Society, the Royal Institution of South Wales, the Ludlow Natural History Society, and the West Riding Geological and Polytechnic Society, were formed, each of which, in its own particular fashion, does good service to Science, and helps to keep the lamp of culture burning in its neighbourhood.

No other regular field-club was instituted until nearly fifteen years after the foundation of the Berwickshire Club, when a sort of offshoot of that Society was formed in 1846 in Newcastle-on-Tyne, under the title of the Tyneside Naturalists' Field Club, which, "guided by the experience of the parent club, at once assumed a perfect organisation." The constitution was, however, somewhat amplified, a proviso being put in the rules that should assuredly have a place in the rules of every similar society in the kingdom. Its last rule, we think, worthy of all commendation and universal imitation; it is as follows:—

> "That the Club shall endeavour to discourage the practice of removing rare plants from the localities of which they are characteristic, and of risking the extermination of rare birds and other animals by wanton persecution; that the members be requested to use their influence with landowners and others, for the protection of the characteristic birds of the country, and to dispel the prejudices which are leading to their destruction; and that consequently the rarer botanical specimens collected at the Field Meetings be chiefly such as can be gathered without disturbing the roots of the plants; and that notes on the habits of birds be accumulated instead of specimens, by which our closet collections would be enriched only at the expense of nature's great museum out of doors. That in like manner the club shall endeavour to cultivate a fuller knowledge of the local antiquities, historical, popular, and idiomatic, and to promote a taste for carefully preserving the monuments of the past from wanton injury."

We have more than once recently in noticing the proceedings of some societies, and it has been animadverted on in other quarters, referred to the pernicious practice of encouraging, by the offer of prizes for rare specimens, especially of plants, the extermination of the rare flora peculiar to certain districts. One of the prime duties of every local club should be the preservation of such rare specimens, the fact of whose existence is often of great value from a scientific point of view, and the destruction of which, by transference to a herbarium, can serve no good purpose whatever. The Tyneside Club is divided into six sections, each charged with a special department for investigation:—1, Mammalia and Ornithology; 2, Amphibia, Ichthyology, Radiata; 3, Mollusca, Crustacea, Zoophytes; 4, Entomology; 5, Botany; 6, Geology. This club holds meetings during the winter in Newcastle. Up to 1864, it had published six volumes of very valuable Transactions. In that year an arrangement was come to whereby the members (numbering 429), became associates of the Northumberland, Durham, and Newcastle Natural History Society, already referred to. Thenceforth, as we have already said, the proceedings of the two bodies have been published conjointly under the title of "Natural History Transactions of Northumberland and Durham," of which three volumes have been published. "The work of the Club," Sir Walter Elliott says, "has been most conspicuous in zoology. It has the merit of publishing its lists and catalogues in a separate form for sale, so as to make them accessible to all inquirers."

We cannot mention in detail the foundation of the swarm of field-clubs which have come into existence in the various parts of the country since 1846; we can only allude very briefly to two of the most important, the Cotteswold and the Woolhope, the former an offshoot of the Berwickshire Club. The originators of the Cotteswold Field Club, which, like the Tyneside Club, was started in 1846, were Sir Thomas Tancred (who had been a member of the Berwickshire Club), Mr. T. B. Lloyd Baker (the well-known originator of the "Reformatory System"), Dr. Daubeny, of Oxford, Hugh Strickland, and some others, who met "at the Black Bull Inn, in Birdlip, a village on the summit of the Cotteswold range overlooking the vales of Gloucester and Worcester, about six miles south of Cheltenham, and seven south-west of Gloucester." There the club was inaugurated, Mr. Baker being elected the first president. "The labours of the club have been most conspicuous in geological investigation, for which the district offers such a rich field. Many of the members have, by their recorded observations, attained to high distinction. In the words of the president, 'It will suffice to mention the names of Daubeny, Strickland, Woodward, Maskelyne, Wright, Moore, Buckman, Jones, Lycett, Brodie, Symonds, Maw, and Etheridge, all members of the club, to recall at once names of writers well known in the scientific annals of the county, and of whom some have by their works obtained a more than European reputation.'"

The Woolhope Club, in Bedfordshire, whose publications are also well known as among the most valuable of those of provincial societies, was formed in 1851, and derived its name from the mass of Silurian rocks described by Sir Roderick Murchison as the "Woolhope Valley of Elevation." This club and the Cotteswold have occasional joint field days, and their example is followed by several other societies, and might, we think, with advantage be followed much more extensively than it is.

The Worcestershire Naturalists' Club originated in the same year as the Cotteswold, followed the year after by the Huddersfield Naturalists' Society, and in 1849 by the Yorkshire Naturalists' Club. Besides the four field-clubs mentioned, other six societies originated in this decade, most of them distinctly scientific, including the Torquay Natural History Society, the Bristol Microscopic Society, and the Isle of Wight Philosophical and Scientific Society.

In the decade between 1850 and 1860, twenty-two local scientific societies were founded, of which sixteen are field-clubs, including such well-known names as the Woolhope, just mentioned, the London Geologists' Association, the Liverpool Naturalists' Field Society, the Bath Natural History and Antiquarian Field Club, and the Malvern Field Club.

(*To be continued.*)

HARTWIG'S "SEA AND ITS WONDERS"

The Sea and its living Wonders. A popular account of the Marvels of the Deep, and of the progress of Maritime Discovery from the earliest ages to the present time. By Dr. G. Hartwig. Fourth edition, enlarged and improved, with numerous woodcuts and eight chromoxylographic plates. (London: Longmans, 1873.)

NO other evidence is needed beyond the publication of the fourth edition of this work to prove the demand there is in Great Britain for this kind of literature. The reading public want to know what about the sea, and all that is in it; and, in their eagerness to know, they buy even such books as this. When will scientific men turn their attention towards teaching the public as far as it can be taught, in a correct, yet popular manner, the rudiments of biological science? When they do the time for such books as the one we must now notice will have passed away, and the resources of the great publishing firm who issue it will be engaged on more truly solid and important work. As an indication of what we mean, let us contrast the popular works of Hartwig or Figuier with Quatrefages' "Souvenirs d'une Naturaliste," or Gosse's "Devonshire Rambles;" or let the reader imagine what a delightful work the one before us would have been if written by, say Huxley, Allman, Gunther, or Wyville Thomson. But to return to this volume, which consists of three parts; (1) the Physical Geography of the Sea; (2) the Inhabitants of the Sea; (3) the Progress of Maritime Discovery. The latter part commences with the maritime discoveries of the Phœnicians, and ends with a reference of sixteen lines in length to the numerous scientific voyages of circumnavigation of the present century.

Before proceeding to very briefly notice Parts I. and II., we have to object most strongly to the woodcuts not being drawn to any scale; thus, on page 101 the Rorqual is figured as rather smaller than the Herring, while, on the same page, and just above these figures, will be found a Whale Louse, and a Lepas represented as bigger than either. Surely figures like these must terribly mislead the ordinary reader, who, though he may possibly have some notion of the size of a herring, cannot be supposed to be aware of the dimensions of the whale's parasites. Many of the woodcuts are very good, but several of them are bad, and the majority of them are not seen in this volume for the first time; this we would not so much object to if the woodcuts were selected to illustrate the text, and not, as is too often the case in this work, the text written so as to make some forced allusion to the woodcuts.

Though the Dugong is illustrated by copying the woodcut from Tennent's work on Ceylon, yet scarcely a word is to be found about it in the chapter on the Cetacea. The Tailor birds' nest is figured on page 143, but no allusion whatever is made to it in the text. The great Auk is figured, and in the accompanying explanation is said to congregate in vast flocks on the rocky islets and headlands of the Northern Coasts. Surely a little careful supervision would have prevented such mistakes as these occurring. But leaving the subject of the woodcuts, we come to consider the letterpress; and here, too, not only a more careful supervision, but some more acquaintance with the subject would have been desirable. Why, among the Fishes, should the Anchovy have five lines devoted to it, when not one word is to be found about that equally important little fish, the Sardine? and surely half a page would not have been too much to devote to that interesting living wonder of the sea, the Whitebait. It would be an easy, but withal a useless task to point out other errors of omission and commission among the other classes.

Among the Corals and Sponges the author had enough to guide him, for he has borrowed wholesale the really beautiful woodcuts illustrating Prof. Greene's Manuals; if he had borrowed equally largely from their text, he would have made this the most trustworthy portion of his book.

No notice is taken of such important new forms as Rhizocrinus, or Brissinga, nor do we find mention under the Sponges of such strikingly beautiful ones as belong to the genera Euplectella, Holtenia, &c., though, indeed, some allusion is made to these in the chapter on the geographical distribution of marine life. But perhaps we have said enough to show that while the subject of this work is a good one, it might easily have been treated by a writer more familiar with it in a better, a more original, and a more comprehensive manner.

E. P. W.

OUR BOOK SHELF

The Theory of Evolution of Living Things. By Rev. G. Henslow. (Macmillan and Co.)

SCIENTIFIC men cannot but feel how false is the stimulus given to that form of literature of which the above-named work is an example. If considerable pecuniary reward is offered for the production of treatises in favour of any theory, or of the mutual compatibility of any two or more different doctrines, the work will undoubtedly be produced, however inaccurate the theory, or however dissimilar the doctrines. That mistaken enthusiasm which led to the production of the Bridgwater Treatises and the establishment of the Actonian Prize, has resulted in the publication during the last year of two Actonian prize essays, the former of which, by Mr. B. T. Lowne, we noticed on a previous occasion, whilst the latter is the one under consideration. The present author's treatment of his subject is much that which would have been adopted by Paley if he had been living at the present day. Several previously accepted axioms are shown to be incompatible with the existing position of biological science, and their weakness is well brought forward. Other considerations of modern development are introduced, and it is in these that the difficulty of combining the two doctrines appears. For instance, the origin of moral evil is said to be "the conscious abuse of means, instead of using them solely for the ends for which they were designed." But on evolutionary principles, it can hardly be said that there are means for designed ends, because that peculiarity in an organ which is of service is the only one retained, insomuch that if the delicate sensitiveness of the conjunctiva of the eye were to prove of more value to the individual than its sight, the power of vision would most probably become lost at the expense of the developing tactile organ. "The continual effort of beings to arrive at mutual and beneficial adjustments" is said to be a great principle of nature; does not the term "struggle for existence" imply something very different from this? Again, that "animals and plants do not live where circumstances may be best suited to them, but where they *can*, or where other animals and plants will respectively let them live," is quoted by the author as an instance of Nature falling short of that absolute degree of perfection which may be conceived as possible; however, there cannot be many who think a locality a suitable residence, in which they are prevented from taking up their abode, or perhaps entering, by the animals and plants which inhabit it. In other places similar weaknesses may be found in the argument adopted. In one thing Mr. Henslow has done great good: he has shown that it is consistent with a full dogmatic belief, to hold opinions very different from those taught as natural theology some half century and more ago.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Transfer of the South Kensington Museum

I AM glad to see that an effective opposition is likely to be made to the ill-advised proposal of the Government to place the South Kensington Collections under the control of the fifty irresponsible Trustees of the British Museum.

In common with many other naturalists I had always hoped that the national collections of natural history, when removed to the new buildings in South Kensington, would be freed from the rule of the Trustees and placed under a responsible director. The memorial of which I enclose a copy, and the republication of which would, I think, be opportune at the present juncture, will serve to show that I am by no means alone in believing that such a change would be beneficial to Science.

It would seem, however, that the Government, so far from acceding to our views, have resolved to proceed in exactly the contrary direction, and to increase the power of the Trustees. I can only hope that we may succeed in preventing them from carrying this retrograde measure into effect.

P. L. SCLATER

44, Elvaston Place Queen's Gate, Nov. 17

"*Copy of a Memorial presented to the Right Hon. the Chancellor of the Exchequer*

"To the Rt. Hon. the Chancellor of the Exchequer

"Sir,—It having been stated that the scientific men of the metropolis are, as a body, entirely opposed to the removal of the natural history collections from their present situation in the British Museum, we, the undersigned Fellows of the Royal, Linnean, Geological, and Zoological Societies of London, beg leave to offer to you the following expression of our opinion upon the subject.

"We are of opinion that it is of fundamental importance to the progress of the natural sciences in this country, that the administration of the national natural history collections should be separated from that of the library and art collections, and placed under one officer, who should be immediately responsible to one of the Queen's Ministers.

"We regard the exact locality of the National Museum of Natural History as a question of comparatively minor importance, provided that it be conveniently accessible and within the metropolitan district.

GEORGE BENTHAM, F.R.S.
WILLIAM B. CARPENTER, M.D., F.R.S.
W. S. DALLAS, F.L.S.
CHARLES DARWIN, F.R.S.
F. DUCANE GODMAN, F.L.S.
J. H. GURNEY, F.Z.S.
EDWARD HAMILTON, M.D., F.L.S.
JOSEPH D. HOOKER, M.D., F.R.S.
THOMAS H. HUXLEY, F.R.S.
JOHN KIRK, F.L.S.
LILFORD, F.L.S.
ALFRED NEWTON, F.L S,
W. KITCHEN PARKER, F.R.S.
ANDREW RAMSAY, F.R.S.
ARTHUR RUSSELL, M.P.
OSBERT SALVIN, F.L.S.
P L. SCLATER, F.R.S.
G. SCLATER-BOOTH, M.P.
S. JAMES A. SALTER, F.R.S.
W. H. SIMPSON, F Z.S.
J EMMERSON TENNENT, F.R.S.
THOMAS THOMSON, M.D., F.R.S.
H. B. TRISTRAM, F.L.S.
WALDEN, F.L.S.
ALFRED R. WALLACE, F.Z.S.

"London, May 14, 1866"

Deep-sea Soundings and Deep-sea Thermometers

WILL you allow me to reply to a letter from Messrs. Negretti and Zambra that appeared in vol. viii. p. 529, in reference to my Casella-Miller Deep Sea Thermometer, in which they accuse me and the late respected Dr. Miller of "plagiarism."

I presume, by this remark, that they intend to convey the idea of their own introduction having been imitated, because they state also that "their thermometer is identical in every respect except in size." Without venturing to trespass upon your valuable space by now going into more detail to prove the contrary, I will merely remark that if you, or any of your numerous readers who may feel interested in this subject, will favour me with a visit to my establishment, I shall be happy to give the fullest explanation as well as show the great difference existing between the two, will point out the cause of failure in their arrangement, and also the reason of the complete success of my own thermometer.

Though perhaps it is unfortunate for your correspondents that their reference to Dr. Miller was not made during his lifetime, yet, admitting that he said he was not aware of their arrangement, I must ask in all seriousness, What had their thermometer accomplished to make any one acquainted with it?

Facts speak for themselves. Their arrangement still remains without result, whilst my thermometer, which has solved the great problem of the true temperature of the sea even at its greatest depths, has been adopted not only by our own Government, but also by all the principal Governments and scientific authorities throughout the world. LOUIS P. CASELLA

147 Holborn Bars, Nov. 3

Squalus spinosus

ON the 9th inst. the fishermen of Durgan, in Helford Harbour, sent for me to look at a fish new to them, which had been caught (with a ½*d.* hook) on the preceding night near its entrance. Congers had been numerous, but suddenly ceased to bite. The fish (a spinous shark) had been hooked in the corner of its mouth, out of the reach of its sharp teeth, had wound the line many times round its body, which was 7 ft. in length, and 30 in. in girth, being longer and more slender than one of which I sent a notice to the Royal Cornwall Institution 38 years ago. The back, sprinkled over with spines, was of a dark grey colour, the belly nearly white. It was a male fish. The lobes of the liver were 4 ft. in length. In the stomach was a partially digested dogfish, 2 ft. long. The upper lobe of the tail was muscular and long, perhaps to aid its ground feeding, the lower lobe more marked than in Dr. A. Smith's drawing, as given by Yarrel, and entirely unlike that of the Filey Bay specimen. Twelve hours or more after its capture, when all external signs of life had disappeared, I was surprised to observe the regular pulsations of the heart.

Prof. Huxley has not observed a correspondence between the mass and large convolutions of the brain of a porpoise and its intellectual power.

Several years ago a herd of porpoises was scattered by a net, which I had got made, to enclose some of them. It was strong enough to catch tigers if set in the straits of Singapore, across which they sometimes swim. The whole "sculle" was much alarmed, two were secured. I conclude that their companions retained a vivid remembrance of the sea-fight, as these cetacea, although frequent visitants in this harbour previously, and often watched for, were not seen in it again for two years or more.

Trebah, Falmouth, Oct. 27 C. FOX

Zodiacal Light

IT is a matter for regret that with the magnificent opportunities of investigating the character of the Zodiacal Light afforded to Maxwell Hall by his elevated position in Jamaica, he does not seem to have brought the powers of either the spectroscope or polariscope to bear on it.

I think the full importance of the inquiry is hardly appreciated by many. Taking the generally accepted theory of the light—that of a lens-shaped disc of luminous matter, with the sun for its centre and a diameter exceeding that of the earth's orbit—its matter, lying as it does in the plane of the elliptic, actually connects us with the sun, and may be the medium through which the solar magnetic forces act upon our own.

The intimate connection between solar outbursts, auroras, and terrestrial magnetism is an established fact.

To the aurora, the zodiacal light is by many conceived to be nearly allied, and I do not think the evidence hitherto adduced against this theory is at all conclusive. The remarkable wave of light seen by Maxwell Hall is strongly in favour of it; and though spectroscopic observations seem to point the other way, they are as yet so scanty in number that it would be as unfair to argue from them the want of connection between the two phenomena, as it would be to assert that the planets have no volcanic fires of their own because they only give us a reflected solar spectrum.

Assume the zodiacal light to consist of solid particles of matter—planet dust—shining by reflected light, and it is not difficult to imagine the aurora playing amongst these tiny worlds, each of which might have its own small magnetic system, swayed like our own by the master magnet, the sun.

So far as my own experience goes I can see no objections to this assumption. Though I have seen the light very brilliant in both its branches, I have never yet found it to have a decided outline. Nor have I been able to trace it either east or west to 180° from the sun. Granting that this can be done, however, the apparent vanishing point of the earth's shadow lies comparatively near us, and far within this again is the point at which the shadow would subtend only a degree or two of arc, and at which it would be very hard to discern mid the feeble light of this portion of the zodiacal light; so that a slight extension of the diameter of the disc would remove any objection that might be raised under this head.

Imagine one of Saturn's moons revolving in an orbit within his belts, and fairly embedded in the matter, which, for the sake of the argument, we must assume to be illuminated by the planet. To inhabitants of that satellite each night would bring a phenomenon closely resembling our zodiacal light, only far more brilliant. At midnight two cones of light would taper upwards east and west, and meet overhead. The brightest portion of each cone would be that along the axis and nearest the horizon. Towards the summit and on the borders, where the line of sight would lie through less depths of matter, the light would gradually fade away, but from the satellite being embedded in the belt, the entire sky would be more or less luminous.

Has it not been noticed on our earth that when the zodiacal light has been seen unusually bright, a "phosphorescence" of the sky was everywhere visible? May this not arise from our solar belt in a somewhat similar manner?

From my personal observations I see no reason to give a lenticular form to the disc. Parallel faces would afford a perspective such as the zodiacal light appears to me.

I would urge observers who may be fortunately situated, not to neglect opportunities. So far as I am able I shall do my best to aid the work of inquiry, and with the powerful instruments that Browning is forwarding me, placed at an elevation of more than 6,000 ft., under the clear skies of our Indian winter, I trust I shall be able to add something to our knowledge of the zodiacal light.

I should feel much indebted to any of your readers who would inform me which is the best adapted polariscope for such researches, and whose (amongst makers) speciality such instruments are. E. H. PRINGLE

Camp Udapi, South Canara, Oct. 3

Cold Treatment of Gases

ALLOW me to submit to your readers the following sketch of an apparatus for producing extreme cold, by which it might perhaps be practicable to liquefy or even solidify the elementary gases which have hitherto resisted the efforts of chemists.

The gas to be operated on is compressed to any required degree by means of one cylinder, is cooled to the lowest convenient degree in the ordinary way, passes into an expansion cylinder with a properly arranged cut-off, where in expansion its temperature is still further lowered. From the expansion cylinder it returns back to the compression cylinder, extracting the heat from the counter current proceeding from the compression cylinder, so that the latter will be always arriving at the expansion cylinder with a continually decreasing temperature.

As out here I have no possible means of trying whether there is anything in this idea, I offer it to any of your readers who may feel disposed to try it.

Graaff Reinet College, Cape Colony, T. GUTHRIE
July 19.

The Relation of Man to the Ice-sheet

MR. TIDDEMAN has shown for Yorkshire what I proved six years ago for the South of England in a paper in the *Geological Magazine* (vol. iv. p. 193), that glacial conditions have obtained in this country since its occupation by Palæolithic man. Unfortunately an attempt which I made to explain this coincidence between his result and mine in a letter to the same periodical in February last was rendered abortive by a clerical (or perhaps printer's) error. I would press upon geologists to consider whether the point proved is not that a glacial period has intervened since the times of Palæolithic man and the present, rather than that man existed in this country before the glacial epoch, I think Mr. Tiddeman thinks as I do; but I take the liberty of stating this view more distinctly. O. FISHER

Wave Motion

IN NATURE, vol. viii. p. 506, Mr. Woodward has suggested a simple and ingenious illustration of wave motion. Could he, or any other correspondent, supply, or refer to, a popular explanation of the action of the particles upon each other, to which the propagation of the wave is due?

In the case of sound waves, the propagation is comparatively simple, and is fully and clearly explained in Dr. Tyndall's "Lectures on Sound," and elsewhere. Helmholtz, in his "Popular Lectures," has figured the motion of the individual particles of which a water wave is composed. And in Sir John Herschel's "Familiar Lectures," there is an elaborate and beautiful demonstration of the motion of the particles of ether in plane and circularly polarised light; but neither of these expositions appears to deal with the mode of propagation of the motion by which the wave is formed.

On the other hand, Sir Charles Wheatstone's ingenious model beautifully exemplifies the interaction of *waves* and their results. But here the waves are produced by the wooden wave forms introduced into the machine, the beads representing the particles remaining fixed in relation to each other. Neither, therefore, can this explain the manner and direction of the actual impact of each particle upon the adjacent one (beginning with those in contact with the source of motion itself), to which, combined with the tendency to yield in the direction of least resistance, the water wave must owe its form, and upon which the still more complicated conception of the light wave must ultimately depend.

Could a reference be given to any practical explanation of this point, it would confer a benefit on many who are not competent to follow the subject into the higher mathematics. M. F. E.

Sussex, Nov.

Elementary Biology

I, ALONG with many others, who are desirous of obtaining an insight into Nature, would esteem it a great favour, and it would be of the greatest benefit to us, if any of your scientific readers would undertake to give through your columns a short account of the various low forms of life included under the elementary stage of biology of the Science and Art Department. They might give instruction as to where the various objects could be seen, how inspected, names of the best text-books for the students' guidance, &c.

By so doing, they would secure the praise of many who at present cannot find out the modes of studying such subjects.

Hull, Nov. 8 BIOLOGY

Black Rain and Dew Ponds

CAN any of your readers explain the cause of this phenomenon? On Thursday, the 4th Sept., about 5 P.M., in the village of Marlsford, in the valley of the Thames, near Wallingford, a heavy storm of rain occurred: and the water which fell in several parts of the village was found to be nearly black. It is described as being of such a colour as would be produced by mixing ink with water. Another of these black water showers fell during the night of the following Friday.

Would any reader of NATURE also kindly set forth the theory upon which the utility of the dew ponds, found in many of the highest points of the Berkshire Downs, rests. They are circular ponds made with considerable care, and are supposed to receive so much dew as to supply all the water needed for the sheep in their neighbourhood through the driest summer.

Tiverton E. HIGHTON

ALBANY HANCOCK

THE brief announcement by which some of our readers may have first learnt of the decease of one of our greatest biologists is, in its simplicity, in singular harmony with the life the close of which it commemorates.

The retrospect of so serene a career leaves little to the biographer, for its points seem marked rather by phases of study, as indicated by important scientific memoirs, than by incidents which the world regards as striking or noteworthy.

Albany Hancock was born at Newcastle-on-Tyne on Christmas Eve, 1806. His father, Mr. John Hancock, died some six years later, and of the six little children thus left dependent on their mother, Albany was the third. He received a good education as times then went, and on leaving school was articled to a solicitor of good standing in Newcastle. Uncongenial as was the employment, he served his full term, passed the customary examinations in London, and even took an office in Newcastle with the view of establishing himself in practice. But the occupation was irksome, and he gave it up ere long to join a manufacturing firm, and this in turn circumstances led him soon to abandon. The simple fact probably was that neither occupation permitted him to follow the bent of his inclination, and that the desk and counting-house were alike distasteful to a mind pre-engaged as was his by other currents of thought. His early taste for natural history pursuits was probably in part derived from the collections, chiefly conchological, formed by his father, who was in many ways a man of superior ability, and had been something of a naturalist; and association with the late Mr. Robertson and Mr. Wingate, the one a botanist, the other an ornithologist, of repute; with the well-known Mr. Bewick; and above all with his near neighbour Mr. Alder, confirmed his inclination in this direction. He was as a boy, clever with his fingers, and that manual dexterity which in later years served him so well when engaged with dissecting needle and pencil, exhibited itself in many of the pursuits of his early life.

The first mention we find of Mr. Hancock's devotion to natural history is in Mr. Alder's "Catalogue of Land and Fresh-water shells," published in 1830, in which the author handsomely acknowledges the obligations he is under to him and to Mr. John Thornhill "for the communication of many habitats observed during their active investigation of this as well as other branches of the natural history of the neighbourhood" of Newcastle. His earliest appearance as an author seems to have been in connection with two short papers in the first volume of "Jardine's Magazine of Zoology and Botany," published in 1836, the one a "Note on the Occurrence of *Raniceps trifurcatus* on the Northumberland Coast," the other a "Note on *Falco rufipes*, *Regulus ignicapillus* and *Larus minutus*." These notices were, comparatively speaking, of trifling significance, but they were the beginning of a long series of contributions to knowledge which only ceased when his last illness deprived him of the power of continuous work. It is unnecessary here to enumerate the successive memoirs that embody the results of his life's labour. A catalogue of the original papers of which he was author, or joint author, would extend to something over seventy titles.

Early association with Mr. Alder in the study of the mollusca led to the production between the years 1845 and 1855 of their magnificent "Monograph of the British Nudibranchiate Mollusca," which may still be taken as a standard of excellence amongst such publications. Many of Mr. Hancock's earlier papers were devoted to the elucidation of the boring apparatus of the mollusca, and these were followed by similar researches respecting the excavating power of a group of sponges (*Cliona* and allied genera) which until that time had been but little known or understood.

As an anatomist—and after all it was his large knowledge of minute anatomy and infinite skill in dissection that gave its especial value to most of his work—he was, perhaps, best known by his elaborate memoir on the Organisation of the Brachiopoda, published in the Philosophical Transactions for 1857; but many other papers of the same thorough and original character proceeded from his pen. Amongst them will be remembered the following:—"On the Olfactory Apparatus in the Bullidæ" (1852); "On the Nervous Systems of *Ommastrephes todarus*" (1852); "On the Anatomy and Physiology of the Dibranchiate Cephalopoda" (1861); "On the Structure and Homologies of the Renal Organ in the Nudibranchiate Mollusca" (1863); "On the Anatomy of Doridopsis" (1865); "On the Anatomy and Physiology of the Tunicata" (1867).

For some years previous to his death Mr. Hancock had devoted much attention to the fish of the Carboniferous period, and in conjunction firstly with Mr. T. Atthey, whose fine collection afforded ample material for the purpose, and subsequently with Mr. Howse, published a series of fifteen papers on these coal-measure fossils.

The promised Monograph of the British Tunicata, preparations for which had made some progress even before the death of Mr. Alder, had occupied much of his time; and though probably still unfinished, it may be hoped that the results of his investigations are so far complete in themselves, that the work, as far as it has gone, may be saved to science. A supplement to the Monograph of Nudibranchiate Mollusca had been a matter long on his mind, but one that he had never been able to devote himself to realising, beyond the collection of materials.

Allusion has been made to Mr. Alder, Mr. Atthey, and Mr. Howse, as having been associated with Mr. Hancock in certain of his papers; to these must be added the names of Dr. Embleton and the Rev. A. M. Norman as occasional colleagues.

On the establishment of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne in 1829, Mr. Hancock became an active supporter, and was one of the original staff of honorary curators; and on the formation of the Tyneside Naturalists' Field Club in 1846, he was one of its principal and most influential promoters. When the new College of Physical Science in Newcastle was instituted, his name, almost as a matter of course, was placed on the provisional committee; and it was only when this body had completed its labours and gave place to a permanent board, that he was permitted, on the ground of ill-health, to retire from active service in connection with the institution. He was a Fellow of the Linnean Society, a corresponding-member of the Zoological Society of London, an honorary member of the Imperial Botanico-Zoological Society of Vienna, and perhaps of some other similar bodies; but honours of this sort, though valued in their way, were thrust upon him rather than sought. Though living a retired life, no man more highly prized social intercourse. His kindly helping hand was held out to every young naturalist: Such were always welcome at his house; and when appealed to by them, as was often the case, he made their difficulties his own till he could help to solve them.

It is yet too soon to attempt to shake oneself free from a sense of his presence, or to essay to weigh in judicial balance the value of his contributions to human knowledge: considerations of this sort are overwhelmed in the sense of irreparable loss to science.

H. B. B.

FERTILISATION OF FLOWERS BY INSECTS *

IV.

On the two forms of flower of Viola tricolor, and on their different mode of fertilisation.

VIOLA tricolor presents a further example of the same kind of dimorphism as that described in the last article in the case of Lysimahcia, Euphrasia, and Rhinanthus. One of its two forms, illustrated by Fig. 15 in natural size, is more conspicuous than the other (Fig. 16), not only by its larger size, but also by the more striking colour of its petals. When the flower has just opened, its two upper petals are light violet, or, in rarer cases, nearly white; but they gradually become a deep violet, or even dark blue. Far more striking is, ordinarily, the change of colour in the two lateral petals and the lower one, which, immediately after the opening of the flower, are nearly white, while in a fully-developed state they are always violet. The petals of the small-flowered form of *Viola tricolor*, illustrated in natural size by Fig. 16, are, on the contrary, uniform in colour and nearly white during the whole time of flowering. The attractiveness for insects of the two kinds must therefore be very different, whereas those particular marks round the opening of the flower which serve as a guide to insects in search of the honey, the "Saftmal" of Sprengel, are nearly the same in the two varieties. That part of the lower petal immediately before the entrance of the flower (*y*, Fig. 21, 22) is in both dark yellow, and the lower petal is also marked by black streaks converging towards the same entrance. There is only this difference between the two forms as to their guide-mark (*Saftmal*), that in the large-flowered form seven black streaks on the lower petal, and three on each of the lateral ones point towards the entrance of the flower; whereas in the small-flowered form there are but five black streaks in the lower petal, and none at all on the lateral ones.*

Although these two forms have been generally known, at least since the time of Linnæus, all botanists who have published observations on the fertilisation of *Viola tricolor* have apparently turned their attention exclusively to the large-flowered form (Fig. 15), whose beautiful adaptations to cross-fertilisation by insects, have been, therefore, very accurately described; while the peculiarities in structure and fertilisation of the small-flowered form have not even been mentioned. If, in this case, we clearly see that even scientific inquirers have been far more attracted by the larger violet flowers than by the smaller whitish ones, we need not wonder that insects are influenced in like manner, and that from this cause smaller and less conspicuous flowers are so frequently quite overlooked by insects, that they would rapidly become extinct, unless slight modifications of structure and development enabled them to produce seeds by self-fertilisation.

Indeed, in *Viola tricolor*, as in those species hitherto considered, regular self-fertilisation in the small-flowered form is effected by such slight modifications of structure and development, that by far the larger number of the contrivances in the large and small-flowered forms are identical.

In both forms, honey is secreted by two long appendages (*n*) of the lower filaments (*fi*), from which it ascends by adhesion into the uppermost part of the hollow spur (*sp*); the style (*sty*, Fig. 22) is directed downwards on its base, slender and bent like a knee, while above it is straight and gradually thickened, but does not increase at all or only slightly in breadth, ending in a skull-like stigmatic knob (*k*), thick enough to completely stop the entrance of the flower. This knob is provided with a wide open moist stigmatic cavity (*st*,) and is protected from above by two sets of hairs (*pr*, Figs. 21, 22, Sprengel's "Saftdecke") on the two lateral petals, which at the same time defend the entrance of the flower against rain, and prevent insects from entering into the flower in any other way than by the lower side of the skull-like knob. In both forms the five anthers open inwards, are narrowed towards their end, and prolonged above into orange-coloured triangular appendages of their connectives (*c*, Figs. 21, 22), and lie so close together round the style, as to form a hollow cone containing the pollen, and overtopped only by the skull-like crest of the style. This position of the stigmatic knob rising out of the anther-cone but immediately below its summit, is secured by a remarkable contrivance, the skull-like knob being prevented from sliding into the anther-cone by two tufts of hairs, projecting like whiskers from its two cheek-like lateral surfaces. Thus a lifting up of the stigmatic knob, which must always be effected by insects seeking for honey or for pollen, and which is easily accomplished by them in consequence of the base of the style being slender and bent like a knee, will be more likely to tear off the filaments than to push the stigmatic knob into the anther-cone. Indeed, we find that by the swelling of the fertilised ovary the filaments are always torn off, whereas the anthers remain, enclosing like a hollow cone the narrow portion of the style, and the skull-like knob is never drawn between the anthers. If the anther-cone containing the pollen were densely closed all round, the pollen-grains would not fall out unless the anthers were separated from each other by lifting up the stigmatic knob; but there actually exists an opening on the lower side of the summit of the cone directed downwards, the appendages of the two lower anthers being cut out (*op*, Figs. 21, 22), by which nearly all the pollen may fall out spontaneously. When it has fallen out, a great part of the pollen is collected in the close hairy lining of the fore part of the spur.

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* Continued from vol. viii. p. 435.

* My description relates exclusively to those varieties of *Viola tricolor* which grow in the environs of Lippstadt. From Sprengel's, Bennett's, and other descriptions and illustrations, I am aware that in other localities somewhat different varieties are found. But I do not doubt that differences in the manner of fertilisation, identical or closely allied with those here to be described, will be found wherever a large-flowered and a small-flowered form of *Viola tricolor* co-exist.

Thus far the two forms of *Viola tricolor* are identical in structure; and the same, or nearly the same, insects may *a priori* be supposed and have really been observed, to visit the two forms. The distance between the closed entrance of the flower and the honey contained in the uppermost part of its spur being in both of the two forms 6-7 mm., an insect must be provided, in order to reach the honey, with a proboscis of at least that length, unless it be enabled by its small size to crawl with its whole body into the flower. A proboscis of 6-7 mm. length or larger is only to be met with among all our insects in Lepidoptera, Apidæ, and some few Diptera; insects sufficiently minute to be able to crawl into and out of the flowers, are to be found chiefly in the genera Thrips and Meligethes. It may therefore be supposed, *a priori*, that Lepidoptera, Apidæ, and Diptera provided with a proboscis of at least 6 mm. long, and very minute insects of the genera Thrips and Meligethes, will visit the two forms of *Viola tricolor* for honey, and that, besides, some other insects provided with shorter probosces will seek for their pollen. By direct observation this supposition has been thoroughly confirmed, as shown by the following list of visitors actually observed :—

I. As visitors of the large-flowered form, there have been observed :—(*a*) Lepidoptera : (1) *Pieris rapæ* L.[*] (12).[†]—(*b*) Apidæ : (2) *Bombus muscorum* L.[*] (10-15); (3) *B. lapidarius* L. ♀[‡] (12-14); (4) *B. sp.*[§]; (5) *Anthophora pilipes* F.[‖] (19-21); (6) *Andrena albicans* K ♂ (2-2½), in vain seeking for honey.[‡]—(*c*) Diptera : (7) *Rhingia rostrata* L.[§] (11-12); (8) *Syritta pipiens* L. (2-3), eating pollen.[‡]—(*d*) Thysanoptera : (9) Thrips.[¶]

II. As visitors of the small-flowered form, there have been observed :—(*a*) Lepidoptera : (1) *Pieris rapæ* L.;[*] (12), repeatedly; (2) *P. napi* L.[*] (11), repeatedly; (3) *Polyommatus Dorilis* Hfn.[*]—(*b*) Apidæ; (4) *Apis mellifica* L. (6) ☿;[†] (5) *Bombus hortorum* L. ♀[*] (18-21), perseveringly visiting the flowers for honey, although every flower is drawn down by the weight of this large humble-bee; (6) *B. Rajellus* Fll. ♀[*] (10-13), the same individual visiting sometimes *V. tricolor*, sometimes *Lamium purpureum*; (7) *B. muscorum* L. (*agrorum* F.) ♀ (10-14), visiting, without distinction, now the flowers of *V. tricolor*, now the nearly equally large and equally coloured flowers of *Lithospermum arvense*, while omitting the smaller ones of *Capsella busra-pastoris*, *Valerianella olitoria*, and *Myosotis versicolor*; (8) *Osmia rufa* L. ♂[*] (7-9), but once hastily visiting a flower for honey.—(*c*) Diptera; (9) *Rhingia rostrata* L.[*] (11-12), several specimens, repeatedly visiting flowers for honey.—(*d*) Coleoptera; (10) *Meligethes*[*] crawling into the flowers.

Direct observation has thus shown that no essential difference exists between the fertilisers of the large and those of the small-flowered form. But it must appear a striking fact, that not only an equal number of different species, but even one more species has been observed on the small than on the large-flowered form. All the visitors of the small-flowered form, with the exception of only one, having been observed by myself, I must add, as an explanation of this fact, that I have repeatedly watched at the most favourable weather, for several hours, a neglected field, in which, besides some other weeds, there grew an abundance of vigorous specimens of the small-flowered form of *Viola tricolor;* whereas I have never had an opportunity of watching the large-flowered form under favourable conditions. Therefore I have no doubt that, in spite of the incomplete observations hitherto made on this subject, the more conspicuous flowers are in this species also really far more frequently visited by insects than the less conspicuous ones. Otherwise the differences in structure and development of the two forms now to be described would be quite inexplicable. These differences are :—1. In the large-flowered form the stigmatic cavity lies somewhat more towards the top of the skull-like end of the style than in the small-flowered one (as shown by the comparison of Fig. 17 with Fig. 18, and of Fig. 19 with Fig. 20.)

(1) When the skull-like knob in the two forms is pressed against the lower petal, in the large-flowered form the opening of the stigmatic cavity is directed outwards, so that pollen-grains which have fallen out of the anther-cone spontaneously can never fall into the stigmatic cavity unless carried by insects; whereas in the small-flowered form the opening of the stigmatic cavity is directed inwards, so that pollen-grains falling out of the anther-cone spontaneously, fall directly into the stigmatic cavity.

(2) In the large-flowered form the opening of the stigmatic cavity (*st*, Figs. 17, 19, 21) bears on its lower side, as discovered by Hildebrand, a labiate appendage (*l*, Figs. 17, 19, 21) provided with stigmatic papillæ, so that a proboscis inserted into the flower, when charged with pollen of a previously visited flower, rubs off this pollen on to the stigmatic lip (*l*), thus regularly effecting cross-fertilisation; whereas, when withdrawn out of the flower, charged with its pollen, the proboscis presses the lip (*l*) against the stigmatic opening (*st*), thus preventing self-fertilisation. This nice adaptation to those visitors provided with a long proboscis (Lepidoptera, Apidæ, Rhingia) is completely wanting in the small-flowered form (Figs. 18, 20, 22).

(3) In the large-flowered form there is a black wedge-shaped streak (*w*, Figs. 17, 19) on the front side of the style, to which Mr. A. W. Bennett first called atten-

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[*] By W. E. Hart (NATURE, vol viii. p. 121).

[†] The numbers enclosed between parentheses after the names of the insects indicate the length of their probosces in millimetres.

[‡] By myself ("Befruchtung der Blumen durch Insecten," p. 145)

[§] By Ch. Darwin, who writes me, May 30, 1873 :—"Between twenty and thirty years ago I observed, for two or three years, large beds (of *V. tricolor*) in the flower-garden, and saw several times *Rhingia rostrata*, and a nearly black humble-bee visit and fertilise the flowers I say fertilise, because I had watched the flowers for a long time previously, and saw no insect visit them; but two or three days after the above visits a multitude of flowers withered and set capsules."

[‖] By Delpino ("Ulteriori osservazioni," p. 62)

[¶] By Sprengel ("Das entdeckte Geheimniss," p. 397), and Mr. A. W. Bennett (NATURE, vol viii. p 49)

[*] By myself (June 1873)

[†] By Sprengel (*loc. cit.*) and by myself (June 1873), perseveringly visiting the flowers for honey.

tion,* and which he has interpreted as a guide-mark for those visitors, which are diminutive enough to crawl entirely into the flower. This streak is also wanting in the small-flowered form (Figs. 18, 20).

(4) In the large-flowered form pollen-grains do not spontaneously fall out of the anther-cone before the flower has been fully developed for several days; whereas in the small-flowered form, in by far the majority of cases, a great number of pollen-grains fall spontaneously out of the anther-cone into the stigmatic cavity, and there develop long pollen-tubes, even before the opening of the flower, in much rarer cases a short time after it has opened.

(5) When the visits of insects are prevented by a fine net, the flowers of the small-flowered form wither two or three days after opening, everyone setting a vigorous seed-capsule; those of the large-flowered form remain in full freshness more than two or three weeks, at length withering without having set any seed-capsule; when fertilised they wither also after two or three days.

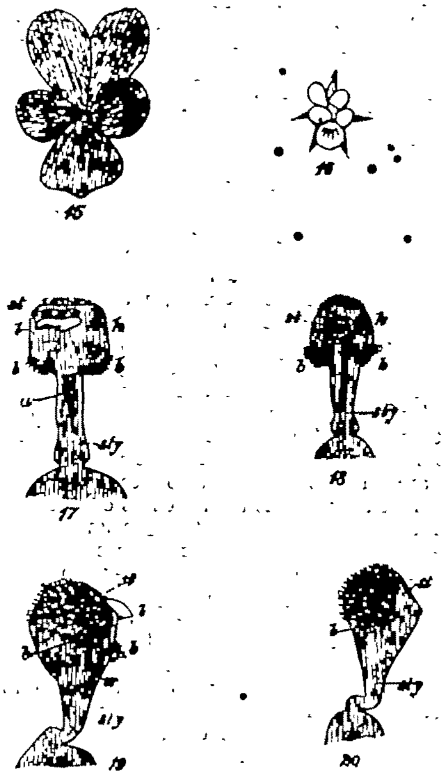

FIG. 15.—Front view of the more conspicuous flower of *Viola tricolor*, natural size. FIG. 16.—Front view of the less conspicuous flower. FIG. 17.—Pistil of Fig. 15, viewed on the under side, 12 times natural size FIG. 18.—Pistil of Fig. 16. FIG 19.—Lateral view of the pistil of Fig 15. FIG. 20.—Lateral view of the pistil of Fig. 16.

The following explanation of the lettering applies to all the figures:—
a, anthers; *a*¹, upper, *a*², lateral, *a*³, lower anther, *ap*¹, appendage of the upper sepal, *b*, beard, *t c* tuft of hairs on the lateral surface of the skull-like crest of the style; *c*, appendage of the connective, *fi*, filaments; *k*, knob of the stigma; *l*, lip, labiated appendage of the stigmatic opening; *n*, nectary, *t a*, honey-secreting appendage of the lower filaments, *op*, opening of the anther-cone; *ov*, ovary; *p*, petals; *p*¹, lower, *p*², lateral, *p*³, upper petal; *pc*, pollen-collecting hairs, *pr*, protective hairs (Sprengel's "Saftdecke"); *s*, sepals; *s*¹, upper sepal (with the appendage *ap*¹); *s*², lateral sepal; *sp*, the uppermost part of the spur, containing the honey; *st*, stigmatic cavity; *str*, streaks converging towards the opening of the flower, *sty*, style; *w*, wedge-shaped streak of the style; *y*, yellow coloured part of the lower petal.

Summary:—The more conspicuous flowers of *Viola tricolor* are adapted to regular cross-fertilisation by Lepidoptera, Apidæ, and Rhingia; whereas self-fertilisation by these visitors is prevented. Pollen-eating flies and diminutive insects crawling into the flower may effect both self- and cross-fertilisation; fertilisation by insects is possible from the opening of the flower for twenty days or more; spontaneous self-fertilisation never takes place. On the contrary the less conspicuous flowers are adapted to regular self-fertilisation; although visited now and then by the same insects as the more conspicuous flowers, cross-fertilisation by these visitors is by no means secured; in most cases it is even prevented by the pollen having previously fallen into the stigmatic cavity; it is possible only in those cases where the flower has opened before its pollen has filled the stigmatic cavity; and even in these rare instances the possibility of cross-fertilisation lasts but a few hours.

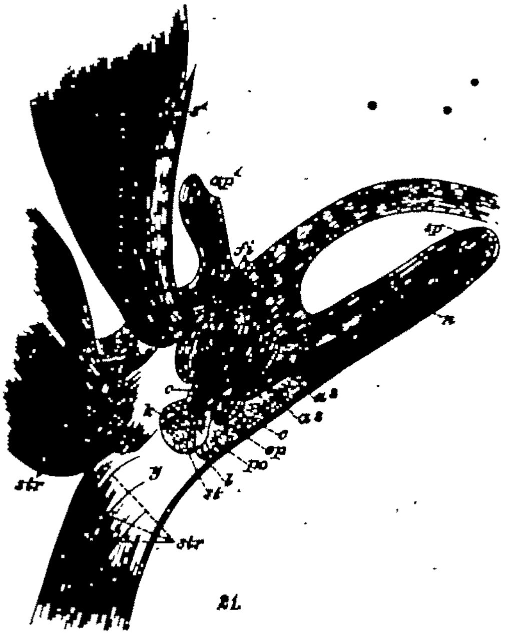

FIG. 21.—Lateral view of Fig. 15 after the half of its sepals and petals having been removed, 7 times natural size.

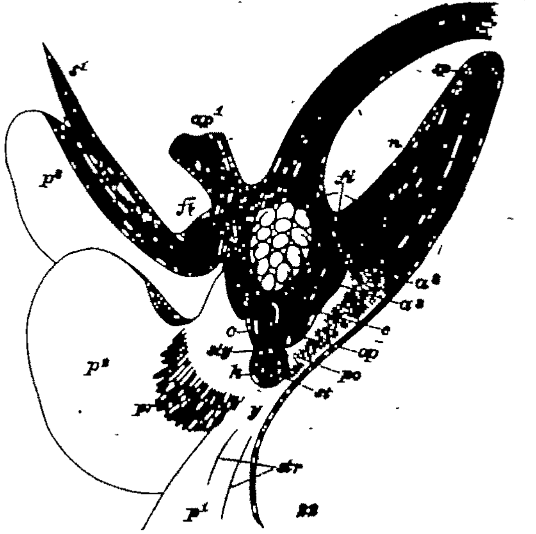

FIG. 22.—Lateral view of Fig. 16, but one lateral anther and the half of one lower anther have been removed and the pistil bisected longitudinally.

Lippstadt, October 1873 HERMANN MÜLLER

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* In his interesting article on the Fertilisation of the Wild Pansy, NATURE, vol. viii. p 49.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE*

VIII.

THE ordinary method of commercial weighing by putting the weights in one scale and the commodity to be weighed in the other, and then observing when a sufficient equilibrium is produced, is inadmissible for scientific weighings, as it is subject to errors arising from defects in the balance itself. To avoid any such errors, and obtain scientific precision in the results, a check is required which is found in a system of double weighing. There are two methods of double weighing for the comparison of two standard weights. One method, known as Borda's, and generally used in France, is that of *substitution*, or weighing first one of the standard weights to be compared, and then the other substituted for it, against a counterpoise placed in the other pan. The difference between the mean resting points of the index needle in these two weighings shows the difference of the two weights in divisions of the scale. The second method, known as Gauss's, but which was first invented by Le Père Amiot, and is now generally used in England and Germany, except for hydrostatic weighings, is that of *alternation*, or first weighing the two standards against each other, and then repeating the weighings, after interchanging the weights in the pans. By this second method no counterpoise weight is required, and *half* the difference between the mean resting points of the index needle shows the difference of the two weights, in divisions of the scale.

In all scientific weighings of standards with balances of precision, it is necessary that the weights to be compared should be so nearly equal that neither pan shall absolutely weigh down the other. The balance must merely oscillate so that the pointer does not exceed the limits of the index scale. In order to obtain an equipoise within this limit, it is requisite to provide small balance weights, most accurately verified, to be added to either pan, as may be found necessary.

The mode of reading adopted by the best authorities in the process of weighing by Gauss's method is as follows:—The comparing standard being in the left-hand pan, and the compared standard in the right-hand pan, and sufficient equipoise being obtained by adding small balance weights, if requisite, the balance is put in action, and the movement of the needle observed through a telescope. The reading at the first turn of the pointer is disregarded. The three next turns are noted, and the reading at the third turn of the pointer, and half the sum of the readings at the second and fourth turns are taken as the highest and lowest readings. Their mean is the resting point of the balance, or the reading of its position of equilibrium. The balance is then stopped, and the weights interchanged, when similar readings are taken and dealt with in the same manner. These two observations constitute one comparison. In cases where great accuracy is required, several successive comparisons are taken, in order to obtain a mean result. Some additional weighings are taken after adding a small balance weight to either pan, in order to ascertain the value of a division of the index scale. And if this balance-weight be added successively to each pan the weighings may be used as additional comparisons.

In using Gauss's method of weighing, it is very desirable to be able to transfer the pans and the weights contained in them from one end of the beam to the other without opening the balance case, and thus to avoid sudden changes of temperature of air within the balance case and consequent production of currents of air. For this purpose, the following plan is adopted. A grooved brass rod is fixed inside the balance case over and a little behind the beam. Upon this rod a brass slider is made to traverse by being attached to a slender brass rod drawn backwards or forwards from the outside of the case. A descending wire with a hook at the end is attached to the slider. For changing the weights, the slider and hook are brought to the right-hand end of the beam, when the pan and weight are lifted from the beam and transferred to the hook by means of a brass rod curved at the end and introduced through a small hole at the side of the balance case. The pan and weight are then slid to the other end of the beam, when the left-hand pan and weight are lifted in a similar manner from the beam and the right-hand pan and weight substituted. It only remains then to transfer the left-hand pan and weight to the right-hand end of the beam.

This method possesses a further advantage. In making a great number of comparisons between two standard weights, they are exposed to some risk of being injured by wear, if they are taken up in the ordinary way with a pair of tongs. This risk is obviated by their being kept in the pans when lifted. Two light pans are used of as nearly as possible equal weight, each of which has a loop of wire forming an arch with the ends attached to the opposite sides of the pan, so that it can be easily lifted with the curved end of a brass rod. The pans are marked X and Y respectively. By interchanging the weights in the pans after a series of comparisons, and making a second series and taking the mean result, it gives the difference between the two weights, unaffected by any possible difference in the weight of the two pans. This contrivance is especially useful, when either of the weights to be compared consists of several separate weights. It was used by Prof. Miller for all his more important weighings during the construction of the imperial standard pound.

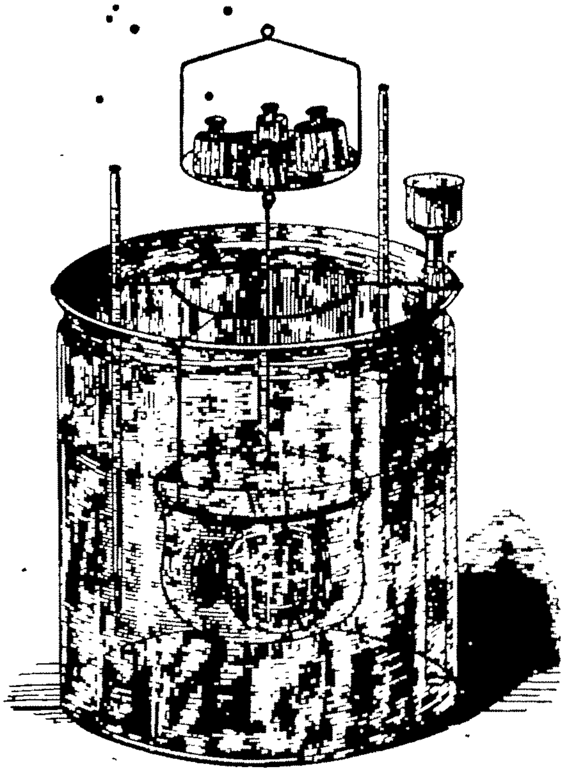

FIG. 18.—Mode of hydrostatic weighing (one-third size).

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* Continued from p. 555.

The advantage possessed by Gauss's method of alternation over Borda's method of substitution has been proved by Prof. Miller as follows :—

Let P and Q be two standard weights of the same denomination to be compared, and C the counterpoise of each.

For Borda's method, let the readings of the index be denoted by (C, P), when C is in the left pan and P in the right pan, and by (C, Q), when C is in the left pan, and Q in the right pan.

For Gauss's method, let (Q, P) denote the readings when Q is in the left pan and P in the right, and (P, Q), when P is in the left pan and Q in the right pan.

Let e be the probable difference between the recorded and the true position of equilibrium, that is to say, the probable error of a *single weighing* (not of a comparison, which requires two weighings).

Then by Borda's method, (C, P) has a probable error e, and (C, Q) has a probable error e; and the two weighings give the value of P – Q with a probable error of $\sqrt{(e^2 + e^2)} = e\sqrt{2}$.

By Gauss's method, (Q, P) has a probable error e, and (P, Q) has a probable error e; and the two weighings give the value of P – Q with a probable error of $\frac{e}{2}\sqrt{2}$.

Thus the probable error of the result of two weighings by Borda's method is twice as great as by Gauss's method.

To obtain a value of P – Q by Borda's method with a probable error of $\frac{e}{2}\sqrt{2}$, we must make four comparisons of two weighings each. Therefore one comparison by the method of Gauss gives as good a result as four comparisons by Borda's method.

The result of this weighing of two standard weights against each other gives only their apparent difference when weighed in air. In order to ascertain their true difference, it becomes necessary to determine the weight of air displaced by each, from the data which have been already mentioned, and to allow for any difference of weight of air displaced, according to the following formula :—

If the weights P and Q appear to be equal in air, the weight of P – weight of air displaced by P is equal to the weight of Q – weight of air displaced by Q.

In determining the weight of ordinary atmospheric air in rooms where standard weights are compared, and containing a certain quantity of aqueous vapour and carbonic acid, the practice has been to take, as the unit of weight of air, a litre of dry atmospheric air free from carbonic acid, = 1·2932227 gramme, at 0° C., as determined by Ritter from the observations of M. Regnault in Paris, lat. 48° 50′ 14″, and 60 metres above the level of the sea, under the barometric pressure of 760 millimetres of mercury. Assuming that atmospheric air contains, on an average, carbonic acid equal to 0·0004 of its volume, and the density of carbonic acid gas being 1·529 of that of atmospheric air, the weight of a litre of dry atmospheric air containing its average amount of carbonic acid, under the stated circumstances, is 1·2934963 gramme.

Allowance should be made for the difference of the force of gravity in latitudes other than Paris, as well as for the difference of height of the place of observation above the mean level of the sea. Although the absolute weight varies with the latitude and with the height above or below the mean level of the sea, yet this variation is not felt in the comparison of standard weights in a vacuum, because the weights are equally affected on both sides of the beam. But in all weighings of standards in air requiring special accuracy, such variation must be taken into account in computing the weight of air displaced by each standard weight.

Mr. Baily has shown from his pendulum experiments * that if we take G to denote the force of gravity at the mean level of the sea in lat. 45°, the force of gravity in lat. λ, at the mean level of the sea

$$= G\,(1 - 0{\cdot}0025659 \cos 2\lambda).$$

And Poisson † has proved that the force of gravity in a given latitude at a place on the surface of the earth at the height s above the mean level of the sea—

$$= \left\{1 - \left(2 - \frac{3\rho'}{2\rho}\right)\frac{s}{r}\right\} \times \text{(force of gravity at the mean level of the sea in the same lat.)}$$

where r is the radius of the earth, ρ its mean density, and ρ' the density of that part of the earth which is above the mean level of the sea. If as is probable,—

$$\rho' : \rho = 5 : 11;\ 2 - \frac{3\rho'}{2\rho} = 1{\cdot}32 \text{ nearly};\ r = 6366198 \text{ metres},$$

it follows that the weight in grammes of a litre of dry atmospheric air containing the average amount of carbonic acid, at 0°, and under the pressure of 760 millimetres of mercury at 0°, at the height s above the mean level of the sea in lat. λ is—

$$1{\cdot}2930693\left(1 - 1{\cdot}32\,\frac{s}{r}\right)\cdot(1 - 0{\cdot}0025659 \cos 2\lambda).$$

At Cambridge, where Prof. Miller's observations for determining the weight of the new standard pound were made, in lat. 52°.12′ 18″, about 8 metres above the mean level of the sea (and for which place his tables were computed,) the weight of a litre of dry air containing the average quantity of carbonic acid was found by him to be 1·293893 gramme. This weight of air is therefore a little greater than at Paris. From similar data, after taking a further correction by Lasch of the weight of a litre of dry air at Paris = 1·293204 gramme, the weight of a litre of dry air at Berlin (lat. 52° 30′, and 40 metres above mean sea level) has been computed to be 1·29388 gramme.

The co-efficient of expansion of air under constant pressure between 0° and 50° C. is taken from Regnault's determination to be 0·003656 for 1° C., in other words between 0° and 50° C., the ratio of the density of air at 0° to its density at $t°$ is $1 + 0{\cdot}003656\,t$.

With regard to the barometric pressure of the air and the allowance to be made for the pressure of vapour present in it, the density of the vapour of water is determined to be 0·622 of that of air ; that is to say, the ratio of the density of the vapour of water to that of air is 1 – 0·378.

Hence, if t be the temperature of the air, b the barometric pressure, v the pressure of the vapour present in the air, b and v being expressed in millimetres of mercury at 0° C., the weight of a litre of air at Cambridge becomes

$$\frac{1{\cdot}293893}{1 + 0{\cdot}003656\,t}\ \frac{b - 0{\cdot}378\,v}{760}.$$

The ratio of the density of air to the maximum density of water is found by dividing the above expression by 1,000, as a litre of water is the volume of 1,000 grammes of water at its maximum density. Prof. Miller's Table I. gives the logarithms of this ratio at the normal barometric pressure of 760 millimetres, at the several degrees of temperature from 0° to 30°. These logarithms require to be diminished only by 0·000026 for weighings at the Standards Office, Westminster, lat. 51° 30′, and about 5 metres above the mean sea-level ; and when diminished by 0·000132, they may be used for the reductions of weighings at Paris.

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* "Memoirs of the Astronomical Society," vol. vii. p. 94

† "Mémoires de l'Institut," tome xxi. pp. 91, 236.

The values of the pressure of vapour at the same temperatures in millimetres of mercury at 0°, according to Regnault's observations, are stated by Prof. Miller in a separate Table II. These values are given on the assumption that the pressure of vapour in rooms that are not heated artificially is two-thirds of the maximum pressure of vapour due to the temperature, as shown by the results of experiments on the authority of Biot, Regnault, and Bianchi.

The actual mode of ascertaining the weight of air displaced by two standard weights may now be described.

For determining the temperature of the air and of the two standard weights during the weighings, two standard thermometers are placed in the balance case, and their readings noted at the beginning and end of the weighings. The weight of air displaced by each of two standard weights is to be ascertained by the following formula:

Log. weight in grains of air displaced by P = log. h + log. At + log. (1 + ePt) + log. weight of P in grains − log. △P.

Here t denotes the temperature of the air by the Centigrade thermometer;

b the barometric pressure of the air in millimetres of mercury at 0° C.;

v the maximum pressure of aqueous vapour contained in the air, also in millimetres of mercury;

$h = b - 0{\cdot}378 \times \frac{2}{3} v$;

At the ratio of density of air at t° to the maximum density of water;

ePt the allowance for expansion in volume of P, or the ratio of its density at 0° to its density at t;

△P the ratio of density of P at 0° to the maximum density of water.

By this formula, the required result is to be obtained. The logarithms of the three first terms may be found in Prof. Miller's tables, pp. 785-791 of his account of the construction of the new standard pound, Phil. Trans., part iii. of 1856.

Reference has already been made to the mode of ascertaining the volume or density of a standard weight by determining the difference of its weight in air and in water. The following practice for all such hydrostatic weighings was adopted by Prof. Miller when determining the densities of all the standard weights constructed under the sanction of the Commission for restoring the Imperial Standards, and is also followed in the Standards Department. In this process it is requisite to employ pure distilled water, and with this object the water used in the Standards Department is twice distilled in a still of the best construction, erected in the office, and the best chemical tests are employed for ascertaining that the water is free from any foreign substances.

The vessel for containing the distilled water is a glass jar, rather more than 6 inches in internal height and diameter. A stout copper wire is stretched across the mouth of the jar (see Fig. 18) in such a manner as to leave a circular space in the middle, large enough to admit the passage of the standard weight P, the density of which is to be ascertained. This copper wire supports two thermometers, adjustable as to their height, for determining the temperature of the water at the mean height of B during the weighings. It also serves to sustain a glass tube, open at both ends, and placed close to the side of the jar. A small glass funnel is inserted in the upper part of the tube, and in the lower part are one or two pieces of clean sponge.

The standard weight P is suspended from a hook under the right pan of the balance, specially constructed for hydrostatic weighings. A fine copper wire, the weight of which per inch is known, is attached to the hook by a loop, and has another loop at the other end. To this lower loop is attached a stout wire, bent and terminating in a double hook, which fits round P, and holds it securely. The counterpoise of P is next placed in the left pan of the balance. The glass jar is placed under the right pan of the balance, P being suspended in it, and the water is gently poured into the funnel and the jar filled to the requisite height above P. The bubbles of air are arrested by the pieces of sponge, and, ascending up the glass tube, are thus prevented from entering the jar. It is of importance to ascertain that no bubble of air is attached to P, and if so, it may generally be removed by the feather of a quill. But it sometimes happens that the weight P has an irregular surface, and air attaching to it cannot be thus dislodged. In such cases a small bell-shaped glass jar just large enough to hold P and its supporting wire, is used. This vessel is filled with water sufficient to cover P, and is suspended over the flame of a spirit lamp by a stout wire, bent at its lower end into a ring, into which the jar descends to its rim, and the water is allowed to boil until it is seen that the air has been entirely expelled. When cooled, the small jar containing P is immersed in the water, which nearly fills the large jar, and the small jar, with its wire, is then disengaged and lowered till P hangs clear of it, when it is removed. The transfer of P from the small to the large jar is thus effected without taking it out of the water.

For the actual weighing of P in water, after it has been counterpoised in air, weights equal to the difference of weight of P in water and in air, are placed in the right pan till equilibrium is produced, when the readings of the scale are observed. P is next removed, leaving its hook suspended in the water, and a volume of water equal to the volume of P is added to the water in the jar, so as to leave the same quantity of wire immersed as before. The requisite weights are then added to the right pan, until the equilibrium, which has been disturbed by the removal of P, is again produced, when the reading of the scale is observed and noted. This gives the actual weight in water of P.

The thermometers in the water are so placed as to give the temperature of the water at the centre of gravity of P. Another thermometer is placed in the balance case to give the temperature of the air during the weighings. The reading of the barometer is also noted.

Having determined the weight of P in air of ascertained density, its volume and density are calculated according to the following formula, the unit of volume being the volume of a grain weight of water at its maximum density:—

Let P in water at t° appear to weigh as much as Q in air. Then the weight of water at t° displaced by P = weight of P − weight of Q + weight of air displaced by Q.

Log. volume of P = weight in grains of the water displaced by P + log. W_t − log. (1 + eP_t); where W_t is the ratio of the maximum density of water to its density at t, and ePt is the expansion in volume of P at t. (The logarithms of these values are given in tables.)

Log. density of P = log. weight of P in grains − log. volume of P.

The actual weight of air displaced is to be ascertained by the method already stated.

As the true weight of P in air cannot be ascertained until its volume or density is known, an approximate value of the volume of P may be found by assuming the weight of P to be equal to its apparent weight in air; and this value of the volume of P may be used in reducing the weight of P, and thus a more accurate value of the volume of P obtained, by means of which a closer approximation to the values of the absolute weight of P, and of its density may be found. This process should be repeated when greater exactness is required.

H. W. CHISHOLM

(*To be continued.*)

EARTH-SCULPTURE*

AMONG the questions which may be treated as matters of strict science, and which yet cannot be wholly divested of the strong human, one might almost say personal, interest which belongs to them, is the birth of mountains and valleys. The familiar outlines of his dwelling-place have fixed the attention of man from the infancy of the race up to the present day. Long before science arose to deal with them they had become inwoven with his history, his habits, and his creed. The great mountains had been to him emblems of majesty and eternity, lifting up their fronts to heaven as they had done from the beginning, and would no doubt do to the end. They rose before him as monuments of the power of that great Being who had heaved them out of chaos. It was enough for him in that early time to feel their mighty influences; he had then no questions or doubts as to how or when they first appeared upon the earth.

Happily, in spite of questioning, exacting Science, these first natural and instinctive feelings are not yet dead within us. A knowledge even of all the laws of mountain-making cannot, if our minds are healthy and our hearts beat true, deprive us wholly of that first genuine child-like awe and wonder in presence of noble mountains,—crag and cliff sweeping in rugged and colossal massiveness above dark waves of pine, far into the keen and clear blue air;—the vast mantle of snow, so cloud-like in its brightness, yet thrown in many a solid fold over crest and shoulder; the dark spires and splintered peaks; half snow, half stone, rising into the sky, like very pillars of heaven; and then the verdure of the valleys below, the dash of waterfalls, the plenteous gush of springs, the laugh and dance of brook and river as they one and all hurry down to the plains—who can see these things for the first time, nay, for the hundredth time, without at least some sparkle of the simple child-like emotion of the olden time, or without appreciating, even if he cannot fully share, the feeling of the poet to whom they bring "dim eyes suffused with tears"?

These great dominant features of the land must indeed ever rivet our imagination, and yet when the questioning spirit of modern science asks to know how they came into being, we are no longer permitted to content ourselves with the early belief that they were but parts of the primæval outlines of the earth. The progress of inquiry and knowledge has destroyed that belief. We find, too, that both labour and patience are needed ere we can understand what has been put in its place. But the task of learning this is well repaid. However grandly the mountains rose when they were gazed at only in awe and wonder, they gain an added sublimity when the eyes which look upon them can trace some of the steps whereby their grim magnificence has been achieved.

We naturally associate the more lofty and rugged parts of the land with the operations of former earthquakes and convulsions by which the solid earth has been broken and ridged into these picturesque forms. This obvious inference was early adopted in geology, and though in many cases a mere belief rather than a legitimate deduction from observation, and springing from a conviction of what ought to be, rather than what has been proved to be the case, it has sturdily maintained its hold alike on the popular mind, and also to a very considerable extent in the orthodox geological creed.

Towards the end of last century, however, Hutton and Playfair, names never to be mentioned in Edinburgh without gratitude and pride, proclaimed views of a very different character. They maintained that the rocks of the land, originally accumulated under the sea, have been upheaved by underground movements, and without pretending to know in what external forms these rocks first appeared above the sea, they contended that the present contours of the land had arisen mainly from a process of sculpture,—the valleys having been carved out by rains, streams, and other superficial agents, while the hills were left standing up as ridges between. So satisfied were these bold and clear-sighted men that their idea was essentially true, that they gave themselves no concern in gathering detailed proofs in its support. They were content with general appeals to the face of nature everywhere as their best and irrefragable witness. But, as events proved, they were in advance of their time. The views which they promulgated on this subject were first opposed, then laid aside and forgotten. In the subsequent literature of the science for fully half a century they almost wholly disappear. An occasional reference to them may be met with, where, however, they are cited only to be dismissed, as if the writer seemed hardly able to restrain some expression of his wonder that men could ever have been found so Quixotic as to vent such notions, or that others could have been so gullible as to believe them.

Apart altogether from the truth or error of the Huttonian teaching regarding the origin of the earth's superficial features, no one who has the progress of geology at heart can regard without regret this almost contemptuous dismissal of the question from the range of scientific inquiry. For together with that teaching went all interest in, and even all intelligent appreciation of, the problem which Hutton had set himself to solve. Men turned back to vague notions about cataclysms, earthquakes, subterranean convulsions and fractures, of which they spoke, and sometimes still speak, with a boldness in inverse proportion to their knowledge of the actual conditions of the problem. They studied with praiseworthy assiduity and success the working of the various natural agents whereby the surface of the land is affected, but it was with the view rather of showing how the materials of new continents are gathered together, than of learning how the outlines of existing continents have been produced. The study of the origin of mountain and valley went out of fashion, and from the time of Playfair's Illustrations, published at the beginning of this century, received in this country but scant and haphazard attention until in recent years the subject has gradually revived, and has become one of the most prominent and interesting subjects of geological research.

It is not my purpose to give any historical sketch of the progress of inquiry on this question, although I ought not even to refer to it without an allusion to the names of Scrope, Ramsay, Jukes, Ruskin, Dana, Topley, Whitaker, Greenwood, the Duke of Argyll, Mackintosh, and others, who, though often differing widely in their views, have done so much to renew an interest in what will probably always prove one of the most alluring aspects of geology. Thoroughly convinced of the essential truth on which the Huttonian doctrines were based I wish, on the present occasion, first to define and illustrate some of the leading features of these doctrines as I hold them myself, and as I believe them to be held by the great body of active field geologists in Britain, and secondly, to review certain objections which have recently been reiterated against them.

At the outset it is necessary to ascertain what relation the internal arrangements of the rocks bear to the external forms of the land, in other words, the influence of what is called Geological Structure. It is obvious, as Hutton showed, that since the rocks have been formed as a whole under the sea, they must have been raised out of that original position into land, so that the first point we settle beyond dispute is that the mass of the land owes its existence to upheaval from below. But though we fix securely enough this starting point in our inquiry, it by no means follows that we thereby settle what was the original outline of the land so upheaved. The non-recognition of this fact has involved not a few of the writers on this subject in great confusion and error.

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* The Opening Address for the Session 1873-4 to the Edinburgh Geological Society, delivered Thursday, Nov. 6, by the President, Prof. Geikie, F.R.S.

Among the geologists of the present day there is a growing conviction that upheaval and subsidence are concomitant phenomena, and that viewed broadly they both arise from the effects of the secular cooling and consequent contraction of the mass of the earth. The contraction has not been uniform, as if the globe had been a cooling ball of solid iron. On the contrary, owing to very great differences in the nature and condition of the various parts of our planet and perhaps to features of the interior with which we are yet but imperfectly acquainted, some portions have sunk much more than others. These, having to accommodate themselves into smaller dimensions would undergo vast compression and exert an enormous pressure on the more stable tracts which bounded them. It could not but happen that after long intervals of strain, some portions of the squeezed crust would at length find relief from this pressure by rising to a greater or less height, according to their extent and the amount of force from which they sought to escape. These upraised areas would no doubt tend to occur in bands or lines across the direction of the pressure, much as the folds we produce in the sheets of an unbound book are more or less nearly parallel with the two sides from which we squeeze the paper. They would sometimes be broad folds—huge wide swellings of the earth's surface. At other times they might be long, lofty, and comparatively sharp ridges. In the one case they would give rise to high plateaux or table-lands, in the other they would be recognised as mountain-chains.

This is a rough-and-ready statement of what seems the probable explanation of the origin of the elevated tracts upon the earth's surface. It is evident that the pressure would be vastly greater a few hundreds or thousands of feet underground than at the surface, and hence that though the rocks deep down might be squeezed and crumpled, as we could crumple brown paper, yet that at the surface they might show little or no contortion. Certainly without further proof we could never affirm that a contorted mass of rock which now forms the surface of the ground rose as part of the surface during the time of upheaval and contortion. Intensely crumpled rocks would rather suggest a deeper position, with the subsequent removal of the rocks under which they originally lay.

As the earth has been cooling and contracting ever since it had a separate existence as a planet, its surface must have been exposed to a long series of such shrinkage movements as those we are considering. Apart, therefore, from local evidence, we should expect that ridges and depressions must have been impressed upon that surface in a long succession from the earliest periods downwards, and hence that the present mountain-chains and basins of the earth must be of many different ages. We cannot tell what the first mountains were made of, nor where they lay, although some of the existing ridges of the earth's surface are undoubtedly, even in a geological sense, very old. In not a few cases the same mountain-chain can be shown from its internal structure to be of many successive dates, as if it lay along a line of weakness which had served again and again as a line of relief from the severe earth-pressure.

These questions have been treated with much ability by Constant Prevost, Dana, Mallet, and others, to whose writings I refer for details. In stating them in this general way my object is to show that those geologists who, like myself, believe in the truth of the Huttonian doctrines of denudation, are most unfairly represented when they are said to ignore the influence of subterranean forces upon the exterior of the earth. None can recognise more clearly than they do how entirely have the great surface outlines of the globe been dependent upon the action of these forces, that is, upon the results which flow from the contraction of the planet and from the reaction of the heated interior upon the surface.

But a block of marble is not a statue, nor would a part of the earth's crust heaved up into land form at once such a surface of ridge, and valley, and nicely adjusted water system as any country of which we know anything on the face of the globe. In each case it is a process of sculpture, and the result varies not only with the tools but with the materials on which they are used. You would not expect the same kind of carving upon granite as upon marble. And so, too, in the great process of earth-sculpture, each chief class of rock has its own characteristic style. The tools by which this great work has been done are of the simplest and most everyday order—the air, rain, frost, springs, brooks, rivers, glaciers, icebergs, and the sea. These tools have been at work from the earliest times of which any geological record has been preserved. Indeed, it is out of the accumulated chips and dust which they have made, afterwards hardened into solid rock and upheaved, that the very framework of our continents has been formed. The thickness of these consolidated materials is to be measured, not by feet merely, but by miles. If the removed materials are so thick, they show what a vast mass of rock must have been carved away. And even before knowing anything of the way in which the various tools are used, we should be justified in holding it to be, at the least, extremely improbable that any land surface would long retain its original contour or even any trace of it.

But when we come to watch with attention how the tools really do their work, this improbability increases enormously. Adopting a method of inquiry suggested by Mr. Croll, I have elsewhere shown that even at their present state of progress the amount of geological change which they would accomplish in a comparatively small number of ages is almost incredible. On a moderate computation they would reduce the general mass of the British Islands down to the level of the sea in five or six millions of years, and might carve out valleys a thousand feet deep in a fourth part of that time. It is evident that though the upheaval of some parts of the continents may go back into the remotest geological antiquity, the forms of the present surface must be, comparatively speaking, modern.

There is reason to believe that many, if not most, of the great mountain chains of the globe are, in a geological sense, of recent origin. The Alps, for example, though they may have undergone many earlier movements, were ridged up into their existing mass long after the soft clays were laid down which cover so large an area of the low lands in the south of England, and on which London is built. It would require far more detailed work than has ever been bestowed upon these mountains to enable us even to approximate to what was the original form of the surface just after the upheaval, and before the array of sculpture-tools began their busy and ceaseless task upon these great masses of rock. We may believe that a series of huge parallel folds of curved and broken rock rose for thousands of feet into the air, that when, after the earth-throes had ceased, rain and snow and frost first laid their fingers on the new-born summits, these agents of destruction would have a most uneven surface to work upon, and would necessarily be guided by it in their working; and hence that some, at least, of the dominant earliest ridges and hollows would be perpetuated. Such a belief would carry probability in its favour, but it would certainly not amount to a proof of the supposed perpetuation. That would require to be corroborated by the internal and external evidence of the mountains themselves. In some tracts, as, for instance, among the singularly symmetrical ridges and furrows of the Jura, it would not be difficult to restore the original outline, and to fix exactly how far the subterranean movements had determined the present external forms of the ground, though even there, where this connection is so clear, we should see at the same time how greatly the tops and sides of the long saddle-shaped arches of rock have suffered from subsequent waste. But among the contorted, inverted, and broken rocks of the Central Alps the task would be infinitely more difficult.

We could not advance far, however, in such a quest before observing that one feature stands out conspicuously enough among the mountains, viz., that whatever might have been their original outlines, these were most certainly not the same as those which we see to-day. No part of the history of the ground can be made more self-evident than that, since the birth of these mountains, millions upon millions of cubic yards of rock have been worn off their crests and ridges, and carved out of their sides. There is not a cliff, crag, or valley along the whole chain of the Alps which does not bear witness to this great truth.

If then, even when dealing with the young Alps, we cannot be quite sure what were their first or infant features, how impossible must it be to decide as to the early outlines of such immensely more ancient uplands as those which date from palæozoic times! For, evidently, the higher their antiquity, and the longer, therefore, their exposure to ceaseless waste, the more must these outlines be changed. The general mass of land might still remain land, but trenched and furrowed and worn down, as the Alps are now suffering, until not a single vestige or indication of its first contour survived, the remaining portions being, as it were, merely the stump or base of what once was.

Now this is the position in which the question presents itself in Britain. The hills of the Highlands and Southern Uplands of Scotland, of the Lake district, and of Wales, are not mountains in the same sense as the Alps or Pyrenees, or other great continental mountain-chains. However much these long lines of elevated ground may have had their outlines modified by the universal waste of the earth's surface, their linear character, the general parallelism of their component ridges, the undulations of the strata along their flanks, as well as their internal geological structure, bear witness to the fact that they are but huge wrinkles upon the shrivelled globe—tracts which have been thrust up while the neighbouring regions have sunk down. But in Britain these characteristic features are wanting. In all probability there never was any true mountain-chain in our region. There is good reason to believe that in very ancient times, that is to say, previous to the Old Red sandstone, a wide plateau-like mass of land was upraised on the north coast of Europe, surviving portions of it being represented by the detached hilly regions of Britain and the great table-land of Scandinavia. The rocks underlying this upheaved tract underwent, at the time of elevation, enormous compression and consequent contortion. This could not happen without an infinite amount of resistance. The heat thus evolved among the grinding masses may have been amply sufficient even to melt them in part. And no doubt it was during this process that they became crystalline over such wide areas, and were injected with granite and other melted products. But all this had been wholly, or almost wholly, completed before the time of the Old Red sandstone, for the deposits of that geological system are formed out of the older altered rocks, and lie undisturbed upon them. Even now, in spite of all the subsequent denudation, the patches of old red conglomerate which remain show to what an extent the older rocks had been buried under it, for, they are found rising here and there to a height of 2,000 or 3,000 ft. above the sea. But they prove further, not only that the contortion of the underlying rocks preceded the Old Red sandstone, but that these rocks had suffered a vast extent of waste at the surface, before even the oldest visible parts of the conglomerate were deposited upon them. This waste has been in progress ever since. We need not, therefore, hope to discover any vestige of the aboriginal surface. A geological section drawn across any part of the hills proves beyond question that the general surface of the country has had hundreds or even thousands of feet of solid rock worn away from it. Such a section shows moreover that our present valleys are not mere folds due to underground movements, but are really trenches out of which the solid rock has been carried away.

So far, this is a question of simple fact, and not merely of opinion. The language of Hutton may be literally true of Britain:—"The mountains have been formed by the hollowing out of the valleys, and the valleys have been hollowed out by the attrition of hard materials coming from the mountains." Our British hills, unlike the chains of the Jura and the Alps, are simply irregular ridges depending for their shape and trend upon the directions taken by the separating valleys. The varying textures of the rocks, their arrangements with relation to each other, their foldings and fractures, and the other phenomena comprised under what is termed "geological structure," have greatly modified this result, but the process has nevertheless, as I believe, been one of superficial sculpturing, and not of subterranean commotion and upheaval. On the details of this process it is not needful to dwell.

From these cursory statements, which express, I believe, the general concurrent opinions of the modern Huttonian school, it should be clear how far that school must be from ignoring the influence of subterranean forces. Hutton himself never did so, and his followers now know far more of these forces than he did. But on the other hand, they claim for the surface-agents in geology a potency great enough to cut down table-lands into mountain-ridges and glens, to carve out the surface of the land into systems of valleys, and in the end to waste a continent down to the level of the sea.

(*To be continued.*)

ASTRONOMY AT OXFORD

DR. DE LA RUE having, in the course of last summer, made a munificent offer of several astronomical instruments and apparatus, including a large reflecting telescope, to the University, the subject was brought under the consideration of the delegates of the Museum, who, at their first meeting in this term, appointed a committee to "report on the desirability of accepting the munificent offer of Dr. De La Rue to present to the University his celebrated reflecting telescope, on the probable cost of a building to receive the instrument, and on the precise purposes for which this instrument may be usefully employed, in distinction to the refracting telescope now being set up."

The committee, after full and careful examination of the whole subject, have sent in a report, to which they have unanimously agreed, and which the delegates recommended, with entire confidence, to the favourable consideration of the council. In consequence of this report, the following forms of decree will be submitted to a convocation to be held on Thursday, Nov. 27:—

1. That the reflecting telescope and other apparatus offered to the University by Dr. De La Rue be accepted; and that the Vice-Chancellor be requested to return the thanks of the University to Dr. De La Rue for his munificent gift. And that the curators of the University chest be authorised to pay to the delegates of the University Museum a sum not exceeding 1,500*l.*, to be expended by them on the erection of buildings in the park suitable for the reception and use of the telescope and other apparatus presented by Dr. De La Rue, as also of the instruments at present in the small observatory on the east side of the museum, according to plans and specifications prepared by Mr. Charles Barry, architect, and adjoining the observatory now nearly completed.

2. That the curators of the University chest be authorised to pay annually to the Savilian Professor of Astronomy during five years, or until provision is made from some other source, the sum of 200*l.* for providing an assistant and defraying the expenses incurred in the maintenance and use of the instruments in the observatory, an account of the expenditure of such sum to be annually submitted to the auditors of accounts.

We cannot doubt that Convocation will sanction a decree which promises to make Oxford first in the field in this country in the power of aiding the new astronomy which is dawning upon us—thanks to the spectroscope and the application of photography.

Such a position may not be thought much of now, but in the coming time Oxford men will refer to it as one of the things of which Oxford has the greatest reason to be proud.

NOTES

THE Copley Medal and the two Royal Medals in the gift of the Royal Society, have this year been awarded as follows:—The Copley Medal to Prof. Helmholtz, the distinguished physiologist, physicist and mathematician, of Berlin; a Royal Medal to H. E. Roscoe, F.R.S., Professor of Chemistry in Owens College, Manchester; and a Royal Medal to Dr. Allman, Professor of Biology in the University of Edinburgh.

THE Annual Meeting of the Royal Society will be held on December 1, when, after dining together, the Fellows will adjourn to their new apartments.

A DEPUTATION from the Council of the Society of Arts had an interview on Friday last with the Royal Commissioners of Scientific Instruction with reference to museums and galleries of science and art. The deputation consisted of Major-General F. Eardley-Wilmot, R.A., F.R.S. (Chairman of the Council), Mr. E. Chadwick, C.B., Colonel Croll, Mr. Hyde Clarke, the Rev. Septimus Hansard, Admiral Ommanney, C.B., F.R.S., Colonel Strange, F.R.S., Mr. Seymour Tewlon, with Mr. Le Neve Foster, Secretary. The Chairman of the Council stated that the object the Council had in view was to bring before, and ask the support of, the Commissioners to the action the society was now taking in reference to museums, and pointed out that this had special regard to the State giving increasing aid to existing museums, to aid in the multiplication of such museums, and rendering them available for educational purposes. He further pointed out the necessity for all such museums being placed under the control of a Cabinet Minister responsible to Parliament. He handed to the Commissioners a copy of resolutions embodying the views of the Council, stating at the same time that a large and influential committee was in the course of formation, and that a considerable number of members of both Houses of Parliament had already given in their names.

THE first award of the Grand Walker prize of 1,000 dols. was voted by the Council of the Boston Society of Natural History on October 1, to Alexander Agassiz, of Cambridge, U.S.A., for investigations on the embryology, structure, and geographical distribution of the Radiata, and especially of the Echinoderms, and the publication of the results as embodied in his recent work. The Annual Walker Prize of 60 dols. for 1873 was at the same meeting awarded to A. S. Packard for his essay on the development of the common house-fly. For the Annual Prize of 1874, the subject is "The Comparative Structure of the Limbs of Birds and Reptiles." Memoirs offered for competition must be forwarded on or before April 1, addressed to the Boston Society of Natural History, for the Committee of the Walker prizes, Boston, Mass., U.S.A., and each memoir must be accompanied by a sealed envelope enclosing the author's name, and superscribed by a motto corresponding to one borne on the M.S.

IN the examination for Foundation Scholarships at Trinity College, Cambridge, to be held at Easter, 1874, one or more Scholarships will be obtainable by proficiency in the Natural Sciences. The Examination in Natural Science will commence on Friday, April 10, and will include the subjects set forth in the regulations for the Natural Sciences Tripos. It will be open to all undergraduates of Cambridge or Oxford, and to persons not members of the Universities, provided that these last are under twenty years of age. Candidates who are not members of Trinity College must send their names to the Master, together with a certificate of age and good character, on or before Saturday, March 21.

WE congratulate the University of Edinburgh on being the first in the United Kingdom to recognise the duty of universities so to frame their regulations for degrees in science as to encourage original work in opposition to mere book-knowledge. The University of Edinburgh has just issued a regulation that every candidate for the degree of Doctor of Science shall in future be required to submit a Thesis containing some original research on the subject of his intended examination, and that such thesis shall be approved before the candidate is allowed to proceed to examination.

PROF. CHEVALLIER, for many years Professor of Mathematics and Astronomy in the University of Durham, died on the 4th inst., at the age of 80 years.

WE learn from *Ocean Highways* that Prof. Mohn, of the Meteorological Institute at Christiania, and Mr. O. Sars are preparing a plan for the investigation of the sea between Norway, the Faro Islands, Iceland, and Spitzbergen, the expense of which will, it is expected, be defrayed by a grant of the Norwegian Storthing.

DR. RUDOLPHE WOLF has recently published in the *Vierteljahrschrift* of the Zurich Society of Natural Science, the thirty-third number of his *Astronomische Mittheilungen*. The paper is important in reference to sun-spots chiefly, and as bringing out with great clearness the connection of these with variations in declination of the magnetic needle. The author gives a series of daily observations of sun-spots, during 1872, made at Zurich, Peckeloh, Munster, Palermo, and Athens. The mean relative number obtained is 101·7; and for the years 1866–72 inclusive, the series runs thus:—16·3, 7·3 (min. 1867), 37·3, 73·9, 139·1 (max. 1870), 111·2, 101·7. Dr. Wolf has constructed a formula by which the average yearly variations of magnetic declination, in a particular place, may be calculated from the relative sun-spot number (two constants for the place being given). In this way, for example, he obtains for Munich the quantity 10′·80 as representing the magnetic variation for 1872; the number got from observation is 10′·75, showing a close agreement. In the second portion of his paper Dr. Wolf discusses several points connected with the history of the telescope, the vernier, the pendulum clock, &c.; among other things, attributing to Burgi (who lived in the early part of the sixteenth century), a share in the discovery of the isochronism of the pendulum. The last portion of the paper reproduces some of the earlier sun-spot literature. The same number of *Astronomische Nachrichten* contains a note by M. von Asten, furnishing evidence against the supposed identity of a cometary object observed by Goldschmidt on May 16, 1855, with Tempel's comet. (1867, II.)

THE recent meeting of the American Association for the Advancement of Science held at Portland, Maine, was considered on the whole a successful one. 157 papers were entered, and abstracts were received of all but nine; most of the remainder were passed by the sectional committees for reading, but a number of those that were read were not approved by the committees for publication, an example that might be very usefully followed in the case of our British Association. The general character of the meeting was stated to be decidedly scientific, and the discussions to have been carried on with good feeling, and free from personalities; though complaint was made that less sympathy was exhibited on the part of the citizens with the objects of the Association than at any previous meeting. The next meeting will be held at Hartford, Connecticut, on the second Wednesday in August 1874, when a report will be received from a special committee appointed to revise the constitution of the Association with a view to a better carrying-out of its objects. The general officers for the meeting will be Dr. J. L. LeConte, president; Prof. C. S. Lyman, vice-president; Dr. A. C. Hamlin, general secretary; and Mr. J. W. Putnam, permanent secretary.

Dr. Beke writes to the *Times* as follows with respect to Dr. Livingstone:—"If the intelligence from the West Coast of Africa is to be depended on, we may very shortly expect the return of our great traveller, Dr. Livingstone, to his native country On the 1st and 4th inst. you inserted communications from me, to the effect that our countryman was detained a prisoner at a place about 300 miles from Embomma, on the Congo. According to the news brought by the last African-Royal mail steamer, it was reported at St. Salvador that Livingstone was then in the interior, about 30 or 40 miles from that place. Now, as St. Salvador is only 80 miles from Embomma, the distance to the latter town from the spot at which, according to the later intelligence, our adventurous countryman was, is not more than 120 miles; and, Embomma being 70 miles from the mouth of the Congo, he would have been within 200 miles of the coast. As the hardy and energetic traveller is not in the habit of letting the grass grow under his feet, he may well be supposed to have come on nearly, if not quite, as quickly as the natives who brought the news of his whereabouts. Consequently, on the assumption that the intelligence received is founded on truth, we may not unreasonably look for the veteran traveller's arrival in England by the next mail steamer from the West Coast of Africa."

We learn from the Journal of the Society of Arts, that one of the first results in the rise of the price of coal has been the formation of a company in France, whose object is to utilise the power of the ocean tides on the French coast by proper machinery. The first experiment is to be made at St. Malo, where the tide rises nearly 80 ft., and overflows many square miles of flats.

Dr. George Burrows, F.R.S., has been appointed one of the Physicians-in-Ordinary to Her Majesty, in the room of the late Sir Henry Holland.

At a meeting of the Trustees of the Hunterian Collection of the College of Surgeons, held on Saturday, 8th inst., George Busk, F.R.S., was elected a member of the board, to fill the vacancy occasioned by the death of the Bishop of Winchester.

Dr. Lyon Playfair, C.B., F.R.S., M.P. for the Universities of St. Andrews and Edinburgh, has been appointed Postmaster-General in succession to Mr. Monsell. Dr. Playfair was a pupil of Liebig, was formerly Professor of Chemistry in the University of Edinburgh, and was at one time Government Inspector-General of Schools and Museums of Science and Art. We hope the new Postmaster-General will endeavour to introduce something like scientific method into the postal department.

The promoters of the railway tunnel which is intended to cross the Mersey, the shafts for which have already been sunk, have always believed that they would have only a continuous mass of solid sandstone rock to penetrate. A paper has just been published in the transactions of the Liverpool Geological Society for 1872, by Mr. T. Mellard Reade, C.E., of Liverpool, in which he contends that in all probability a deep gorge, filled up with clay or sand, will be met with, being the site of an ancient river or torrent formed in or before the times when England was covered with ice, and when its valleys were filled with glaciers. Mr. Reade believes that the ascertained data warrant the hypothesis, that before the boulder clays and other recent strata were laid down, a river draining the land now drained by the Mersey flowed past Runcorn Gap, between land of some considerable elevation, to the sea.

We have received, in the form of a neat little pamphlet of 20 pp., price only one penny, an exceedingly interesting lecture on "How Flowers are Fertilised," delivered by Mr. A. W. Bennett, F.L.S., at Manchester, on the 5th inst. It is one of a series of Science-Lectures for the People, published after delivery by Mr. Heywood of Manchester; they are carefully and neatly printed, and judging from the one before us, purchasers have a very good pennyworth indeed. The enterprise is very creditable to the publisher.

Among the papers presented to Parliament, says the *Times*, relating to the South Sea Islanders, is a report by Captain C. H. Simpson, of Her Majesty's ship *Blanche*, giving an account of his visit last year to the Solomons and other groups of islands in the Pacific Ocean. While at Isabel Island, Captain Simpson, with a party of officers, went a short distance inland to visit one of the remarkable tree villages peculiar, he believes, to this island. He found the village built on the summit of a rocky mountain rising almost perpendicular to a height of 800 ft. The party ascended by a native path from the interior, and found the extreme summit a mass of enormous rocks standing up like a castle, among which grow the gigantic trees, in the branches of which the houses of the natives are built. The stems of these trees lie perfectly straight and smooth, without a branch, to a height varying from 50 ft. to 150 ft. In the one Captain Simpson ascended the house was just 80 ft. from the ground; one close to it was about 120 ft. The only means of approach to these houses is by a ladder made of a creeper, suspended from a post within the house, and which, of course, can be hauled up at will. The houses are most ingeniously built, and are very firm and strong. Each house will contain from ten to twelve natives, and an ample store of stones is kept, which they throw both with slings and with the hand with great force and precision. At the foot of each of these trees is another hut, in which the family usually reside, the tree-house being only resorted to at night and during times of expected danger. In fact, however, they are never safe from surprise, notwithstanding all their precautions, as the great object in life among the people is to get each other's heads.

The additions to the Zoological Society's collection during the past week include an Alligator Terrapin (*Chelydra serpentina*) from North America, presented by the Smithsonian Institution of Washington; a large Hill Mynah (*Gracula intermedia*) from North India, presented by Rev. T. Main; twelve Gray's Terrapins (*Clemmys grayi*) from Bussorah, presented by Captain Phillips; a Changeable Tree Frog (*Hyla versicolor*) from North America, presented by Prof. Rolleston; a Ground Rat (*Aulacodus swinderianus*) from West Africa; a Sharp-nosed Badger (*Meles leptorhynchus*) from China; a Telerang Squirrel (*Sciurus bicolor*) from the East Indies; two Mantchurian Crossoptilons (*Crossoptilon mantchuricum*) from North China, and two Blue-crowned Hanging Parrakeets (*Loriculus galgulus*) from Malacca, purchased; an Agile Gibbon (*Hylobates agilis*) from Sumatra, deposited.

SCIENTIFIC SERIALS

THE November number of the *Monthly Microscopical Journal* commences with a paper by Dr. R. L. Maddox on an organism found in Fresh-pond Water, which he thinks to be new. The accompanying illustration, as well as the description, shows that the monads under consideration are of the simplest structure, and amœboid in character, of a violet tint, and highly refracting. They vary in size, and contain great numbers of little granular bodies embedded in the gelatinous matrix. The name *Pseudo-amœba violacea* is proposed for the new form.—Mr. F. Kitton describes some new species of Diatomaceæ, including *Aulacodiscus superbus* from Barbadoes, and others of the genera *Stictodiscus*, *Isthmia*, *Nitzschia*, and *Tryblionella*.—Mr. Carruthers answers Dr. Dawson's comments on his interpretation of the microscopic appearances of *Nematophycus* (Carruthers) or *Protolaxites* (Dawson). As he remarks, the question whether the plant under consideration is a sea-weed or a conifer, is entirely an histological one. Dr. Dawson, in his sections of the fossil found "wood cells, showing spiral fibres and obscure pores;" Mr. Carruthers finds "elongated cylindrical cells of two sizes, interwoven irregularly into a felted mass," and the latter observer substantiates the correctness of his observations and his drawings, which prove the accuracy of his views as to the affinities of the plant.—Mr. J. J. Woodward explains the optical principles involved in the construction of Mr. Tolles' new immersion objective that has caused the contest between him and Mr. Wenham.—Dr. Braithwaite continues his description of bog mosses, treating of figuring *Sphagnum rigidum* and *S. molle.*—This paper is followed by one on the investigation of Microscopic Forms by means of the images which they furnish of external objects, by Prof. O. N. Rood, of Troy, N.Y., which gives an extremely ingenious and simple method of testing with certainty, when the refractive indices of the body examined and the fluid in which it is immersed, are known, of determining whether markings, as of *Coscinodiscus triceratium*, are depressions or elevations; by regarding the object as part of the optical system, and thence finding whether its influence is that of a convex or concave lense.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, Nov. 5.—Prof. Ramsay, F.R.S., vice-president, in the chair.—The following communications were read:—"On the Skull of a species of *Halitherium* from the Red Crag of Suffolk," by Prof. W. H. Flower, F.R.S. A description of this has been already given in NATURE, at p. 13 of the present volume.—"New Facts bearing on the Inquiry concerning Forms intermediate between Birds and Reptiles," by Henry Woodward, F.R.S. The author, after giving a brief sketch of the Sauropsida, and referring especially to those points in which the Pterosaurians approach and differ from birds, spoke of the fossil birds and land reptiles which he considered to link together more closely the Sauropsida as a class. The most remarkable recent discoveries of fossil birds are:—(I.) *Archæopteryx macrura* (Owen), (II.) *Ichthyornis dispar* (Marsh), (III.) *Odontopteryx toliapica* (Owen). The author then referred to the Dinosauria, some of which he considered to present points of structure tending towards the so-called wingless birds. (I.) *Compsognathus longipes* (A. Wagner), from the Oolite of Solenhofen. (II.) The huge carnivorous *Megalosaurus*, ranging from the Lias to the Wealden. The author next drew attention to the Frilled Lizard of Australia, *Chlamydosaurus Kingii* (Gray), which has its fore limbs very much smaller than the hind limbs, and has been observed not only to sit up occasionally, but to run habitually upon the ground on its hind legs, its fore paws not touching the earth, which upright carriage necessitates special modifications of the sacrum and pelvis bones. The Solenhofen Limestone, in which Pterosauria are frequent, and which has yielded the remains of *Archæopteryx* and of *Compsognathus*, has also furnished a slab bearing a bipedal track, resembling what might be produced by *Chlamydosaurus* or *Compsognathus.* It shows a median track formed by the tail in being drawn along the ground; on each side of this the hind feet with outspread toes leave their mark, while the fore feet just touch the ground, leaving dot-like impressions nearer the median line. Hence the author thought that while some of the bipedal tracks which are met with from the Trias upwards may be the "spoor" of struthious birds, most of them are due to the bipedal progression of the Secondary Reptiles.—"Note on the Astragalus of *Iguanodon Mantelli*," by J. W. Hulke, F.R.S. The author exhibited and described an astragalus of *Iguanodon* from the collection of E. P. Wilkins. The bone was believed to be previously unknown. The upper surface presents a form exactly adapted to that of the distal end of the tibia, so that the applied surfaces of the astragalus and tibia must have interlocked in such a manner as to have precluded all motion between them. The author remarked upon the interest attaching to this fact in connection with the question of the relationship between the Dinosauria and Birds.—"Note on a very large Saurian Limb-bone, adapted for progression upon land, from the Kimmeridge Clay of Weymouth, Dorset," by J. W. Hulke, F.R.S. The bone described by the author presents a closer resemblance to the Crocodilian type of humerus than to any other bone, and he regarded it as the left humerus of the animal to which it belonged. The author refers it provisionally to a species of *Ceteosaurus*, which he proposes to name *C. humero-cristatus.*—A despatch from Mr. Alfred Biliotti, British Vice-Consul at Rhodes (dated June 16, 1873), communicated by H.M. Secretary of State for Foreign Affairs, and relating to a volcanic outburst in the island of Nisiros, one of the Sporades, in which there existed a volcano supposed to be extinct. Shortly before June 10 new craters opened in this volcano, and from them ashes, stones, and lava were ejected; many fissures, from which hot water flowed, were produced in the mountain, and the island was daily shaken by violent earthquakes.

Royal Astronomical Society, Nov. 14.—Prof. Cayley, president, in the chair. Sir Geo. B. Airy, the Astronomer-Royal, explained the general state of the preparations for the transit of Venus. First, as to the selection of stations. He had originally selected five observing-stations, and in making his choice he had endeavoured to keep in mind what other Governments were likely to do. He had been induced to recommend another station in Northern India for the purpose of taking a series of photographic observations to be used in conjunction with the photographic records to be obtained at the southern stations. As the French would not support the station which he had selected in the Sandwich Islands, by an expedition to the Marquesas Islands, he had found it necessary to recommend to our own Government that there should be two subsidiary observing stations in the Sandwich Islands. The station which had originally been chosen was Honolooloo, at about the middle of the islands; the new stations were to be Ha-wai-i to the east and an island at the western extremity of the group. The three stations would thus be distributed over a distance of some 300 miles—a fact which would greatly add to their chances of fine weather. He had also been considering the propriety of establishing stations at Christmas Island, at Hurd Island, and in Whisky Bay, but at present they knew little of the chances of anchorage or fine weather at these places. The *Challenger* was, however, about to visit and survey them. It would then proceed to Australia, whence the results of their investigations would no doubt be telegraphed to England. As to the selection of stations in the extreme south, the Admiralty would have nothing to do with any station where there was no anchorage, and where there were no human beings. Any station which laboured under both disqualifications must undoubtedly be rejected as unsuitable. He felt himself borne out in this determination by the fact that other nations had adopted the same practical view in their selection of stations. The Astronomer Royal then enumerated and pointed out upon a globe the stations which had been selected: 8 American, 5 French, 4 German, 19 Russian, and 8 English, besides the private enterprise of Lord Lindsay. He then proceeded to give a description of the now well-known "black drop," which was sometimes described as being so large as to make Venus appear "pear-shaped," at other times the illegitimate connection between Venus and the limb consisted only of a narrow black strap or band. The Astronomer-Royal had had a working model prepared at Greenwich with a black disc moved by clock-work. The black ligament, or drop, came out as a very marked feature of the contact with the artificial limb. And he hoped that Capt. Tupman would be able, from a discussion of the observations of different observers with different telescopes, to determine in what proportion the phenomenon was due to the aperture of the telescope used, and to what he might call the personal equation of the observer. He then proceeded to explain how when Venus was upon the sun's limb measures are to be made of the common chord of Venus and the limb, and how these measures are affected by the formation of a "black drop" between the two images.—Lord Lindsay then showed some photographs of a model of Venus upon the limb, in which the "black drop" was photographed as a remarkable feature. He pointed out that when the exposure was longest the "black drop" was most marked; and he showed that its size might be greatly reduced by using a stop which only permitted the rays from the central parts of the lenses to reach the plate. Dr. De La Rue said it was quite wonderful to see the amount of preparations which were going forward at Greenwich. It was not right to throw out such insinuations as Mr. Proctor had done about "official obstructiveness." Mr. Proctor's last paper in the *Monthly Notices* was a disgrace to the Society. In former days such papers never appeared.—A paper was read by Mr. Lassell on the finding of longitude with small instruments.—Mr. Ranyard then read a note upon a remarkable spot observed by Pastorff upon the sun's disc of May 26, 1828. In June 1819 Pastorff observed a nebulous spot with a bright nucleus upon the sun, which has since been recognised as being the comet of 1819 projected upon the bright background of the photosphere. The drawing referred to by Mr. Ranyard contained a similar though smaller nebulous marking with a bright centre. His object in bringing the drawing to the notice of the society was to inquire whether any small comet or known meteoric stream was between the earth and the sun on May 26, 1828.

Anthropological Institute, Nov. 11.—Prof. Busk, F.R.S., president, in the chair.—Mr. T. J. Hutchinson, F.R.G.S., H.M.'s consul at Callao, read a paper on "Explorations amongst ancient burial grounds, chiefly on the sea-coast valleys, of Peru," Part I. The object of the paper was to describe the "huacas" or burial-grounds, especially those lying beweent Arica and the Huatica Valley, and to expose some popular errors respecting them. Every bit of old wall, every heap of gravel, mound of earth, large or small cluster of ancient ruins of any kind is there called a "huaca." The term huaca (Quichua) is synonymous with Quilpa (Aymara) and means "sacred;" the title may therefore be considered as much applicable to the burying-grounds of Ancon, Pasamayo, and other places where there is no elevation above the country, as to those of Pando and Ocharán, large burial mounds in the valley of Huatica. The author proceeded to describe in detail the mode of interment and the various articles discovered. The celebrated Pacha-Cámác was described. Along the whole course of the Huatica Valley—from Callao to Chorillos—a distance of ten miles direct or sixteen miles round by Lima, there is no natural elevation that could be made available as a sub-structure for those colossal burial mounds. He gave at considerable length his reasons for concluding that there was no "Temple of the Sun" and no "House of the Virgins" of the Inca religion, and that every huaca was not a "Huaca de los Incas."—Dr. Simms, of New York, gave a most interesting and instructive communication on a flattened skull from Mameluke Island, Columbia River, and described minutely the practice of flattening the head in infancy. In reply to questions put to him, he said that the flattening does not seem to cause pain; that males and females are treated alike, although it had been supposed only males were so treated; that flattening is not apparently transmitted from parents to children; and that, judging from the general intelligence of the native Indians, the practice does not seem in any way to affect the brain or injure the health of the people.

MANCHESTER

Literary and Philosophical Society, October 7.—Edward Schunck, F.R.S., vice-president, in the chair. W. Boyd Dawkins, F.R.S., exhibited a fragment of a post struck by lightning on June 2, 1873. It was completely shattered, fragments being driven as far as the walls of the house, twenty-five yards off, and the downward direction of the loose splinters implied that the explosive force was exerted from below upwards, instead of from above downwards.—Mr. Baxendell thought it was most probably due to the sudden conversion of a portion of the moisture in the post into steam of high tension by the heating action of the electrical discharge, and mentioned instances in which condensed vapour was said to have been seen rising from trees immediately after they had been struck by lightning.—"On the Relative Work spent in Friction in giving Rotation to shot from Guns rifled with an increasing, and a uniform twist," by Osborne Reynolds, M.A., Professor of Engineering, Owens College, Manchester, and Fellow of Queen's College, Cambridge. The object of this paper was to show that the friction between the studs and the grooves necessary to give rotation to the shot consumes more work with an increasing than with a uniform twist; and that in the case of grooves which develop into parabolas, such as those used in the Woolwich guns, the waste from this cause is double what it would be if the twist was uniform. The following conclusions were arrived at by Prof. Reynolds:—

1. That when the pressure of the powder is constant,

$$\frac{\text{Work spent in friction with parabolic grooves}}{\text{Work spent in friction with plane grooves} \ldots} = \frac{3}{2}$$

2. That when the pressure diminishes rapidly the above ratio = 2.

3. That this ratio may have any values between these two, but that it cannot go beyond these limits.

PARIS

Academy of Sciences, November 10.—M. de Quatrefages, president, in the chair.—The following papers were read:—An examination of the law proposed by Herr Helmholtz for the representation of the action of two elements in a current, by M. J. Bertrand.—Remarks on an historical point in relation to animal heat, by M. Berthelot.—On the foundation of a meteorological observatory at the foot of the peak Du Midi by the Ramond Society, by M. Ch. Sainte-Claire Deville.—An extract from a letter from M. de Lesseps to Lord Granville on the projected Central Asian Railway. In the letter M. de Lesseps argued against the supposed danger of a Russian invasion of India, and expressed a hope that the Viceroy would permit his son and Mr. Stuart to commence their surveys.—On the structure of the teeth of the *Helodermata* and *Ophidians*, by M. P. Gervais.—Memoir on the problem of three bodies, by M. E. Mathieu.—Note on magnetism, by M. J. M. Gaugain. This formed the fifth of the author's notes on this subject.—Researches on the absorption of ammonia by saline solutions, by M. Raoult. The author stated that the difference between the coefficient of solubility of this gas in pure water and in saline solutions of the same salt is proportional to the weight of the salt dissolved in a given volume.—On the transpiration of water by plants in air and in carbonic anhydride, by M. A. Barthelémy.—New researches on the upward transport of nourishment by the bark of plants, by M. Faivre.—On the development of swellings on the rootlets of the vine, by M. Max. Cornu.—On certain cases of intermittence of the electric current, by M. A. Cazin.—On a process for finding the nodes of a sonorous tube, by M. Bourbouze.—On the presence and estimation of titanium and vanadium in the basalts of Clermont-Ferrand, by M. G. Roussel.—A method of estimating sugar by means of iron, by M. E. Riffard.—Certain facts relating to the development of bony tissue, by M. Ranvier.—On the *Pemphigus* of *Pistacia terebinthus* compared with the *Phylloxera quercûs*, by M. Derbès.—On a new kind of fossil Lemur recently found in the Quercy deposits of tricalcic phosphate, by M. Filhol.—On the influence of the moon on meteorological phenomena, by M. E. Marchand.—On a method for the determination of the direction and force of the wind; abolition of weathercocks, by M. H. Tarry.

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THURSDAY, NOVEMBER 27, 1873

THE SOUTHERN UPLANDS OF SCOTLAND *

II.

THE next member of the series of rocks making up the upper Llandeilo series in the Southern Uplands has received from the officers of the Scotch Geological Survey the name of the Lowther group. In its typical area, which is in the N.W. of Dumfriesshire, this group is composed of "fine grey shales and finely laminated felspathic greywackes with occasional grit beds." The estimated thickness of this group amounts to 5,000 feet. It is seen overlying the Haggis rock group in the streams which drain the upper portion of the Lowther hills; with the underlying Haggis Rock group it forms a synclinal trough in which the Lowther hills are contained.

In Wigtonshire the Lowther group rests upon the Dalveen group. The strata here generally correspond with those of Dumfriesshire, but shales are less abundant, and flagstones and grit with shale bands become more developed. In this county, however, the proportion of the fine and coarse rocks of this group varies in different localities. The rocks of the Lowther group in Wigtonshire are best exposed on the shores of the Irish Channel. Here between Morroch Bay and Knockienausk Head, cliffs are seen from 100 to 300 feet high composed of strata often very twisted and broken, belonging to the Lowther group; and in the higher portion of this group, where the flags are well developed, they have been worked for roofing and flooring purposes.

Above the Lowther group, and forming the highest member of the Upper Llandeilo series, as these occur in the Southern Uplands, are strata composed of grey shales with bands of fine-grained blue greywacke and flinty mudstones. Numerous bands of dark anthracitic shales with graptolites interstratify these rocks. These strata, with their associated anthracitic beds, have received the name of the "Upper Black Shale Group." Their estimated thickness is about 3,400 feet. This Upper Black Shale group occurs near the northern limits of the Upper Llandeilo rocks, and is more abundantly developed in Lanarkshire than in Dumfriesshire.

The Upper Black Shale group, in its typical area, has yielded the officers of the Geological Survey a rich graptolitic fauna, no less than 27 species having been obtained from this series of rocks. These species bear a very close resemblance to such as occur in the Moffat Shales, a horizon much below the Upper Black Shale group in position. Two Brachiopods have also been found in connection with these Upper Black Shales, viz., *Siphonotreta micula*, a form also occurring in the Moffat Shales, and likewise in the Upper Llandeilo rocks of Wales, especially in the neighbourhood of Bulith; and a *Discina* which has not yet been specially recognised.

The Upper Black Shales group, following the persistent strike of the Upper Llandeilo rocks of the Southern Uplands of Scotland, makes its appearance in Wigtonshire. Two bands of this group lying in a synclinal trough traverse the portion of Wigtonshire contained in Sheet 3. One of these bands is well seen in Morroch Bay, about a mile and a half south-east of Port Patrick. The other appears south-west of Stranraer, and crossing the moors to the north-east, is seen in the bed of the Luce below Cairnarzean. In Morroch Bay the Upper Black Shale group exhibits a threefold petrological nature. The higher beds consist of thin black shales, having in them lenticular masses and seams of coarse clay, ironstone, and nodular layers of greywacke and pyritous kernals. The strata here are much crumpled, and intrusive masses and veins of felstone have invaded them. It is in this upper portion of the group that graptolites occur, but the number of species obtained from these strata is considerably under what have been found in the Upper Black Shale group of Lanarkshire.

The representatives of the Upper Llandeilo rocks in the Southern Uplands of Scotland attain to a very great thickness. Of the lower portion of the series, the Ardwell group, the Lower or Moffat Black shale group, the Queenberry grit group, the Hartfell group, and the Daer group, the officers of the Geological Survey have not given their thickness in Dumfriesshire. Of the other four groups, the Dalveen, the Haggis Rock, the Lowther and the Upper Black Shales, these have an estimated thickness of 13,000 ft. If to this amount be added the five groups below, we have a development of Upper Llandeilo strata in the south of Scotland which must amount to nearly 20,000 ft. This great thickness of strata much exceeds the same series of rocks developed elsewhere in the British Isles.

The Upper Llandeilo rocks of the Southern Uplands of Scotland have a greater uniformity in their mineral nature than is usually common to the series. Greywacke in the form of shales, sandstones, grit, and conglomerates, having in some of their sub-divisions black shales containing graptolites, constitute this great thickness of sedimentary rocks. There is an absence of limestone strata, only nodules occurring occasionally, and the calcareous flags which are so characteristic of this portion of the Lower Silurian in its typical area Llandeilo, have no representatives in the South of Scotland. The rocks in this district have been originally greyish and reddish muds, grey and purple sands, and pebble-beds, with occasionally dark carbonaceous muds, which may have derived their black colour either from decaying sea-weeds or decomposing Hydrozoa. The presence of carbonate of lime seems to have been very rare in the Upper Llandeilo seas of the areas which are now recognised as the Southern Uplands, during the deposition of their strata, and to this great absence of carbonate of lime we may probably attribute the absence of some of the fossils which are so abundant in Wales in this series of rocks. Graptolites are essentially the characteristic fossils of the Upper Llandeilo of the Southern Uplands. The same species seem to run through whole strata from the Moffat Shales to the highest member of the series, having a range of probably 18,000 ft.; and many of these forms of graptolites are common alike to the Upper Llandeilo rocks of Wales and Scotland.

The case is, however, very different when we come to compare the crustacea of the two regions. In Scotland the Upper Llandeilo crustaceans are very few, and almost confined to Phyllopods, being *Peltocaris Harknessi*, *P. aptychoides*, and *Disinocaris Brownii*, while in Wales we have a considerable development of trilobitic life. Of the latter only one specimen, in the form of a tail, has yet been obtained from the Upper Llandeilo strata of the South of Scotland; and this specimen is too imperfect to admit of its being specifically determined. With reference to molluscs, these are nearly equally rare in the Southern Uplands. Only two Brachiopods have hitherto been recognised, while many forms appertaining to several genera have been obtained from the Welsh Upper Llandeilo strata. Notwithstanding the paucity of varied forms of organic remains in the Upper Llandeilo rocks of the Southern Uplands, their rich graptolitic fauna is at once indicative of their age, and the absence of other forms is most probably referable to want of calcareous strata in connection with these deposits.

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* Continued from p. 24.

The labours of the officers of the Geological Survey among the highly contorted and crumpled rocks of the Southern Uplands have afforded further information, were such required, of the causes from whence *cleavage* results. In a country so subject to flexures and contortions, where anticlinal axes and synclinal folds have been inverted, we should naturally look for abundant evidence of the superinduced structures from which true slates have derived their origin. The great mass of the Upper Llandeilo rocks of the South of Scotland rarely furnishes anything in the form of slates proper; and when we consider the nature of these rocks, which consist for the most part of greywacke sandstones and grits, we cannot fail to discover that the cause of the general absence of cleavage from these rocks has arisen from their petrological nature. The officers of the Survey have, however, in several instances, pointed out the recurrence of cleavage among the finer shales; and this occurrence usually accompanies violent contortions of the strata.

Although rocks of an Upper Llandeilo age enter so largely into the composition of the Southern Uplands, they are not the exclusive representatives of the Lower Silurian rocks in this area; above the Upper Llandeilo strata rocks referable to the Bala or Caradoc age occur. These Caradoc rocks, which occupy a very small area when contrasted with the Upper Llandeilo strata, are marked in the Southern Uplands by a feature which is unknown to their occurrence elsewhere. They are *unconformable* to the underlying Upper Llandeilo beds, a circumstance which Prof. Geikie well describes as "a new feature in the geology of Britain." The Caradoc rocks have not been recognised in Wigtonshire. They are described in connection with Sheet 15. They occur in a trough extending from Wedder Dod N.E. at least as far as the hills on the right bank of the Clyde, below Abington in Lanarkshire.

Here they are seen as greywackes, "passing on the one hand into a crumbling sandstone, and on the other into pebbly grits, with shale partings and with beds of conglomerate found chiefly at their base." In one spot a little concretionary limestone is seen, "the only example of limestone met with in the Lower Silurian rocks in Sheet 15." This limestone has afforded no fossils, but the conglomerates and the pebbly and gritty beds higher up in the series are abundantly fossiliferous. Denudation has probably removed some higher beds from this group. Its total thickness amounts to about 1,700 feet.

From the Caradoc rocks of the Lead Hills the geological surveyors have obtained a good series of fossils. We miss from their list the whole of the graptolites so abundant in and so characteristic of the Upper Llandeilo strata. In their place we have corals, trilobites, many forms of brachiopods, two lamellibranchiates, several gasteropods, and an arthorceras. Most of the species are characteristic Caradoc forms; but they have associated with them some which occur also in the Llandovery series.

The Southern Uplands of Scotland have other members of the great Silurian series besides those which have been referred to. These occur along a portion of the south-east flanks of the range, and consist of rocks having a general resemblance to the greywacke strata which form so large a part of the Upper Llandeilo rocks in the South of Scotland. The newer Silurian strata occurring on the south-east margin have, however, a very distinct series of fossils; and associated with their shales are found calcareous concretions frequently affording organic remains; the greywackes flaggy beds also in this higher group often contain fossils, especially graptolites. These graptolites belong to species occupying a much higher horizon than the forms which make their appearance in the Upper Llandeilo rocks; and the organic remains derived from the calcareous nodules also indicate strata higher in position than the Caradoc series. The rocks of an Upper Silurian age are well developed on the shores of the Stewarty of Kirkcudbright, especially on the eastern side of the mouth of the Dee. They occur also in Dumfriesshire, being seen near the southern margin of the Silurians at Dalton Mill, in the parish of Dalton, where the flaggy strata yield the same forms of graptolites which occur near the mouth of the Dee; and they have been extensively recognised in Roxburghshire.

As contrasted with the nearest area where Silurian rocks occur in England, the strata and the organic remains of the Southern Uplands of Scotland show great dissimilarity.

The distance of the nearest portion of the area where Silurian rocks are seen in England from the south-east side of the South of Scotland strata of the same series does not exceed 30 miles; for the northern flank of the Caldbeck range in Cumberland is not greater than this, in distance from the axis of the Lower Silurian rocks in Dumfriesshire where the Ardwell group occurs.

The Lake district of the north of England, occupied principally by Silurian rocks, exhibits strata of a lower position than any of the Silurian deposits of the Southern Uplands. These lower rocks of the Lake district are the Skiddaw slates of Prof. Sedgwick, which in many localities contain graptolites.

The facies of this graptolitic fauna is, however, widely different from that of the graptolitic fauna of the Upper Llandeilo rocks of the south of Scotland. In the Lake district there are no strata which can be paralleled with the Upper Llandeilo rocks. Above the Skiddaw slates of the north-west of England there occur great accumulations of igneous rocks in the form of traps, ashes, traptuffs and similar volcanic products. And it is only when the highest of these rocks is reached, which appear to have resulted from sub-aerial volcanic action, that strata occur in which organic remains are met with.

These strata, the Coniston limestones and their associated shales, are prolific in fossils of a nature indicative of the Caradoc age.

It is difficult to conceive how all traces of the vast igneous action which occurred within the distance of 30 miles from the Scottish Silurian area should be absent from the rocks of the Southern Uplands. The unconformability of the Caradoc deposits on the Upper Llandeilo strata in the Southern Uplands may perhaps afford some clue to this difficulty. The Skiddaw slates were probably ancient land in the area now occupied by the Lake district during the period of the deposition of the Upper Llandeilo rocks of the south of Scotland. This ancient land seems to have been subject to violent sub-aërial volcanic action, being the earlier epoch of the Caradoc series. During the later portion of the same epoch this violent volcanic action ceased, the area covered with igneous products again subsided beneath the sea, and allowed of the accumulation of the materials of the Coniston limestone and the succeeding groups.

In the Southern Uplands of Scotland the well-marked break recognised by the officers of the Survey points to a lapse of time between the deposition of the highest of the Upper Llandeilo groups and the conglomerates at the base of the Caradoc rocks. It is probably during this lapse of time that volcanic action was so rife on the other side of what is now the Solway Firth. This lapse of time is still further indicated by the comparative small development of the Caradoc rocks of the South of Scotland, as contrasted with those of the typical Caradoc areas of Shropshire and Wales, and also by their fossil contents, which indicate that only a portion of the group is represented in this area, and that this portion appertains to the upper part of the series.

From what has been said it will be apparent that the labours of the officers of the Geological Survey of Scotland have put us in possession of most important information concerning the very difficult series of rocks making up the strata of the bulk of the Southern Uplands. There are other matters amply detailed in the "Explanatory Memoirs" such as the metamorphism which the Silurian rocks have in some places undergone, and the intrusive rocks which are associated with them. The Old Red Sandstones as laid down in Sheet 15 are fully described. The important carboniferous areas of New Cumnock and Guelt, of Lugar and Muirkirk, and of Glespin or Douglas Water, with their thin limestone and low coal, are largely detailed. In relation to Dumfriesshire, the Sanquhar coal-field, made up of strata belonging to the true coal measures, and the carboniferous rocks which underlie it are also fully described. The Permian rocks of a portion of the Nith basin, having porphyries in different beds at their base, and brick-red sandstones with trapean detritus forming their upper portion, and also rocks of the same age occurring on the shore near Corsewall House, Wightonshire, are subjects treated of in the Memoirs. Igneous rocks of an age posterior to the Permian are also referred to. Superficial deposits in the condition of drift sands, and gravels, brick clays, and erratic blocks, also still more recent products in the form of raised sea beaches, blown sands, peat and alluvium are fully alluded to. Finally the explanations afford information concerning the economic minerals of the several districts, the whole containing a record of an amount of careful observations and inferences such as could only have been arrived at by the labour and experience of such a staff of officers as that which constitutes the Geological Survey.

ROBERT HARKNESS

LEYBOLD'S EXCURSION TO THE ARGENTINE PAMPAS

Escursion a las Pampas Arjentinas: hojas de mi diario: Febrero de 1871: Seguido de tablas de observaciones barométricas, un boceto de la ruta tomada. Por Federico Leybold. 8vo, pp. 108. (Santiago, 1873.)

THE publication of a book relating to Natural History in Chili is a rare event, and therefore well worthy of record. Except Philippi and Landbeck's "Catalago de las Aves Chilenas," and some few papers by the same authors in the "Anales" of the University of Santiago, the present is almost the first that has come before our notice. And these, it must be recollected, are not the productions of native Chilians, but of members of the all-pervading Teutonic race, who have brought their science with them from their distant fatherland.

Herr Leybold, or Don Federico Leybold, as we suppose we must call him, for he writes in Spanish, has been long resident in Santiago, and active in investigating every branch of Natural History in his adopted country. During the last few years, as he tells us in the introductory chapter of the present work, he has sent six expeditions over the Andes to explore the natural riches of the "Argentine Tempe," and finally in the month of February of 1871 was able to make arrangements to proceed himself upon a collecting tour into the same district. The route taken from Santiago was up the valley of the Maipo, to the junction with it of the "Valle del Yeso," and thence up this northern branch to the foot of the "Portillo de los Piuquenes," where the watershed was crossed. But a second and more elevated pass—the "Portillo Mendocino"—succeeds on this route over the main chain, which is, we believe, that usually taken to Mendoza. From the summit the descent was made over the elevated eastern slopes of the Mendozan Andes to an estancia called Vistaflores, situated at the foot of the range, which was made the headquarters of the party while they explored the surrounding country. Rainy weather and drunken servants much hindered operations during the stay at this place, which appears only to have lasted about a week, when it was determined to return to Santiago by the more southern "Paso del Diamante." This pass leads under the volcano of Maipo into the main valley of the Maipo, and thus enabled the travellers to join their former route after about a week's difficult and occasionally dangerous travel amid the snows and storms of the higher Andes.

Herr Leybold's diary of this interesting month's excursion is replete with notes and observations in every branch of Natural History—Zoology, Botany, and Geology. Birds, beetles, and plants appear to have engaged his chief attention—but other objects are not passed unnoticed. Not only are frequent references given to known species observed in the Andes and on the adjacent districts of the Argentine Republic, but descriptions are introduced of species believed to be new to science, and discovered on this occasion. Thus we have characterised

(p. 29) a new Crustacean—*Æglea andina* (pp. 36, 37), two new Violets, *Viola acanthophylla*, and *V. portulacea* (p. 38), a new Pigeon, *Columbina aurisquamata* (p. 45), *Oreosphacus*, a new genus of Menthoideæ; and subsequently two new Snakes, *Bothrops ammodytoides* and *Pelias trigonatus*.

As regards these and other supposed novelties, it may be remarked that it is not very convenient to scatter such descriptions through the pages of a book of travels, where they are liable to escape notice. Moreover, an isolated worker in a remote part of the earth's surface is in great danger of not knowing what is already known to others, and should take the precaution of consulting some correspondent in the great European centres of scientific activity before publishing what is new to him as new to every one else. Dr. Finsch has already shown that Leybold's *Conurus glaucifrons* is a well-known species of Parrot; and we do not doubt that most of the other supposed novelties will be found to have been previously described elsewhere.

P. L. S.

A HEALTHY HOUSE

What a House should be, versus Death in the House. By William Bardwell, Architect and Sanitary Engineer. (London: Dean and Son.)

THE author of this work is evidently an enthusiast in sanitary matters, but there is much in it worth the attention of the professional architect and builder, as also of the house-owner and occupier. It will be some time before the precepts of hygienic architecture can be expected to pervade all classes of the community; but reforms in this direction must commence from above, and will gradually be accepted by the poorer classes: this work will assist the dissemination of wholesome rules.

The subject of drainage, which necessarily occupies much of the work, has been forced into prominence by the dangerous illness of the Prince of Wales, in the Autumn of 1871; and this work meets to some extent the demand for further and better information on the subject. Our author is not new to the task, having so long ago as 1828 turned his attention to the sanitary conditions of buildings, and has published several treatises on cognate subjects. The work before us, however, is suggestive rather than profound, and we find a tendency in it to describe very prosaic details in stilted language. There is also a general want of references, so that many of the statements cannot be easily verified—such, for instance, as this, p. 6, art. 10:—"We have progressed some little since 1828, when my first essays on health were published, and public attention has been directed to the subject; but still, one half of the children born in London and other large towns, die before they are three years old; while at a parish in Norfolk, where the principles here set forth are rigidly enforced by the excellent rector, a child is never known to die." After making, however, every abatement—as we are bound to do—the work will not fail to prove very useful, and will assist in leading people to better sanatory arrangements.

In p. 8 he justly animadverts on many modern cottages, which "from admiration of mediæval architecture are irregular in plan, and irregular in outline from an idea of being picturesque; and hence the chimneys are outside, involving loss of heat, the roof all hips and valleys, and dormer windows requiring constant repairs, and exhibiting an utter ignorance of the very first principles of a healthy home." Some fallacy seems, however, involved in the passage which follows, and which describes the effect of asphalted ground floors in some Essex cottages. The inhabitants suffered from rheumatism until the asphalte was covered with boards—"because the boards were conductors of damp, whilst the asphalte was a non-conductor of moisture." It must have been the conduction of temperature, and not of moisture, that led to the inconvenience.

Chapter ii. is on bad drainage, and opens sensibly thus:—"The use of water in cabinets in disobedience to God's command to the Israelites to bury excreta in the earth is unquestionably the cause of those alarming modern diseases—the something in the air—with which the whole country is affected." It may be impossible to return to the more primitive practice, but the fact remains that even the old cesspool system was less unhealthy than the modern more artificial one. Some valuable hints are given in pages 12—13, for discovering the inlets of sewer gases into houses. The closet soil-pipe is often the origin of these irruptions; for the inclosed gases decompose the soldered joints of the lead pipe in a few years' time, if the pipe is not ventilated, as indeed it seldom is, and the junction of the lead-pipe with the drain is often defective. Every sink, too, which modern luxury has introduced to save the old-fashioned labour of throwing slops away out of doors, opens a pathway for the poisonous gases, of which one part in 260 mixed with common air is fatal to life, and of which no sensible proportion can long be breathed with impunity.

There is also a moral aspect to the question. The following passage is introduced as a quotation, but it does not appear from what author, p. 19:—

> "A clean, fresh, and well-ordered house exercises over its inmates a moral no less than a physical influence, and has a direct tendency to make the members of the family sober, peaceable, and considerate of the feelings and happiness of each other."

In chap. iii. are some valuable remarks about drains stink-traps, and rain-water pipes. Water-closets, it is said in p. 29, should never be in a basement—for if so, the house is liable to draw its supply of air through them—but always in a back-yard. Those that are wanted to be in immediate connection with the house should be in the upper floors only, and, whenever practicable, approached through a greenhouse.

At p. 33 are some remarks on the necessity of pure, untainted water; and, in p. 34, on the danger of lead-poisoning. The pipes made by Messrs. Walker, Campbell & Co., of Liverpool—lead-cased block-tin pipes—are recommended in those cases where the water acts upon lead. A caution as to the use of these pipes should, however, have been added, as very great care and peculiar arrangements are required in jointing them; otherwise, the combination of the two metals becomes exposed to the action of the water at the joints, when decomposition will take place, and the water will still be affected with lead.

In p. 41 the importance of a dry basement is inculcated, and with a well-merited encomium on Mr. John Taylor's clever contrivance of the damp-proof course' which both keeps down the damp and ventilates the ground-floor. Proceeding to fire-proofing methods, Mr. David Hartley's simple but little known contrivance for protecting dwelling-houses from fire by interposing sheet-iron or copper between the floor boards and the joists is mentioned. The plan described a little farther on, p. 46-47, would probably not be so effective as Hartley's.

In pp. 48-58 fire-grates are mentioned, and with a decided preference (perfectly justified in the experience of the writer of these remarks) for Mr. John Taylor's smoke-consuming grate; but the author should hardly have left Dr. Arnott's smoke-consuming contrivances unnoticed; and when at pp. 61-66 he speaks of ventilation, he should have mentioned at greater length Dr. Arnott's ventilating valve. Boyle's ingenious ventilators, however, quite deserve the praise given them in p. 63.

It would be interesting to have had some references given to sanction our author in claiming the authority of the Duke of Wellington, together with that of Aaron and the High Priests, his successors, for the practice of placing their beds nearly north and south so as to be in the line of the magnetic current. The theory no doubt has its advocates, but can hardly be of universal application, as there are many sound sleepers at all degrees of orientation.

Chapter iv. contains some good suggestions respecting London street improvements and the Sanitary Recipes at the end will be found deserving attention.

OUR BOOK SHELF

Natural Philosophy. Part I. Mechanics. By J. Alfred Skertchley. Pp. 168. (London: Thomas Murby, 1873.)

THIS work belongs to a series of small manuals which the publisher calls the "Science and Art Department Series of Text Books." It is designed for students who possess but little mathematical knowledge, and each of the theorems discussed is explained in very simple language. In some respects the work keeps pace with modern text-books, in others it lags behind them. Thus while we have chapters on Kinetics and Kinematics, and on Actual and Potential Energy, we find some of the units as primitive as possible, and the Metric system is ignored. The unit of length is given as the yard, and the unit of weight as the grain. The definitions leave much to be desired: thus Mechanics is defined as "the Science which treats of the laws of motion and force, especially as applied to the construction of Machines;" Hydrostatics "the science treating of the pressure of water." Again we find the following very loose definition of the force of gravity: "Every particle of matter has a tendency to draw to itself every other particle, and this tendency is called the force of gravity." The other attractive forces are here ignored, the student is left quite in ignorance as to whether the force acts through a sensible or insensible space, whether it acts between particles or masses, whether such particles or masses are necessarily of similar or dissimilar substances. A screw is defined as "an inclined plane *revolving* round a centre." "Any body capable of moving freely about a fixed axis is a pendulum." The chapter relating to Energy requires to be carefully revised, as, indeed, does much of the work so far as accurate and logical definition is concerned. The examples are useful, and the questions at the end of the book will be found of service in teaching elementary Science, but the book can scarcely be recommended until the definitions are more precise and absolute.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Dutch Photographs of the Eclipse of 1871

IN the account of the proceedings of the meeting of June 13 last of the Royal Astronomical Society, as published in Vol. viii. p. 175, of NATURE, I read the following:—

"Mr. Ranyard remarked that the paper copies of the Dutch photographs which he had seen had been printed from enlargements on glass, in which the moon had been stopped out with black paper or some other material. On measuring he had found that the body of the moon, as given in the photographs was by no means circular, and Mr. Davis had pointed out to him that the irradiation under the prominences was perfectly sharp at the edges, as it would be when printed through paper: It was therefore unfair to institute any comparisons as to the amount of the irradiation in these and in the other photographs."

I beg leave to state, in opposition to Mr. Ranyard's and Mr. Davis's remarks, that no stopping out with black or any other paper has taken place. I enclose hereby copies on paper of the originals and of one of the enlargements. In the first-mentioned everyone may see that the moon is sufficiently dark to render unnecessary every artifice before making a good enlargement. In fact I have seen the enlargements myself, and in them, too, the moon was as dark as the surrounding sky.

I think Mr. Dietrich's merit to be especially this, that he has directed the attention of astronomers again to a method, as it seems already wholly abandoned, if ever earnestly tried, viz. that of taking an image with a photographic lens of short focus but great force, so that a very short exposure might be sufficient. As to the profit his photographs brought to our knowledge of the sun, Col. Tennant says, almost every depression of outline of the Indian photographs could be recognised in the Java ones, and thereby it is proved that in the interval of time needed by the moon's shadow to make the traject from India to Java, say 50 minutes, almost no change whatever took place in the solar corona.

Of course the method could be improved by moving the camera by clockwork. Then the exposure could last a little longer, *e.g.*, one second, and the exterior outline would reach farther; a larger camera, with photographic lens of the same force would without doubt give more details.

As to the not-circular (in fact elliptical) form of the moon in the photographs, I think it pleads more against than in favour of Mr. Ranyard's remark, for if a disc of paper were to be used to stop out the moon, of course a circular one would have been made, and not an elliptical one. The fact is that the copies of the original *cliché* present the same peculiarity, the difference between the longest and shortest diameter being about $\frac{1}{10}$th of a millimeter, as is easily recognised with a lens and a measure of half-millimeters. In the accompanying diapositive the difference = $\frac{1}{2}$ mm. As in other photographs of total eclipses, the diameter corresponding to the poles of the sun is the longer. This phenomenon is in our case only partially explained by the moon's motion during the time of exposure; perhaps a stronger impression at the equatorial regions of the sun, or a trembling of the camera-stand has done the remainder.

In the glass photographs, of which I have sent a pair to Lord Lindsay and to Messrs. Lockyer, Huggins, Warren De La Rue, and Main, the details are finer and sharper than in the paper ones.

J. A. C. OUDEMANS

Batavia, Sept. 10

[We have no doubt from an inspection of the photographs sent, that no stop was used.—ED.]

Elevation of Mountains and Volcanic Theories

THE accompanying letter from Captain Hutton is in acknowledgment of my paper on "The Elevation of Mountains by Lateral Pressure," which I read at Cambridge in 1869. I sent it to him in consequence of seeing his lecture on Mountains, in the *Geological Magazine.* He could not have received my critique on that lecture at the time of his writing this letter. In accordance with his suggestion I forward it for publication in NATURE without comment.

OSMOND FISHER

Harlton Rectory, Cambridge

I have to thank you for sending me your paper on the Elevation of Mountains, which I have read with great interest. You and Mr. Mallet have done great service to geology by exploding the old-fashioned idea of cavities existing in the interior of the earth. I quite agree with you that a cooling earth must give rise to great pressure in the outer consolidated layers, and that this pressure must crush the rocks composing it; but I cannot think that this crushing is the cause of the elevation of mountains. My reasons for disagreeing with you are the following:—

1. The pressure from a shrinking globe must be uniform, and the lines of least resistance, once chosen, should remain always the same, and the elevation should be continuous. All minor differences would be insignificant in comparison with the flatter arch at the poles. These areas, therefore, would subside, and mountain chains should have had from the first an east and west direction. I see no provision for changing the localities of movement.

2. Where deposition was going on the rocks would be heating and no contraction could occur below them. But mountain chains have been always formed where the deposits were the heaviest, and where, therefore, uplifting would not be likely to occur.

3. All mountain chains are not formed on the same system, but can be divided into two groups, as I have pointed out in my lecture on this subject.

4. Whether a glacial epoch has ever extended over the whole earth or not, it is certain that the northern parts of America and Europe are much warmer now than they were in the Pleistocene period, consequently the rocks under them could not have contracted, and yet we know that extensive movements are even now going on in this area.

5. In order to produce a strain on the surface, the lower contracting rocks must be solid, consequently there would be nothing to support a large anticlinal, and no rocks to pass into the liquid state; the result would be a general small crumpling all along the surface. The relief also to the compression of the upper rocks could not be obtained by a single rising at a point, or along a line, without a horizontal movement of one bed over another, which appears to me to be impossible. Consequently I do not think that the shrinking could produce the observed effects, more especially as the Himalayas, &c. are of tertiary age, and the contraction of the globe, since the cretaceous period, cannot have been very great. These remarks apply also to Prof. Shaler's theory (Proc. Bost. Soc. Nat. Hist. 1866). Mr. Medlicott's section of the Himalayas is, to my mind, physically impossible. It is inconceivable that the beds could be engineered into the positions in which he has placed them.

6. The theory does not account for the numerous minor oscillations of level that coal measures often prove to have taken place.

7. The theory makes no provision for tension in the rocks. But it is a fact not sufficiently dwelt upon by geologists, that faults just as surely prove tension in rocks as contortions prove compression.

I have also a few objections to your theory of Volcanoes, and also to that of Mr. Mallet. They are as follows:—

1. The density of the crust has been shown by General Sabine to increase in volcanic regions, while, by your theory, it should decrease. Mr. Mallet's theory would account for this, as also would the one proposed in my lecture.

2. To cause a volcano the heat must go to the water, for the water cannot go to the heated rock, as your theory would require.

3. Volcanoes are not found in contorted countries, or where great lateral pressure has existed. In the older volcanic districts (*e.g.* North Wales) the eruptions occurred before the folding of the strata. This is also a strong point against Mr. Mallet's theory.

4. By Mr. Mallet's theory the crushing must be very sudden, or the heat would be conducted away, and as each eruption would require a fresh accession of heat, it ought to be preceded by elevation or subsidence on a large scale. The earthquakes that precede eruptions are just as likely to be effects as causes.

5. Faults show no heating where considerable crushing has taken place.

Such are the objections that occur to me, but, after all, we cannot well burke the question as to the state of the interior of the earth, and I must confess that the "Viscidists" appear to me to have a better position than the "Rigidists."

Mr. Hopkins' argument, drawn from precession and nutation, has proved untenable, and the only stronghold that the "Rigidists" now retain is the absence-of-internal-tide argument of Sir W. Thomson. This has not yet been assaulted, but it probably has a weak point somewhere, for its author has allowed that the interior of the earth is probably "at, or very nearly at, the proper melting temperature for the pressure at each depth," which seems hardly consistent with its being "more rigid than glass." On the other hand, the "Viscidists" have a very strong point in the fact that faults are known with throws of several thousand feet (which apparently must penetrate into some yielding material), as well as some minor positions, such as the supposed effect of the moon on causing earthquakes, the composition of volcanic rocks (which contain more alkali than could be obtained by merely melting sedimentary rocks), and the mode of occurrence of granitic rocks, none of which have been seriously attacked by the "Rigidists."

At this distance I cannot take part in a discussion, as I must always be five months behind hand, but if you think that a preliminary skirmish in the pages of NATURE would do good, although it did not bring on a decisive battle, you are quite welcome to publish this letter. F. W. HUTTON

Wellington, N. Z., July 21

P.S.—At the time of writing my paper on Elevation and Subsidence (*Phil. Mag.* Dec. '72), I was not aware that Mr. Scrope had been the first to suggest* the theory there developed, or I should certainly have mentioned his name, and not proposed to call the theory after Herschel and Babbage. I feel that I owe Mr. Scrope some apology for my inadvertence.

Deep-Sea Sounding and Deep-Sea Thermometers

WE have again to claim your indulgence for occupying space for a few comments on Mr. Casella's reply to our letter.

It is not true that we abstained from drawing attention during the lifetime of Dr. Miller to the fact that he had plagiarised our invention; on the contrary, we wrote to Dr. Miller as soon as we were told that he had read a paper before the Royal Society on his supposed invention, and we have before us Dr. Miller's answer, dated Nov. 23, 1869, wherein he writes:

"I am sorry if I have inadvertently done anything which may fairly be considered an injustice to you in respect to the deep-sea thermometer," &c.

We believe Dr. Miller did not know of our thermometer, but Mr. Casella did, having had one or more in his possession years previously, and as a fact our thermometer was well known in the trade; therefore he as the workman employed by Dr. Miller ought to have acquainted that gentleman with the fact. It is most likely that we should not have taken any further notice had the thermometer retained the modest title given to it by Dr. Miller, viz. the "Miller-pattern." This, however, did not suit Mr. Casella. Mr. Miller died—"mors tua vita mea,"—and forthwith the thermometer is styled the Miller-Casella, then by a little "progressive development," the instrument is brought out at the British Association as the Casella-Miller, and to day we have it in Mr. Casella's letter as "*my thermometer*."

On reference to the Royal Society's Proceedings, vol. xvii. p. 482, we find no mention of Mr. Casella's name except as the workman who took Dr. Miller's instructions, and we have yet to learn what right a workman has to appropriate to himself an instrument made for Dr. Miller, or any other customer, supposing, even for argument's sake, that we had no priority in its invention.

Mr. Casella asks "What has Negretti and Zambra's thermometer done that it should be known?"

In the first place it served him as a pattern, it showed him how the best deep-sea thermometer was constructed, and how to make others on the same principle; and we contend that had our instruments been placed in the hands of skilful, careful, and trained observers, such as are now engaged in the *Challenger* Expedition, they would have given results equal to those now obtained with the instruments supplied by Mr. Casella, and obviously so, their principle being precisely the same.

Mr. Casella talks about our thermometers having failed. Can Mr. Casella point out where are recorded any of the failures? Was Mr. Casella able to make them fail when he tried by placing one of them in his hydraulic press in the presence of gentlemen connected with the Meteorological Office? But this is not the point at issue, the sole question is, are the thermometers supplied to the expedition the same in principle as ours, or are they not?

Doubtless it would be much more agreeable to Mr. Casella that these questions should be decided by himself in private, hence his invitation to your readers "to go to his establishment and hear his explanation." Surely no such arrangement will satisfy "all the scientific men in the world." We contend that as Mr. Casella has publicly claimed the invention as his own, it ought to be decided with equal publicity whether he has done anything more than copy our instrument.

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* "Volcanoes," 1st ed. 1826, p. 30.

We again give the description of our thermometer (not in our own words, for we might be accused of shaping them to suit our purpose) but in the words of the late Admiral Fitzroy as they appear in the first number of Meteorological Papers, page 55, published July 5, 1857, in referring to the erroneous readings of all thermometers consequent on their delicate bulbs being compressed by the great pressure of the ocean, Admiral Fitzroy says:—

"With a view to obviate this failing, Messrs. Negretti and Zambra undertook to make a case for the weak bulbs which should transmit temperature but resist pressure. Accordingly, a tube of thick glass is sealed outside the delicate bulb between which and the casing is a space all round which is nearly filled with mercury. The small space not so filled is a vacuum into which the mercury can be expanded, or forced by heat or mechanical compression, without doing injury to, or even compressing the inner or much more delicate bulb," &c. &c.

Mr. Casella "did not wish to take up your valuable space to describe his thermometer." Well, it matters not; the late Admiral Fitzroy has done it for him. He described it sixteen years ago; and if the reader will take every syllable of the extract above quoted, and substitute the word "alcohol" for "mercury" (which colourable change was effected by Mr. Casella, to the detriment of the instrument), they will have a correct description of Mr. Casella's thermometer in the most minute details.

HY. NEGRETTI AND ZAMBRA

Rain-gauge at Sea

I BEG to send you a copy of a letter I received lately from Capt. Goodenough, of the Royal Navy, respecting the use of my rain-gauge at sea. (See NATURE, vol. vii. p. 202.)

Nov. 8 W. J. BLACK

"H.M.S. *Pearl*, lat. 6° S., long. 22 W.

"Dear Sir,—I should have taken an earlier opportunity of writing to you about the instrument which you were so good as to design for use on board ship, but have not had the good fortune to fall in with any rain up to the present time with which I could at all events in some measure test and chronicle the rain-gauge. It is odd that in a journey of twenty days I have had only ·07 in. of rain, and that although I am at this moment in a district in which an average of seven hours' rain usually falls at this time of the year. On that one occasion ·07 in. did fall and was duly caught in your instrument as well as in another mounted on gimbals, the measurements being exactly alike in each. I much prefer the mounting of your instrument, and will report to you as to the amount of weight it requires after some experimenting with it. The usually most steady instrument is one which is heavy, and whose centre of gravity is very near its centre of oscillation. I do not think it would be well to increase the size of the instrument, as it would become inconvenient to place, except for the use of a man who wishes to devote himself very much to that order of observation. Our poop is so high here that I do not anticipate any mixture of sea-spray in the gauge, but if it were so your table would be sufficient to clear it, supposing we had Carpenter's Hydrometer to test with, as we might not expect enough water to float an ordinary one.

"I remain, yours very truly,
"JAMES E. GOODENOUGH
"Captain R.N. Command H.M.S. *Pearl*, proceeding *viâ* the Cape to Australasia."

Glaciers

IN a letter printed in your number for Oct. 16 (vol. viii. p. 506), Mr. J. H. Rohrs states that he believes that glaciers existed at or near the sea-level in central Hindustan in the glacial period. Glaciers undoubtedly existed in the Himalyas at a much lower elevation than at present; there are traces of their action in Sikkim in valleys, the bottoms of which are now only 4,000 ft. above the sea, and in the north-western Himalayas, Mr. Medlicott, I think, considers that in some valleys, glaciers descended to within 1,000 ft. of the sea-level, but I have never heard of any marks of old glacial action in the Indian peninsula south of the Himalayas. There are no mountains in central Hindostan exceeding about 4,000 ft. in height, and a careful examination of the portions of the Nilgiri mountains in Southern India, which rise above 8,000 ft., has not afforded any proof of the former presence of ice. It is very probable that Mr. Rohrs possesses information upon this subject with which I am unacquainted, and it is without the least wish to express a doubt of the accuracy of his information, that I ask for any evidence he can produce in favour of his assertion, as the subject is one in which I am greatly interested.

W. T. BLANFORD

JOHANN NEPOMUK CZERMAK

JOHANN NEPOMUK CZERMAK was born June 17, 1828, in Prague. His father, Johann Conrad Czermak, was a medical practitioner of high repute in that city, and his uncle, Joseph Julius Czermak, enjoyed a considerable reputation as Professor of Medicine and Physiology, first at Gratz and afterwards at Vienna. Educated at the high school of his native town, Johann Czermak entered upon the study of medicine at the University of Vienna in 1845. In 1847 he moved to Breslau, where he had the great advantage of living with the distinguished physiologist Purkinje. From Breslau he passed on in 1849 to Wurzburg, where in 1850 he received the degree of M.D., publishing on that occasion an inaugural dissertation on "The Microscopical Anatomy of the Teeth," in which he called attention to the larger "interglobular" spaces so often found in the upper part of the dentine. After a visit to England he settled at Prague, where he became assistant to Purkinje, who then held the chair of Physiology in that place. In 1855 he left Prague to take the chair of Zoology at Gratz; but zoology was not his proper province, and he gladly accepted in 1856 the offer of the Professorship of Physiology at Krakau, which however he left in the following year for the like chair in Pesth. In both these universities he established physiological laboratories and gave a decided impulse to physiological research; but the political agitations then rife made life distasteful to him there, and in 1860 he resigned his chair and returned to Prague. Such frequent changes must have interfered greatly with sustained research, but by this time Czermak had made his name known as well by several investigations in experimental physiology and in subjective vision, as especially by his researches on the laryngoscope, his treatise on which ("Der Kehlkopfspiegel und seine Verwerthung") embodying the results made known in various papers in 1858 and 1859, he published shortly before his return to Prague.

Here he resided some years, visiting at times England, Holland, and France, in order to make the value of the laryngoscope better known to his fellow-workers in science and medicine. There are many in England who retain pleasant memories of these visits.

The ample means brought to him by the gifted lady whom he had the happiness to marry, enabled him to build in Prague and furnish at his own expense a private laboratory for research, in which he not only worked himself, but which he also placed at the disposal of others. Many would have envied, and few would willingly have let slip, such an opportunity for quiet labour; but Czermak, conscious of the power he possessed of lucid exposition, delighted in teaching, and felt perhaps the want of the stimulus which pupils afford. Accordingly, when in 1865 he was offered the chair of Physiology in Jena, vacated by the removal of von Bezold to Wurzburg, he at once accepted it. Here he continued until, in 1869, finding the disease to which he eventually succumbed (and the beginning of which he himself attributed to the irritation caused by the controversies which arose out of his laryngoscopic work), was rendering him unfitted for the energetic performance of his professorial duties, he withdrew to Leipzig, where he was made Honorary Professor at the University, and where he continued to reside until his death, on Sept. 16 in the present year.

Carried off while yet in the prime of his life, and the energies of his last few years impaired by an insidious disease, Czermak has perhaps left a mark on the scientific progress of his time incommensurate with his talents or his promise. He will doubtless be best remembered through his laryngoscopic labours. We owe to him the real introduction into medical practice of this valuable instrument. But his other researches, such as those on the action of the vagus, the pulse, the sense of touch, the manége movements resulting from injuries to the brain, those on dyspnœa, and others, show remarkable acuteness and clearness of insight.

Two talents he possessed deserve special notice. He had remarkable aptitude in devising apparatus for observing or for demonstrating physiological phenomena. It was this faculty which made him successful where others had failed in the use of the laryngeal mirror; and would be difficult to exaggerate the immense help to experimental physiology which has been afforded by the ingenious "holder" which bears his name.

The other faculty, that of popular exposition, less common in his country than in ours, he possessed to a very high degree. And his popular lectures, which were originally delivered at Jena, and which were reviewed in an early number of NATURE, achieved and deserved great popularity.

Perhaps had his love of teaching been less strong, his work as an investigator would have been more sustained and weighty. But while in this country we might with profit often lose a lecturer and gain an investigator, Germany could well afford that one whose powers of rigorous and yet clear and popular demonstration were so exceptionally great, should somewhat slacken in his work as an inquirer. Or perhaps we should not so much say that Czermak slackened in inquiry, as that the consciousness of his power as an expositor, and the delight he consequently took in exposition, drew much of his energy in that direction. In the grounds of his residence at Leipzig he had built and fitted, at his own expense, a large hall, or "spectatorium," as he called it, in which he proposed to deliver lectures on physiology, richly illustrated with experiments. In connection with the hall, the construction of which was admirably adapted in every way for its purpose, he had also erected a private laboratory for research; and on both he had spent much time and labour. They were intended to be a supplement—not a rival—to the more technical institute of Prof. Ludwig in the same city. The writer will never forget the delight with which Czermak showed this "Erklarungs-Tempel," —as he was fond of calling it—to Dr. Sharpey and himself in the summer of 1871, and pointed out all its ingenious contrivances, and the enthusiasm with which he looked forward to the lectures which would be delivered, and the work which would be carried on in it. He lived to open it by an inaugural lecture in December 1872; but the effects of his fatal disease were already painfully evident; and after a vain struggle during the following summer, Czermak—just as the British Association was gathering for its meeting at Bradford—was taken away from his unfinished work. He was a man of broad culture, outside his professional attainments. In philosophy especially he was well versed; and his last contribution to scientific literature—a paper in "Pfluger's Archiv," on the mesmerism of animals—was doubtless prompted by his interest in psychological questions. His straightforward, generous, and unostentatious manner formed a fitting frame for his intellectual attainments.

A widow and children mourn his death. He is also mourned for by many friends in many lands, both by those who had known him long and by those who knew him for a short while only.

M. FOSTER

THE ATMOSPHERIC TELEGRAPH

THE *Times* of the 15th inst. contained an article on the Pneumatic Despatch, which has never been used to any extent in this country. From that article we learn the following particulars as to the working of this method of conveyance in London:—

The pneumatic tube extends from the London and North-Western Railway Station at Euston Square to the General Post Office in St. Martin's-le-Grand. The central station is in Holborn, where is also the machinery for effecting the transit of the trains. Here the tube is divided, so that in effect there are two tubes opening into the station, one from Euston to Holborn, and the other from the Post Office. The length of the tube between Holborn and Euston is 3,080 yards, or exactly a mile and three-quarters, a greater length than was originally contemplated, but which was rendered necessary by the avoidance of certain property on the route. The tube is of a flattened horse-shoe section 5 ft. wide and 4 ft. 6 in. high at the centre, having a sectional area of 17 square feet. The straight portions of the line are formed of a continuous cast-iron tube, the curved lengths being constructed in brickwork, with a facing of cement. The gradients are easy; the two chief are 1 in 45 and 1 in 60, some portions of the line being on the level; the sharpest curve is that near the Holborn station, which is 70 ft. radius. The tube between Holborn and the Post Office is 1,658 yards in length, or 102 yards less than a mile, and is of the same section, and similarly constructed to the first length. Two gradients of 1 in 15 occur on the Post Office section, but this steep inclination is in no way inimical to the working of the system. The Holborn station is situated at right angles to the line of the tubes, which are therefore turned towards the station into which each opens. All through trains, therefore, have to reverse there, and this is effected in a very simple manner by a self-acting arrangement. A train upon its arrival runs by virtue of its acquired momentum up a short incline, at the summit of which it momentarily stops, and then quickly descends by gravity. In its descent it is turned on to a pair of rails leading to the other tube, into which it enters and through which it continues its journey, the whole process of reversing occupying barely 30 seconds. Trains containing goods for the Holborn station are simply run down from the top of the incline on to a siding.

The waggons, or carriers, as they are termed, weigh 22 cwt., are 10 ft. 4 in. in length, and have a transverse contour conforming to that of the tube. They are, however, of a slightly smaller area than the tube itself, the difference—about an inch all round—being occupied by a flange of indiarubber, which causes the carrier to fit the tube exactly, and so to form a piston upon which the air acts. The machinery for propelling the carriers consists of a steam engine having a pair of 24-in. cylinders with 20 in. stroke. This engine drives a fan 22 ft. 6 in. in diameter, and the two are geared together in such a manner that one revolution of the former gives two of the latter, or, in technical terms, the engine is geared at 2 to 1 with the fan. The trains are drawn from Euston and the Post Office by exhaustion, and are propelled to those points by pressure. The working of the fan, however, is not reversed to suit these constantly varying conditions; it works continuously, the alternate action of pressure and exhaustion being governed by valves. The engine takes steam from three Cornish boilers, each 30 ft. long and 6 ft. 6 in. in diameter. Telegraphic signalling is carried on between the three stations by means of needle instruments.

The system of Pneumatic Despatch, or "Atmospheric Telegraph," as the French call it, is utilised to a much greater extent in Paris than in London, though with some important differences in construction and object. We have thought that some details concerning the working of this system in Paris might be useful and interesting at the present time, and we therefore give an abstract of some articles on the subject which have recently appeared in *La Nature*.

The question of the distribution of messages in the interior of towns has revived the systems of pneumatic transport, which, after having had their day of celebrity, seemed for twenty years doomed to oblivion.

In following the aspects of this question, we shall show in what way the atmospheric telegraph is a result of the electric telegraph; we shall afterwards consider the former more specially, and after having shown its present condition, shall inquire what future is in store for it.

The telegraphic despatch has become an article of every-day use; as the age is a fast one, it is natural that it should utilise with eagerness so handy a means of transmitting almost instantaneously its impressions or its wishes to all distances. It is necessary to remember that a city like London or Paris sends out and receives every day an immense number of telegrams. The wires which serve as conductors of electricity are multiplied in all directions for the purpose of meeting the demands of this traffic. They meet in the interior at the central office. This central station speaks *urbi et orbi;* in other words, it receives the messages of the city for the purpose of spreading them over the entire world, and it accomplishes also an inverse movement. The aspect with which we are here concerned is the distribution throughout the city itself; let us see what has been done in Paris to accomplish this purpose.

As each house cannot be put in immediate communication with the telegraphic network, it became necessary to adopt some other convenient plan. In the case of Paris, the city is divided into districts of a mean radius of 500 metres in order to limit the journeys of the foot-messengers. The application of this rule gave fifty points, distant one kilometre from each other, where are established so many branches of the chief office.

This system was found, however, not to work well, and was moreover very expensive, for reasons which we need not detail here; and after *voitures* were tried for some time as a means of sending despatches from the head office to the more important branches, it was resolved to have recourse to the pneumatic tube. We have just referred to the extent to which it has been carried in London. Paris and Berlin followed the example of London in 1865: we shall speak here of the system of Paris.

In Paris there are fifty stations, distant from each other about a kilometre, connected by an iron tube, which is interrupted at each station. The central station, by which the transit of messages is effected with the interior, is in the Rue de Grenelle; there are seventeen district stations, in the Rue Boissy-d'Anglas, Grand-Hôtel, Bourse, &c.

How is this network managed? Like a diminutive subterranean railway, in which the waggons are cylindrical boxes and the motive power compressed air prepared in the stations. At the central bureau the trains are formed, composed of as many boxes as there are branch offices to supply. The trains are *omnibus* when they stop at the intermediate stations, *express* when they shoot past them.

Every quarter of an hour an omnibus train leaves the Rue de Grenelle, and accomplishes the distance which separates it from the Rue Boissy-d'Anglas (1,500 metres) in a minute and a half. There it is received in a vertical column, and the box which carries the messages to be distributed in the district having been taken out, the others are put into the section of the line which runs towards the Grand Hotel, a new box having been added containing messages to be transmitted, which have been deposited since the last train. The train again takes its departure, composed of as many boxes as before; it goes through the same operations at the Grand Hotel, the Bourse, the Théâtre Français, and at the Rue des Saints-Pères. It re-enters the Rue de Grenelle twelve minutes after its departure, having changed all its boxes and carried back messages for distribution.

Besides this there is a secondary network, the details of which, however, we need not now enter upon. There is a direct line which goes from the Rue de Grenelle to the Bourse, and to branches in the Champs-Elysées, the Place du Havre, and the Rue des Halles. On the first run the express trains going and returning, the departures of which are intercalated between those of the omnibus trains, for the purpose of supplying those stations which are busiest, twice every quarter of an hour. The departure is accomplished by pressure, the return by aspiration. The same method of working is applied to the branches, which correspond with the omnibus trains of the principal network.

The tubes which compose the lines are of iron, the interior diameter being 0·065 metre. They are connected by bridle joints (*à brides*), and admit of curves having a radius of from 5 to 20 metres.

Various systems for the production of compressed or rarified air are employed. The first in date is an application of the principles of the apparatus known as Hiero's Fountain. Atmospheric air is decanted from a first receiver B (Fig. 1) into a second receiver communicating with the first by means of the tube *b b*, by the introduction of water into the receiver B. The air thus forced is drawn into the receiver for the purpose of being dispersed in the tubes. Where the machines are not allowed to be used, the employment of steam is much more economical for the compression of air. Recourse is then had to ordinary pumps, which insure an active service and are subject to fewer causes of irregularity. The latter method has been preferred in recent establishments.

Trains composed of ten boxes weigh about four kilograms, they are either pushed or sucked along by a difference of pressure of three-fourths of an atmosphere, which gives a mean speed of a kilometre per minute.

The travellers which take their places on the Lilliputian carriages already described are closed envelopes containing messages; they are piled in groups of thirty or forty in a *curseur*, or box. This box is formed of two cylinders, the interior one of sheet-iron, the outer one, enveloping the former, of leather. To make up a train, a piston must be affixed after the last box, for the purpose of enabling the compressed air to take effect. The piston is a piece of wood provided with a leather collarette, which assumes the shape of the interior of the tube, and forms an almost hermetical joint, without much friction.

The apparatus at first adopted for receiving and despatching the boxes having been found neither sufficiently rapid nor convenient, a much more complete system, shown in Fig. 2, is now employed. The figure explains itself: two lines enter the office, each attached to separate apparatus. In the first place, for the purpose of despatching messages, a man opens the door A by means of the lever *d*; the boxes and the piston are thrown into the tube, and await at the bottom the current of air which will propel them. This current is produced as soon as the cock *c* is opened, which commands the head of the apparatus opposite to the tube. The cock *c'* distributes the air upon the second line. In the second place, the receiving door B is opened by a second attendant, who finds the train at the station, and takes out the boxes in order to bring the telegrams to light. The entire apparatus has somewhat the form of a cannon, only the effect is more blessed, the artillerymen are not exposed to death;

the worst accident they have to fear is the bursting of the tube. To this drawback, which happens very rarely, we shall refer by-and-by.

The messages are divided into two classes,—questions and answers, orders and the execution of orders, which can at once be exchanged between any point of the city and any point of the interior, in the provinces, or abroad,—or inversely. All that is necessary in this case is a centre, as the Hôtel des Télégraphes in the Rue de Grenelle is called. Connected in the one part with the exterior by the network of electric wires, and with the interior by the network of pneumatic tubes. These tubes are, moreover, well adapted for the service of the local post, *i.e.* for the exchange of messages within the city. The great advantage in this case is that the despatch can be sent. On the plan adopted, when the network is complete, a letter may always be sent from one quarter to another at any distance within Paris in less than an hour. Every year the development of the lines increases, and the number of Paris telegrams meant for the city, and of which the originals themselves can be transmitted, is getting greater and greater.

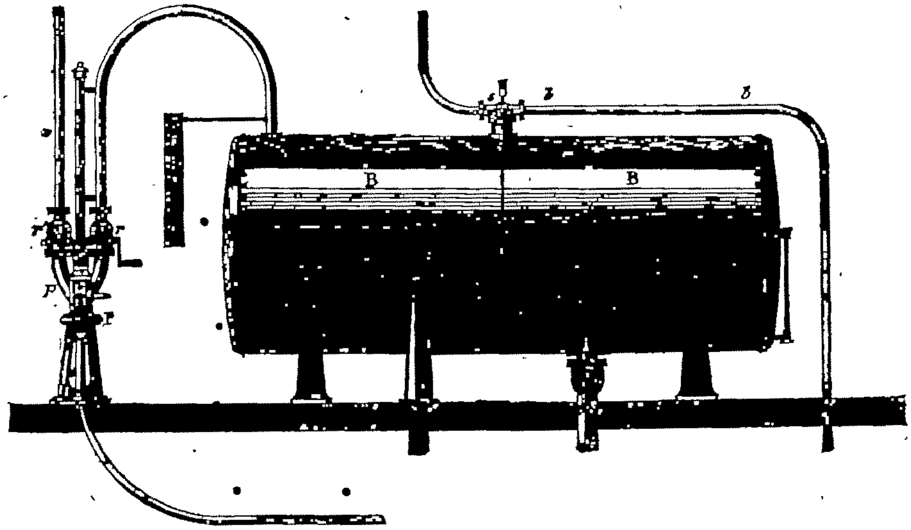

Fig. 1.—Apparatus for the production of compressed air.

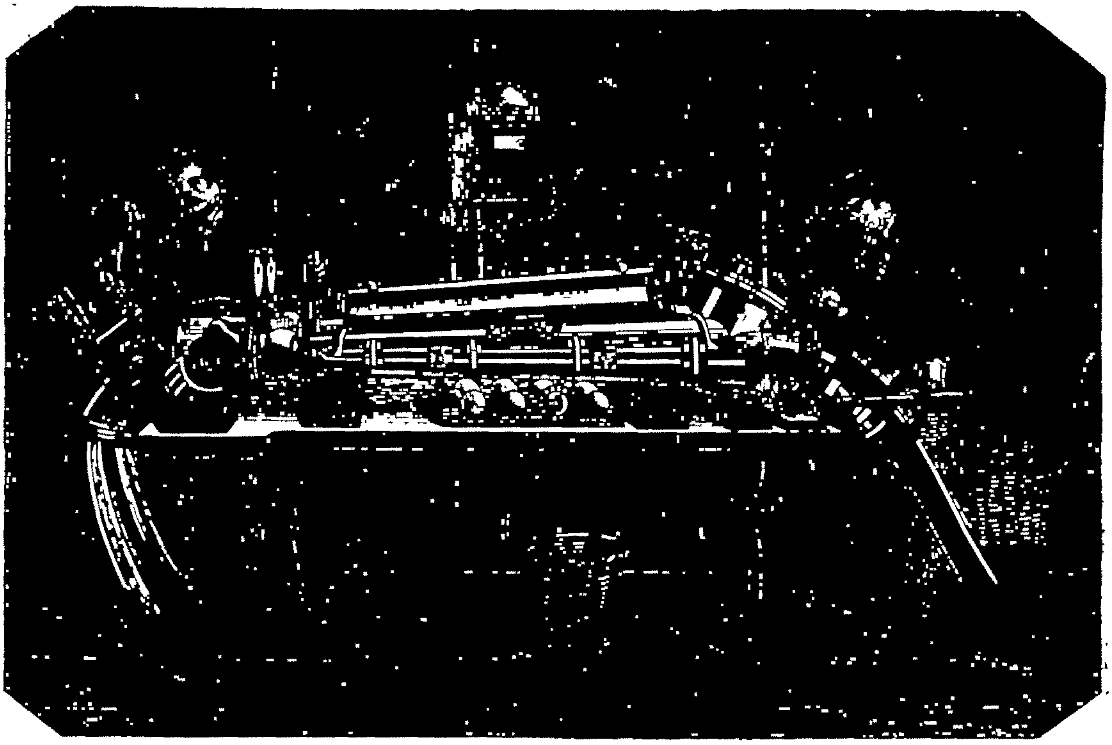

Fig. 2.—Apparatus for reception and despatch.

THE COMMON FROG*

V.

THE third order of the class *Batrachia* is made up of a few creatures the distribution of which is limited to the warmer regions of the earth, where one of the genera (*Cæcilia*) comprising the group is distributed over both hemispheres, being found in India, Africa, and South America. Two other genera (*Siphonops* and *Rhinatrema*) are exclusively American, while a fourth genus (*Epicrium*) is only found in Asia. The order is called *Ophiomorpha*. These creatures are singularly unlike the frog in external appearance, as they are entirely destitute of limbs and have quite the appearance of earthworms, because they are not only very long and slender, but have also a skin which is soft and naked. By earlier naturalists, and even by Cuvier, they were classed with snakes.

In spite of this striking dissimilarity between the *Ophiomorpha* and *Anoura*, the former are really more like frogs than they are like efts in one important respect. This is because, for all their elongated figure, the tail in them is quite rudimentary or altogether absent.

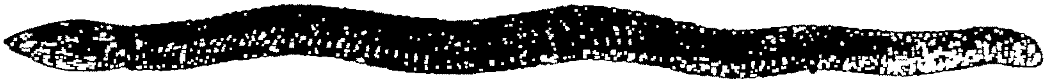

FIG. 22—*Cæcilia*.

The *Ophiomorpha* would by many be supposed to present an analogy with serpents, from their long and elongated bodies, and from the utter absence of limbs.

There are, however, but very few snakes (the "rough-tails" *Uropeltidæ* and the *Tortricidæ*) which have long bodies and very short tails.

It is rather the singular family of lizards, *Amphisbenidæ* (with one exception completely limbless) that the Ophiomorpha resemble.

FIG. 23.—Skeleton of the Icthyosaurus

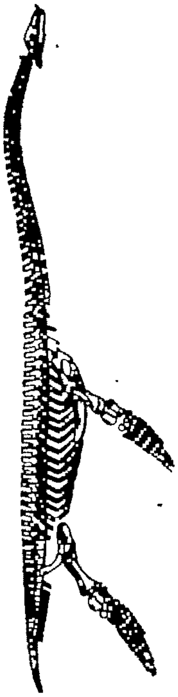

FIG. 24.—Skeleton of the Plesiosaurus.

These Amphisbenians have a softer skin than any other Saurians except chameleons. It is also marked in grooves which are arranged in transverse rings. They have an exceedingly short tail which is blunt, so that, the head being small, one end of the body is as large as the other.

The *Ophiomorpha* also have the body marked with numerous transverse grooves or rings; they are utterly devoid of limbs, and the head is scarcely, if at all, larger than the hinder end of the body.

These creatures burrow beneath the soil (which habit increases their resemblance to earth-worms) and feed on worms and other small animals and mould.

To turn now to another aspect of our subject, let us consider the relations of the Frog to past time. If, extending our survey over the records of past epochs, we search the tertiary and all other rocks above the Lias for fossil allies of our Frog, we shall (judging by what we yet know) fail to find any not at once referable to one or other of the three ordinal groups above enumerated.

Fossil frogs and toads have as yet only been found down to the miocene, the oldest being some found in the so-called "brown coal" which is not a carboniferous deposit at all. The remarkable thing, however, is that the difference between these oldest known Frogs and the existing forms is so very trifling. They are as complete and thorough frogs as any that live now.

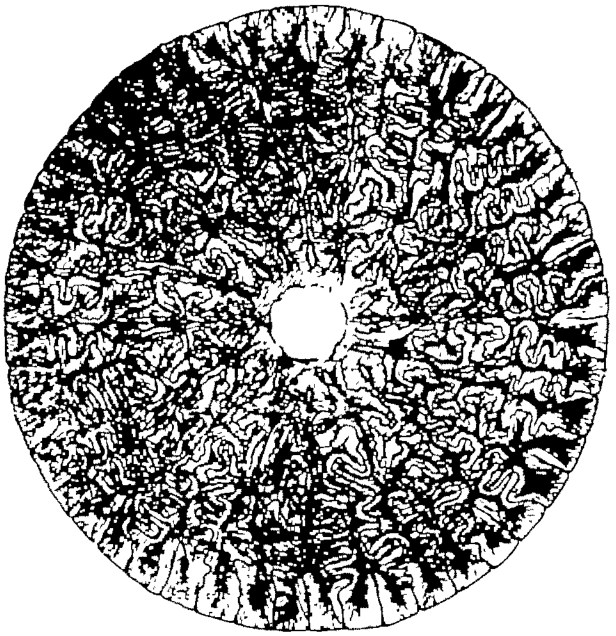

FIG 25.—Much enlarged horizontal section of the tooth of a Labyrinthodon

Again, the fossil Urodeles similarly resemble their existing representatives, and no one extinct species exhibits characters in any way tending to bridge over the chasm which separates the Urodela from the *Anoura*.

When, however, we descend to the Lias, Trias, and Carboniferous rocks, we come upon a rich variety of extinct species of animals evidently allied to those forming the three Batrachian classes already described. They form, however, an order by themselves, to which the term *Labyrinthodonta* has been applied, and thus our search into the past has brought us a rich and important harvest, and has introduced us to the fourth and last Order belonging to the frog's class of vertebrate animals. The Labyrinthodonts were creatures with long tails and mostly two pairs of limbs, but these members were always relatively small with slender toes. Some species attained a greater size by far than does any existing Urodele, even the gigantic Salamander.

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* Continued from p. 30.

To what existing animals can these huge monsters be considered to have affinity? It is impossible to say that they in any way bridge over the chasm separating the Frogs from the Efts. They appear indeed to have been almost equally removed from both—for the possession of short limbs and a long tail (characters common to so many widely different animals) cannot be regarded as any good evidence of affinity.

It is not improbable that they find their nearest allies in the existing insignificant *Ophiomorpha*. The latter, though apparently naked, have minute scales imbedded in the skin and arranged in rings at intervals, and the skull is provided with certain extra ossifications. The Labyrinthodonts have similar extra cranial ossifications, and though they have not rings of scales, the ventral region was protected by minute plates arranged in linear series converging inwards and forwards towards the middle line. Moreover, some forms appear to have been entirely devoid of limbs; at least no remnant of such parts has yet been discovered. Nevertheless the degree of development of the tail constitutes a marked distinction between the *Labyrinthodonta* and the *Ophiomorpha*.

Certain Labyrinthodonts had great formidable teeth in elongated jaws like those of crocodiles. Altogether these singular remains tempt us to speculate as to the succession of life upon this planet's surface. We know that as to the later secondary period that part in the life of the globe which is now played by beasts was then played by reptiles. Instead of the existing bats, Pterodactyles of all sizes flitted through the air. The ocean was peopled not by whales and dolphins, these had not yet appeared, but by huge Ichthyosauri and Plesiosauri. Reptiles of huge bulk (Iguanodons, Megalosauri, Notosauri, &c. &c.) fulfilled the parts of herbivorous and carnivorous beasts, and altogether the Mammalian fauna of to-day was represented by analogous reptilian precursors.

May it not have been similar in yet older periods with regard to animals of the Frog class? We have seen the possibility of aërial locomotion in even the existing *Rhacophorus*. It is true that all existing Urodeles are fresh-water forms, but it may well be that marine creatures once bore the same relation to them as the great marine *Ganoid* fish fauna bears to the few existing Ganoids[*] which now constitute a fresh-water group.

The great crocodile-like Labyrinthodonts must have been no ignoble predecessors of the rapacious reptiles which were to succeed them, and the fossil form *Ophiderpeton* suggests that the existing *Ophiomorpha* may be the last remnants of a race which preceded and represented the subsequently developed serpents.

This, however, is but a conjecture which future discoverers will probably ere long establish or refute.

The name *Labyrinthodonta* was bestowed upon the great fossil group on account of the beautiful and singularly complex structure of the teeth of some members of the order. These teeth are conical, and exhibit slight vertical grooves on their surface. A horizontal section shows that these surface-grooves are the external indications of deep indentations of the substance of the tooth. All these indentations converge towards the centre of the tooth, but not in straight lines, each indentation being elaborately inflected. Radiating from the centre of the tooth are a corresponding number of processes of the central pulp cavity—the radiating processes undulating like the converging folds.

A similar structure of tooth is found in some Ganoid fishes, and an incipient stage (as it were) of the same condition existed in the Ichthyosaurus.

We have now reviewed the closest as well as the more remote allies of our Frog, and have seen how the Frog is a species of a group (*Anoura*) which is one of three existing and widely divergent orders, supplemented by an extinct ordinal group of the carboniferous period—the four orders (1. *Anoura*, 2. *Urodela*, 3. *Ophiomorpha*, and 4. *Labyrinthodonta*) being embraced in a higher unity termed a "Class," which is the Frog's *class*, as "Anoura" is his *order*. This class is with propriety spoken of as the *Frog's class*, since the Frog is the species from which its scientific derivation BATRACHIA is derived. This class may now be considered as a whole.

The Batrachians (of all three existing orders) are in the main aquatic animals, inasmuch as the greater number, even when adult, frequent, at least at intervals, ponds and streams, or delight in humid localities. Water also is necessary for the larval stages of almost all; and absolutely all, at one period of life, possess gills, while some (as we have seen) retain gills during their whole existence, and are permanently and constantly inhabitants of water.

The extinct forms (*Labyrinthodonta*) were, no doubt, also aquatic, as, besides their general relation to other Batrachians, traces or indications of the hard parts which supported the branchiæ of some Labyrinthodonts appear to have been actually found.

It is somewhat singular that in spite of this predominating aquatic habit, all Batrachians, both living and fossil, appear to inhabit, and to have inhabited, fresh water only. No Batrachian of any period is yet known to have been marine. This is the more remarkable since the most nearly allied class, that of fishes, is much more rich in salt-water than in fresh-water forms; while even existing *Reptilia* have (in the true sea-snakes and in chelonians) representatives which inhabit the open ocean, while in the secondary geological period marine reptiles (*Ichthyosauri* and *Plesiosauri*) abounded.

Indeed, of all classes of vertebrate animals, this aquatic class (*Batrachia*) has the least to do with the ocean, for many birds, and a still greater number of Mammals (*e.g.* the whales and porpoises), are constant inhabitants of salt water. All the adult Batrachians feed on animal substances, generally small worms, insects, or slugs, and animals allied to slugs. The larger Frogs and Toads will, however, as has been said, devour vertebrate animals, such as mice and small reptiles and birds. The existing large, tailed Batrachians devour fishes. The extinct tailed Batrachians, in their adult condition, were also undoubtedly animal feeders, but they may, in their young state, have been vegetarians. At any rate the tadpoles of the existing *Urodela* will eat vegetable matter, and indeed probably sustain themselves mainly upon it.

In cold latitudes the Batrachia, like the Reptilia, go into the winter sleep called *hibernation*, as also do the hedgehogs and bats amongst Mammals.

The Frogs and Toads sometimes hide and shelter themselves by creeping into out-of-the-way holes and corners, but more generally they (as also the Newts) bury themselves in mud at the bottom of ponds and streams. In hot latitudes, some forms pass the dry season in a similar state of lethargic inactivity.

Many beasts, birds, and fishes, range in flocks. The Batrachians, however, usually wander about in a solitary manner, and only congregate in the breeding season. It is then that their vocal powers find utterance, though only in the Anourous order; the tailed Batrachians never make more than a very feeble sound.

As regards the geographical distribution of the whole class, the northern hemisphere, and especially the American portion of it, is the more richly furnished. Africa, India, and Australia, are the most poorly supplied on the whole, because, though possessing very many kinds of frogs and toads, the whole Eft order is unknown in those regions.

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[*] Existing Ganoids are the sturgeon, bony pike (Lepidosteus), mud-fish (Lepidosiren), and others as noticed earlier.

Our question "What is a Frog?" has now been somewhat further answered; but it cannot be completely so until the organisation of the animal has been more fully surveyed, and not only the relation of the frog to other Batrachians thus more clearly seen, but also the relations and affinities borne by the several orders of Batrachians and by the whole class to the other orders and other classes of the Vertebrate sub-kingdom.

Accordingly, we have now to make an acquaintance with more than those obvious and external characters which are found in the Frog, and to penetrate into its inner anatomy, surveying successively its bony framework and the various parts and organs which subserve the several actions necessary to its continued existence.

At the same time the more noteworthy resemblances presented by the Frog to other creatures will be pointed out. Thus we shall become acquainted with the relations existing first between the Frog and other members of its order; secondly, between the members of its order (*Anoura*) and its class fellows—*i.e.* other Batrachians; thirdly, we shall comprehend the degree of relationship existing between the Batrachia and the other classes of the Vertebrate sub-kingdom; and fourthly, we shall come to recognise certain singular resemblances which exist between the various groups of Batrachians (the Frog's order of course forming one), and some of the orders into which other vertebrate classes—especially the class of Reptiles—have been divided.

The skeleton of the Frog, both external and internal, naturally comes first as the support and foundation of the other structures. The internal skeleton (or *endo-skeleton*) will include the bones of the head, *i.e.* the skull, backbone (already referred to), and the bones of the limbs. The external skeleton (*exo-skeleton*) will consist of the skin only.

ST. GEORGE MIVART

(*To be continued.*)

ASTRONOMICAL ALMANACS*

V.—*The "Connaissance des Temps" under the continued direction of the old Academy*

LET us return to the *Connaissance des Temps* of the old Academy.

Jeaurat, who succeeded Lalande in 1775, adopted exactly the same principles as the latter; he, however, extended considerably the ephemerides of the moon, giving its declination for every six hours, to facilitate the calculation of the altitude, when at the same time only the distance could be observed. Méchain succeeded Jeaurat in 1788; he followed the example of his two predecessors, and like them, continued to take from the "Nautical Almanac" the distances of the moon, which Maskelyne had the kindness to send him even in manuscript.

Moreover, besides the ephemerides and the lunar distances, the *Connaissance des Temps* still contained observations, memoirs on various astronomical topics, an abridged notice of new books likely to be of interest to astronomers and navigators, and a brief history of astronomy during the past year, due to the skilful and well-informed pen of Lalande. This state of things continued until 1794, the year when Méchain left Paris, to take part in the meridian work. Soon after, the suppression of the academies having dispersed the astronomers, the *Connaissance des Temps* for 1795 was compiled and published by the temporary Commission of Weights and Measures. Finally, on June 25 of the same year, 1795, the publication of this work was placed under the eminent direction of the Bureau des Longitudes. Here we may conclude the first part of our account of the *Connaissance des Temps*—a work at first completely independent, then published with the approbation of the Academy, which included at the time nearly all those who were occupied with astronomy; and afterwards entrusted to the care of the Bureau des Longitudes, a commission which still continues to be charged with its publication.

VI. *The "Connaissance des Temps" under the Bureau des Longitudes*

The first care of the Bureau was to entrust one of its members with the publication and direction of the *Connaissance des Temps*, thus showing, from the first, the true course which ought to have been adopted from the beginning, that a work of this kind demands strictly personal superintendence. Its choice fell upon Lalande, then Astronomer of the Observatory of *l'Ecole Militaire.* As to the calculations, however, the superintendence of this astronomer was more nominal than real; he was occupied mainly with the *Additions* which he had commenced in 1760, and towards which the bent of his mind,—"more of a collector than an inventor"—carried him. Thanks to the great quantity of material which he had acquired, he made of these additions a work really useful, for at this time periodic scientific publications were very rare. His *Journal d'Astronomie* (history of astronomy during the preceding year), contains a mass of information of great value, even at the present day, to all who take an interest in the history of the science of astronomy.

As to the calculations, they were made partly by Bouvard, whom Laplace had appointed adjoint to the Bureau des Longitudes, and partly in the bureau of the *Cadastre*, under the direction of Prony, its chief. It was in the office of this celebrated engineer that the distances of the moon from the sun and from the principal stars were calculated, distances which ceased from that time to be taken from the *Nautical Almanac.* Let us, however, add, that up to the year 1806 the greater part of the other calculations of the *Connaissance des Temps* were drawn from the *Nautical Almanac*, "with the view," according to the preamble, "of accelerating the publication." Despite this assistance, nevertheless, this work appeared only about a year and a half or two years in advance; it was then, at that time, completely useless to navigators who had to make a long round. The attention of the Bureau des Longitudes was not however turned in this direction. Its president was then the illustrious Laplace, one of the glories of the mathematical sciences, and who first knew how to deduce from the great discovery of Newton, all the consequences which it was calculated to yield.

Pierre Simon Laplace was born March 23, 1749, of a family of poor farmers of Beaumont-en-Auge (Normandy, Calvados). It is not known where he got the elements of his education, for when later he was raised to the highest honours, he had the weakness to wish to conceal his humble origin. Appointed in 1770, on the recommendation of d'Alembert, Professor of Mathematics at *l'École militaire* of Paris, he became in 1772 adjoint member of the Academy of Sciences, next succeeded Bezout as examiner of the pupils of the royal corps of artillery, and in 1785 was made titular Academician. During this time, his beautiful memoirs on which he founded his *Mécanique céleste*, succeeded each other almost without interruption. Finally, in 1795, he was nominated president of the Bureau des Longitudes, a position which he held till his death, March 5, 1827.

Under his leadership the Bureau was occupied mainly in perfecting and re-constructing the tables, by means of which are calculated in advance the positions of the different stars. The tables of Delambre (the sun, Jupiter, Saturn, Uranus and the satellites of Jupiter, 1792), of Mayer (corrected by Mason, 1787), for the moon, of

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* Continued from vol. viii. p. 531.

Lalande for Venus and Mercury, showed with the observations very great errors which the theory of Laplace promised to eliminate, or at the very least to diminish. It was to the solution of these questions that Laplace directed the forces of the Bureau, and it was to their practical execution that he applied the resources which the budget granted him.

"To accelerate the work, the different parts were distributed to various members of the Bureau. The tables of the moon, on account of the constant use made of them in astronomy and navigation, were those which it was of special importance should be completed promptly; but the length of the researches, the magnitude of the calculations, which so complicated a theory required, only permitted the hope to be cherished that in the distant future errors might be made to disappear which had gone on increasing from day to day. This was the occasion of making an appeal to all astronomers, national and foreign, who might have sufficiently advanced works upon the lunar tables. With this object the Bureau des Longitudes was authorised to offer a prize." *

This prize of 8,000 francs was awarded by the Bureau to an astronomer of Vienna, Bürg, whose tables, based upon 2,500 observations, made at Greenwich from 1765 to 1795, were deemed the most accurate and convenient. At the same time, Delambre published new tables of the sun; Bouvárd, pupil of Laplace, whom he had assisted in the publication of the *Mécanique céleste* (Laplace resigned to him entirely the detailed investigations and astronomical calculations), published *Nouvelles Tables des planètes Jupiter et Saturne* (1808), a new edition of which he brought out in 1824, to which were added tables of Herschel's planet, Uranus; Delambre published his *Tables écliptiques des satellites de Jupiter* (according to the theory of Laplace and the totality of the observations made from 1662 to 1802); Burckhardt, a German astronomer, whom the conquests of Napoleon had given to France, published new *Tables de la lune* (1812), which, in the estimation of some astronomers, took the place of those of Bürg.

However, the impulse given by the splendid works of Laplace was not confined within the French frontiers. In Italy, a celebrated astronomer, Francisco Carline, published, in 1810, new tables of the sun, which were soon employed everywhere except in France.† In Germany, a man of Science, who was at one and the same time an eminent lawyer, a distinguished captain, and an excellent astronomer, Bernhard von Lindenau, published, according to Laplace's theory, tables of Venus, Mars, and Mercury.‡

Unfortunately these excellent works, due to the powerful initiative of Laplace, were not made use of in the publication of the *Connaissance des Temps*.

In 1808, Delambre, one of the most eminent French astronomers, undertook the direction of the *Connaissance des Temps*. No essential change was made in the work till 1817; at that time the right ascension of the moon, which had until then been calculated only to a minute, was given to a second for noon and midnight. Sailors could thus determine the longitude of their ships with more exactness; and astronomers, instead of finding in the *Connaissance des Temps* only the indication of the time at which they ought to observe our satellite, could thus compare the results of their observations with those which the tables gave, and prepare the material for their improvement. Finally, in 1820, were introduced the differences in right ascension and in declination of the sun, differences useful in calculating the preceding co-ordinates at an hour other than that of noon. This was still another advantage to sailors.

But these improvements were of very little consequence in comparison with those which astronomy, geography, and navigation demanded. Germany was the first to set an example in this direction, and the Royal Astronomical Society of London, after a long and learned discussion, came to the conclusion that they were necessary. Moreover, besides being incomplete, the *Connaissance des Temps* was full of errors from beginning to end, errata being found even among the errata themselves. Radical reforms were indispensable; but to make this clearly evident, we must return to the history of the "Nautical Almanac" and the Berlin "Jahrbuch."

(*To be continued.*)

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* Report of the Bureau des Longitudes, 1800.

† "Esposizione di un nuovo methodo di construire le Tavole Astronomiche applicate alle Tavole del Sole" (Milan, 1810).

‡ "Tabulæ Veneris novæ et correctæ ex theoria gravitatis, *clarissimi de Laplace*, et ex observationibus recentissimis in specula astronomica Seebergensi habitis erectæ" (Gotha, 1810) "Tabulæ Martis novæ et correctæ ex theoria gravitatis, *clarissimi de Laplace*, et ex observationibus recentissimis erectæ" (Eisenberg, 1811) "Investigatio nova orbitæ a mercurio circa soli descriptæ, accedunt Tabulæ Planetæ ex Elementis recens repertis et theoria gravitatis, *illustrissimi de Laplace* constructæ" (Gotha, 1813).

MAN IN THE SETTLE CAVE

UNTIL the appearance of Mr. Tiddeman's paper in NATURE, vol. ix. p. 14, I had not fully realised the important issues which, according to him, depend upon the proper identification of the fragment of bone from the Victoria Cave to which he refers; nor was I aware that he was about to commit me in such very absolute terms to the opinion that it was human, but of this, as it turns out, I have no reason to complain.

Looking, however, at the apparent gravity of the statement, and knowing, also, that opinions might, and as I believe did, differ as to the origin of the bone, I have been induced to go into the matter again, and am now in a position to affirm that there is no room for the slightest doubt on the subject.

Mr. James Flower, the excellent and estimable articulator to the College of Surgeons, to whom I am under many obligations for assistance in such questions, and who at one time suggested, and had almost convinced me, that the bone was elephantine, has, after much search, found amongst the Museum stores of human osteology, a *fibula* which places the question beyond all doubt, and fully confirms the opinion I had come to, especially after seeing the Mentone skeleton, that the Victoria relic, pre- or post-glacial as it may be, is human. It is further important as showing that bones of the same conformation may occasionally be met with at the present day.

GEO. BUSK

Harley Street, Nov. 14

NOTES

DR. A. DEW-SMITH and Francis M. Balfour of Trinity College, Cambridge, have been nominated by the Board of Natural Science Studies, in accordance with the grace of the Senate (May 1, 1873), to study at the Zoological Station at Naples under Dr. Dohrn, until the end of July 1874.

AT the General Monthly Meeting of the Royal Institution to be held on Monday first, a President will be elected in the room of the late Sir Henry Holland, Bart.

PROFESSOR TRAQUAIR, of the Royal College of Science in Dublin, has been appointed to the Keepership of the Natural History Museum in the Edinburgh Museum of Science and Art. This gentleman was formerly one of the Demonstrators to the Professor of Biology in the University of Edinburgh, and is the author of several important contributions to Science.

MR. W. F. BARRETT, F.C.S., has been appointed Professor of Physics to the Royal College of Science, Dublin, in succession to the late Professor W. Barker. We feel sure that this appoint-

-ment will give great satisfaction. Sir Robert Kane, F.R.S., having resigned the post of Dean of Faculty to the College, for the purpose of spending his winters in the south of Europe, Professor Galloway has been selected to fill this post. It is said that there either are, or will very shortly be, a vacancy in the Professorship of Chemistry owing to Professor Sullivan's appointment to the Presidentship of the Queen's College, Cork.

Dr. E. H Bennett has been elected Professor of Surgery in the University of Dublin, in succession to the late Dr. R. W. Smith; and Dr. Thos. E. Little has been elected to fill the post of University Anatomist. In connection with news from the Dublin University, we may mention that it is understood that the authorities have determined to build a new museum for their anatomical and zoological collections. At present, in connection with the Medical School, there is a small collection of human and comparative anatomy, and, in the Arts' School a very good collection of zoology. It is intended to combine these two in a new building. The College authorities would confer a great boon on natural science in Dublin if they would venture to go a step further and make their new museum contain all their biological collections. The advantages would be great of having the distribution of animals in space and time shown in connection the one with the other; and there is something incongruous in separating the specimens illustrating the past and present races of mankind from the zoological collection, and combining the specimens illustrating the anatomy and physiology of the human species with those illustrative of the other animals. For the convenience of the students, we trust that the extensive herbarium of the College may also be lodged under the roof of the new building, which, to be useful, need contain no lofty halls or grand corridors, but should consist of a series of well-lit rooms, after the fashion of, we would suggest, that nicest of museums, the one for Economic Botany at Kew.

The following memorandum on the Whitworth Scholarships, prepared by Sir Joseph Whitworth, has been approved by the Lords of the Committee of the Council on Education:—"I wish that candidates for my Scholarships in 1874, who, owing to the shortness of the notice, may not have been able to be in a mechanical shop for six months before the competition takes place, should be allowed to compete, but that if successful, their scholarship should not begin until they have worked six months in a mechanical shop. I think the same privilege should be accorded to candidates in 1875, who have not served eighteen months in a mechanical shop, the scholarship not beginning until this period is completed."

The 120th session of the Society of Arts was opened on the 19th inst. with an address by the Chairman of the Council, Major-General F. Eardley-Wilmot, F.R.S.

The *magnum opus* of three generations of botanists, De Candolle's "Prodromus Systematis naturalis vegetabilium," containing a diagnosis of every known species of flowering plant, has now been completed as far as Dicotyledons are concerned, and it is not intended to continue the work into the Monocotyledons. In commemoration of the completion of the work, the Horticultural Society of Belgium has awarded M. de Candolle a special medal. The publication of the work was commenced in 1818.

The trustees of the Gilchrist Educational Fund offer a scholarship of the value of 50*l.* per annum, tenable for three years at Girton College, Cambridge, to be competed for at the General Examination for Women, conducted by the University of London in May, 1874.

From the commencement of next year, *The Gardeners' Chronicle and Agricultural Gazette* will be divided into two papers, each weekly, to be devoted to the interests of the two sister sciences.

Dr. William Wallace, in opening recently the session of the Chemical Section of the Philosophical Society of Glasgow, spoke, among other things, of the endowment of research. From what he said on this subject, we think the following pointed remarks worthy of attention:—With regard to students who attended evening lectures and classes, a very great deal had been done for them by the Society of Arts, and by the examiners of the Science and Art Department, both of which had given great encouragement to the class of students whom they were intended to benefit. What was lacked most was a stimulus to men of the highest educational class. In this country, apart from professorships, there were no means of assisting that class except, perhaps, a few sinecures and the conferring of empty titles. In France, at least under the Imperial *régime*, when a man acquired renown in a particular line of investigation, a laboratory with all the best and most suitable appliances was immediately fitted up for him. Hence Paris was provided with a series of the most complete laboratories for metallurgy, for agriculture, for the sugar manufacture, and for many other branches of the science; and students might go to study a particular subject with the certainty that they would have a most efficient teacher and the advantages of a laboratory fitted up specially, and, as one might say, regardless of expense, with the apparatus and requirements necessary for the teaching and study of the subject. It appeared to him (Dr. Wallace) that the endowment of research would form a desirable stimulus for chemists, many of whom had the necessary education and talent, but could not afford the time nor the expense, often considerable, of obtaining the apparatus and materials required.

A Society of Physical and Natural Science was founded four years ago at Caracas, Venezuela; but the political agitations of the country have, until recently, hindered its development. Meanwhile it has commenced the publication of a Bulletin under the title of *Vargasia*, so named in honour of the American botanist Vargas. *L'Institut* learns, by a letter from Dr. Ernst, who is at once president, secretary, and treasurer of the society, that the present Government of Venezuela intends to promote, as much as it can, the growth of scientific studies, mainly by the establishment of various institutions for public instruction. Dr. Ernst, appointed Professor of Botany in the University of Caracas, where hitherto there has been no such chair, has been charged with the direction or rather the creation of a botanic garden and a museum of natural history. In the museum Prof. Ernst intends to collect—1st, a herbarium of Venezuela; 2nd, a general herbarium; 3rd, a collection relating to economic botany. He intends to publish in a few years a Flora of Caracas. Dr. Ernst appeals to European botanists and collectors for exchanges to assist him in the formation of these herbaria.

It is not often that Mr. Disraeli says anything which calls for particular notice in a journal of this kind, therefore it is with peculiar pleasure that we quote the opinion he uttered last week at the Glasgow banquet as to the share which Science has had during the present century in moulding the world. Coming from a man of his shrewdness and sentimentality withal, the words have a striking force. Speaking of the last fifty years, he said:—"How much has happened in these fifty years—a period more remarkable than any, I will venture to say, in the annals of mankind. I am not thinking of the rise and fall of empires, the change of dynasties, the establishment of Governments. I am thinking of those revolutions of science which have had much more effect than any political causes, which have changed the position and prospects of mankind more than all the conquests and all the codes and all the legislators that ever lived."

At the first meeting of the Edinburgh Botanical Society for the winter, Mr. James McNab, curator of the Royal Botanical Gardens, delivered an address on the change of climate in Scotland, which, during the last fifty years has undergone a considerable lessening of the summer heat. From this cause peaches and nectarines cannot be ripened to the same perfection in the open air as formerly, while asparagus, mushrooms, and tomatoes are gradually disappearing. The larch, in spite of the enormous quantities of seed annually imported, is declining in vigour, and there is a talk of substituting for is the Wellingtonia as a nurse-tree. Mr. McNab proposes that a central committee should be appointed to investigate the whole subject of the change of climate in Scotland.

THE following is an ephemeris (for 0^h Berlin time) of the comet discovered by M. Coggia at Marseilles, on the evening of the 10th inst:—November 22, $14^h\ 51^m\ 25^s$—$6^\circ\ 8'\cdot2$; November 30, $14^h\ 14^m\ 30^s$—$22^\circ\ 43'\cdot0$; December 8, $14^h\ 0^m\ 17^s$ —$32^\circ\ 1'.8$. Its elements are:—T=Dec., 4·1348, Berlin mean time; $\pi = 94^\circ\ 23'\ 14''$; ☊ $= 254^\circ\ 14'\ 9''$; $i = 27^\circ\ 2'\ 7''$. Mean Equinox, 1873·0 *log. q.* = 9·83810.

ONE of the special results of the United States geological and geographical survey of the Territories, in charge of Prof. F. V. Hayden, during the past summer, has been the discovery that Colorado Territory is the centre of the greatest elevation of the Rocky Mountain chain. In Central Colorado the chain proper is about 120 miles broad, made up of three lofty parallel ranges, running nearly north-north-west, and flanked from the west by great plateaus and groups of peaks. Between the ranges lie the great elevated basins known as "parks." The front range, which rises abruptly from the plains, is seen from Denver in a grand panorama 120 miles long. From its snowy serrated crest rise many peaks between 13,000 and 14,000 ft. high. On the west side of the parks is the Park Range, whose highest group is at Mount Lincoln, this and Quandary Peak each rising to about 14,000 ft. The survey has established a permanent meteorological station at Fairplay, 10,000 ft. above the sea, and another at Cañon City, about 6,000 ft. These stations are all connected by a spirit-level line, and the comparison of their observations will be of remarkable interest. The National Range lies east of the Park Range, and is separated from it by the Arkansas Valley. West of the National Range rises the great group of Elk Mountains, five of whose peaks are 14,000 ft. high. So far as known, there are in the district explored during the past season by the survey 72 peaks, ranging from 14,000 to 14,200 ft. in height.

IN the article on Local Societies (vol. ix. p. 24) we inadvertently confounded the Manchester Natural History Society with the Microscopical and Natural History section of the Manchester Literary and Philosophical Society. The former of these is extinct—having handed its collections over to the Owens College—and also contributed a handsome sum of money to promote, permanently, the study of Natural History in the Literary and Philosophical Society. This endowment now enriches the Natural History section of that society. Manchester science will gain rather than lose by these changes. The defunct society was never more than the creator and guardian of a museum. That museum will still be preserved and increased, as well as utilised, by the College, whilst the Natural History section affords promise of a healthy career of scientific work.

M. CHARREL, of Marseilles, writes as follows to the *Bulletin International*, of the Observatory of Paris, on the invention of balloons:—In the literary history of the City of Lyons, published by Father Colonnia (1830, vol. i. p. 112), it is stated that in the reign of Louis le Debonnaire, the Archbishop of Lyons learned that some aerial navigators had fallen with their boat on the banks of the Saone, and were going to be put to death as sorcerers. He ordered them to be brought into his presence, and after having heard them, he caused them to be nonsuited (*le fit mettre hors de procès*). The memoir of the prelate bears such a character of authenticity as leaves no doubt of the fact. The following words are taken verbatim from the memoir: "Videmus exhibere vinctos quatuor *homines*; tres viros et unam feminam; quasi qui de ipsis navibus ceciderunt, quos . . . exhibuerunt in nostra presentia tanquam lapidandos." It follows, then, from this memoir, that already, in the ninth century, aerial navigation was known; how it was accomplished the memoir does not give any indication.

THE first Annual Exhibition of the West London Entomological Society, established 1868, will be held on December 2 and 3, at the "Mason's Arms," Tichborne Street, Edgware Road.

A *Times* telegram from Teheran, November 24, says that Colonel Baker and Lieutenant Gill have arrived at Teheran, and leave immediately for England, *viâ* Tabreez and Erivan. Travelling to the north from Meshed, they passed along the Turcoman frontier by Kelat, Abiverd, Dereguez, Annan, Astrabad, and Nissa. Striking south, they discovered the source of the Attrek at Karakazan, an extraordinary spring near Shirvan, and followed the course of the river a considerable distance north-west of Bojnoord, until stopped by hostilities between Bojnoord and the Turcomans. Striking into the mountains, they were enabled to trace the course of the river until it fell into the plains, and also to observe the great range of mountains which runs along the whole Persian frontier from Sarakhs to Kizil Arvat. Existing conjectural maps of this country are quite incorrect.

ON the 1st inst. the Earl of Dalhousie formally opened the Art Exhibition and Museum of the Albert Institute of Dundee, which, with the previously opened portions—free library and lecture-hall—form one handsome block of buildings. In the list of towns, with their scientific societies, published by us a week or two ago, we were surprised to see Dundee, so rapidly advancing in population and wealth, occupy so humble a place. We cannot see how towns like Leeds, Newcastle, Manchester, Glasgow, and others should have their flourishing and well-equipped scientific societies, while Dundee has only one small struggling society of young men, the Naturalists' Field Club. The neighbouring and comparatively stagnant town of Perth, with its large and efficient society, puts Dundee to the blush in this respect. We shall be disappointed if the opening of the Albert Institute in Dundee, a town so dependent for its commercial and manufacturing success on the applied results of Science, does not give an impetus to the study of Science. There are already Science and Art Classes in the town, and we hope to hear soon of the establishment of regular courses of scientific lectures, such as those which are found in several of the large English manufacturing towns, and the formation of at least one flourishing scientific society and field club around the small nucleus already existing. We hope also that the collections in the museum will be made worthy of the wealthy town and be really representative of the treasures of the various kingdoms of Nature. We feel sure that the citizens of Dundee only need their attention to be drawn to the backward state of their town in the matter referred to to rouse them to put it on a level in this respect with the large English towns.

THE additions to the Zoological Society's collection during the last week include an Eagle (*Spizaetus ?*) from Burmah, presented by Mr. H. Fielder; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Gore; a pair of Jaguars (*Felis onça*) born in the Jardin des Plantes, Paris, received in exchange.

ON SOME RECENT RESULTS WITH THE TOWING NET ON THE SOUTH COAST OF IRELAND *

1.—*Mitraria*

ONLY a single specimen was obtained of the little *Mitraria*, which formed the subject of the present communication, and neither its structure nor development was made out as completely as could have been wished. From the Mediterranean species described in a former communication (British Association Report for 1872), it differs in some points of structure and in the mode of annulation of the developing worm. It possesses the usual *Mitraria* form, that of a hemispherical dome having its base encircled by a band of long vibratile cilia. In the side of the dome a little above the ciliated band is the mouth which leads into a rather wide pharynx clothed with a ciliated epithelium. The pharynx runs through the dome parallel to its base and opens into a capacious stomach which continues in the same direction until it joins the intestine. This then turns down abruptly at right angles to the previous portion of the alimentary canal, and then projects for a slight distance beyond the base of the dome, carrying with it hernia-like the walls of the base.

The true body walls of the future worm, of which the *Mitraria* is the larva, seem as yet confined to the intestinal segment of the alimentary canal. They already present the commencement of annulation, which, however, exists only on the dorsal and ventral sides, while two broad bands of very distinct fibres may be seen, one on the right and the other on the left side, extending transversely from the dorsal to the ventral surface.

The ciliated band which runs round the base of the dome possesses a rather complex structure. It consists of two concentric rings: an outer one composed of large oval distinctly nucleated cells, and an inner one of a granular structure and yellowish colour, in which no distinct cells could be demonstrated. The cilia form two concentric wreaths borne by the under side of the band, an outer wreath consisting of very long cilia, and borne by the inner edge of the outer portion of the band, and an inner wreath of much shorter cilia borne by the inner edge of the inner portion. The band with its cilia is interrupted for a very short space at the aboral side of the dome. There is probably at this spot an entrance into a water-vascular system. No such system, however, was observed in the specimen, though the author had described in another species of *Mitraria* a system of sinuses which appear to exist in the walls of the dome, and which he regarded as representing a water-vascular system (Brit. Assoc. Report for 1872).

Occupying the very summit of the dome is a large, somewhat quadrilateral ganglion, from which two distinct filaments are sent down, one on each side of the alimentary canal, but he was not able to follow these filaments to their destination. The bilateral symmetry of the ganglion suggests its formation out of two lateral halves. Though its very superficial position gives it the appearance of being a mere thickening of the walls, the view here taken of its being a nervous ganglion seems to be the only one consistent with its relations to the surrounding parts.

On each side of the pharynx, a little behind the mouth, is a small oval ganglion-like body from which a filament runs to the ciliated band. Some delicate filaments may also be seen lying between the pharynx and the walls of the dome on which they seem to be distributed, but the author could not trace them to any distinct ganglionic centre.

The great apical ganglion carries two very obvious black ocelliform spots, and besides these two clear vesicles enclosing each a clear spherical corpuscle. The two vesicles may probably be regarded as auditory capsules.

The further development of this larval form has not been observed. It probably consists chiefly in the continued prolongation of the alimentary canal beyond the base of the hemispherical dome, the completion of the annulation by its extension to the right and left sides, and the gradual contraction of the dome and final absorption of the ciliated band.

2.—*Tornaria*

Two specimens of the larval form originally discovered by Johann Muller, and described by him under the name of *Tornaria*, were obtained, but these unfortunately perished before a sufficiently exhaustive examination of them could be made. On the whole their structure agrees closely with what has been pointed out by Alex. Agassiz in his valuable and elaborate memoir on *Tornaria* and *Balanoglossus*. The species appears to be different from those hitherto described. The gills had not begun to show themselves, and there were but traces of the "lappets" described in other species as appended to the posterior extremity of the stomach.

The author believed that he could distinguish a minute ganglion on each side of the œsophagus; filaments were sent off from it to the neighbouring parts, and the two were connected to one another by a sub-œsophagal commisure. The water-vascular chamber was very distinct, but the so-called heart was not observed; while within the body-cavity, lying close to the dorsal pore and over the canal by which the great water-sac communicates with the external medium, was a small, closed, rather thick-walled vesicle containing numerous oval corpuscles. Of the nature of this vesicle the author could not offer any opinion.

The cushion-like body which occupies the summit of the larva exactly as in *Mitraria*, and supports the two ocelliform spots, was very distinct, and so also was the contractile chord which extends from this to the walls of the water-sac. The author, however, could not here, any more than in *Mitraria*, regard the cushion-like body as a mere thickening of the walls; he believed it to be a nerve-mass, and thought he could trace two fine filaments proceeding from it and running down, one towards the right and the other towards the left side of the alimentary canal, but he was not able to follow them for any distance, and he does not regard their existence as confirmed. The extremely superficial situation of this body, which makes it resemble a mere thickness of the walls, is paralleled by that of the great ventral nerve-mass in *Sagitta*.

The contractile chord which runs to the water-sac is probably attached to a capsular covering of the ganglion, rather than directly to the ganglion itself. This chord, though showing strong contractions by which the summit of the larva is drawn down towards the water-sac, is of a homogeneous structure, presenting no appearance of distinct fibrillæ or of other contractile elements.

The author instituted a comparison between *Tornaria* and *Mitraria*. We have in both the external transparent pyramidal or dome-shaped body, with a lateral oral orifice, and a basal anal orifice, enclosing an alimentary canal which is divisible into three regions, and takes a partly horizontal and partly vertical direction in its course from one orifice to the other;* we have in both, near the base of the body, the circular band which carries long vibratile cilia accompanied by a row of pigment spots, and in both the cushion-like ganglion carrying ocelli.

From *Mitraria*, *Tornaria* chiefly differs in the presence of the thick sinuous and convoluted bands which give it so close a resemblance to certain Echinoderm larvæ, and which are entirely absent from *Mitraria*, and in its water-vascular system with the contractile cord which extends from this to the apical ganglion. If a water-vascular system is present in *Mitraria*, it consists there of a system of sinuses excavated in the walls of the dome, but without any representative of the great central sac. In *Mitraria* the great apical ganglion carries not only the two ocelli, but also two capsules, probably auditory; these capsules do not exist in *Tornaria*. In *Mitraria* the two nerve chords which the apical ganglion sends down one on each side of the alimentary canal are very distinct; in *Tornaria*, if they exist at all, they are by no means obvious. Finally, the ciliary circlet is simple in *Tornaria*, while in *Mitraria* it is double.

According to Alexander Agassiz's account of the development of *Tornaria* into *Balanoglossus*, the great transverse circlet of cilia becomes, by the elongation of the body, gradually pushed backwards, so as to form the anal ciliated ring of the young worm; in *Mitraria* the great ciliary circlet remains unchanged in position, and is probably ultimately absorbed, the worm during its development acquiring a new anal wreath of cilia.

3. *Ametrangia hemisphærica* (nov. gen. et spec.)

Among the most abundant products of the towing-net was a little hydroid medusa, remarkable for the want of symmetry in the distribution of its gastro-vascular canals. It is of a hemispherical form, with the base about half-an-inch in diameter, and provided with very numerous (more than 100) marginal tentacles, which are very extensile, and may at one time be seen floating away to a length of three or four inches and at another coiled into a close spiral against the margin of the umbrella.

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* Paper read at the Meeting of the British Association, Bradford by Prof. Allman, F.R.S.

* In the species of *Mitraria* described by J. Müller and by Metschnikoff, both oral and anal orifices are basal, and the alimentary canal presents a U-shaped curvature

Each tentacle originates in a bulbous base with a distinct ocellus. No lithocysts are visible on the margin. The velum is of moderate width.

The manubrium forms a small projection from the summit of the umbrella, and terminates in four rather indistinct lips. From the base of the manubrium three rather wide offsets are sent off at equal intervals into the walls of the umbrella. These gradually contract in diameter, and then, as three narrow tubes of uniform diameter, run towards the margin, where they open into the circular canal. The symmetry of the radiating canals is confined to these three primary trunks. From their wide proximal ends each sends off branches, some of which may be traced to the margin where, like the three primary canals, they enter the circular canal, while others can be followed for various distances in the umbrella walls, in which they terminate by blind extremities without ever reaching the margin. These branches are very irregular in the number sent off from each primary canal, as well as in their length and directions.

The generative elements are formed in oval sporosacs developed one on each of the three primary canals at the spot where the wider base passes into its narrower continuation. The ova may be seen within them in various stages of development; they increase considerably in size before the commencement of segmentation, always showing up to that period a large and distinct germinal vesicle with germinal spot and with a distinct nucleolus in the interior of the germinal spot. The development of the ovum proceeds within the sporosac to the segmentation of the vitellus and the formation of the planula, which now breaks through the outer walls of the sporosac and remains for some time adhering to their external surface. The planula differs remarkably from the typical hydroid planula. It remains of a nearly spherical form, never acquiring cilia, and possesses little or no power of locomotion. The gastric cavity, however, is fully formed. The author was unable to follow the ova in their further development.

The little medusa now described, departs in several important points from the typical hydroid medusa. From this it differs in the ternary disposition of the primary radiating canals, and in the irregular non-symmetrical arrangement of those which are subsequently formed. Among the very many specimens examined, the author never found any in which the canals had become regular in their disposition, even in those which had discharged the contents of their sporosacs, and had evidently attained the term of their existence. It differs also from the typical medusa in the form and non-ciliated condition of the planula; and still further in the fact that while the generative elements are borne on sporosacs, developed on the radiating canals, the marginal bodies are ocelli and not lithocyst.

4.—*Circe invaginans* (nov. spec.)

Among the hydroid medusæ captured in the towing-net, were two or three specimens of a species referable to the genus *Circe* of Mertens. It measures about half-an-inch in its vertical diameter, and about a quarter of an inch transversely. It is cylindrical from its base upwards, for about two-thirds of its height, and then contracts abruptly, and arches dome-like towards the truncated summit, which is surmounted by a solid cone of the gelatinous umbrella substance. From the summit of the umbrella-cavity, a solid somewhat fusiform extension of the roof hangs down in the axis of its cavity for about two-thirds of its depth, and at its free end carries the manubrium, which extends nearly to the codonostome. The margin of the umbrella carried eighty very short and but slightly extensile tentacles, which were connected at their bases by a very narrow membraneous extension of the margin, with rather irregular free-edge. Lithocysts are situated at irregular intervals upon the margin. There are about sixteen of them; they consist each of a minute spherical vesicle with a single large spherical concretion. There are no ocelli. There is a moderately wide velum.

The radiating canals are eight in number. They spring from the base of the manubrium, run up the sides of the solid process which hangs from the summit of the umbrella; pass from this to the walls of the umbrella, and then run down towards the margin in order to open into the circular canal.

The generative elements are borne in pendent sporosacs, which spring from the radiating canals close to the summit of the umbrella cavity.

The motion of the medusa takes place by means of sudden jerks, reminding us of the way in which certain Diphydæ dart through the water.

The medusa possesses also a very singular habit of partial inversion. This takes place along the line which separates the dome-like portion of the umbrella cavity from the lower cylindrical portion, and consists in the withdrawal of this dome-like summit and the lower portion of the cavity. When thus inverted the little animal presents a drum-shaped form, with the manubrium hanging far out of the codonostome.

Alexander Agassiz considers the genus *Circe*, of Mertens, as synonymous with *Trachynema* Gegenbaur, and points out that the name of *Circe* had been already used for a genus of mollusca. He further removes it from among the true hydroid medusæ, and regarding it as closely allied to the *Æginidæ*, places it along with those in the *Haplostomeæ* Agassiz, a sub-order of the *Discophora*.

The author, however, could not see sufficient grounds for the removal of Mertens' genus from the true *Hydroida*, with which the medusa now described agrees in all essential points, including the form and disposition of the gastro-vascular and generative systems and the structure of the marginal lithocysts. Neither could he agree with Alexander Agassiz in identifying it with *Trachynema*. The greatly developed solid peduncle by which the manubrium in *Circe* is suspended from the summit of the umbrella-cavity in a way, however, which has its parallel in *Tima* among others, is of itself a character of generic importance by which *Circe* must be kept apart from *Trachynema*. It is true that Gegenbaur's *Trachynema* has the character of a young form, and until we have further evidence of its adult state its affinities cannot be regarded as established.

Gegenbaur believes that he has established the direct development of *Trachynema* from the egg without the intervention of a hydriform trophosome, but unfortunately we have no data by which to compare in this respect *Circe* with *Trachynema*.

It must be admitted too that in the imperfect contractility of the marginal tentacles and in the somewhat greater firmness of the umbrella walls the little medusa described in the present communication possesses characters which look towards the *Æginidæ*, but these are by no means sufficiently strong to justify its separation from the ordinary hydroid medusæ.

5.—*Tomopteris*

A few young specimens of this beautiful little worm were obtained, and the author was enabled to confirm the statements of Grube and of Keferstein, who describe in it a double ventral nerve chord, though other observers have failed to discover this part of the nervous system and throw doubt upon its existence. In adult specimens examined some years previously by the author no ventral chord could be detected.

The ventral portion of the nervous system consists of two flat ribbon-shaped chords which are given off from the inferior side of the nerve ring which surrounds the pharynx just behind the mouth. These run parallel to one another, separated by a narrow interval; they lie on the ventral walls of the animal, and may be traced through the narrow tail-like termination of the body as far as its extremity. They present no ganglionic swellings, but opposite to every pair of feet each sends off a filament which passes to the foot of its own side in which it is distributed.

Dr. Anton Dohrn has just informed the author that he too had distinctly seen the ventral chord of *Tomopteris*.

SCIENTIFIC SERIALS

AMONGST the papers in the October and November numbers of the *American Naturalist*, are included Dr. J. L. Smith's Address to the American Association for the Advancement of Science, on Science in America and Modern Methods of Science.—Mr. R. Ridgway describes some new forms of American Birds, which he considers as geographical races, and not distinct species. Included are *Catherpes mexicanus*, var. *conspersus*; *Helminthophaga celata*, var. *lutescens*; *Dendroica vieillotii*, var. *bryanti*; *D. dominica*, var. *albilora*; *D. graciæ*, var. *decora*; *Myiodioctes pusillus*, var. *pileolata* (Pallas), and *Collurio ludovicianus*, var. *robustus* (Baird), which are described and followed by a synopsis of the genera of *Certhiola*, *Junco*, and *Cardinalis*.—Prof. C. A. Riley has a paper on the Oviposition of the Yucca Moth, in which he shows that the female conveys her eggs into the young fruit by a lateral puncture. The Structure and Growth of Domesticated Animals, forms the subject of a popular lecture by Prof. Agassiz, which is followed by one on Staurolite

Crystals and Green Mountain Gneisses of the Silurian Age, by Prof. Dana.—The Rev. D. T. Hill gives instances of intelligence in *Bufo americanus*.—Mr. G. W. Morehouse analyses the structure of the scales of *Lepisma saccharina*.—Mr. D. Scott gives a popular explanation of the differences between the two genera of North American Goatsuckers, the Whippoorwills (*Antrostomus*), and the Nighthawks (*Chordeiles*), which is followed by a short note from Mr. Packard, jun., on the Embryology of Limulus, with remarks on its affinities. His results are confirmatory of those of M. Alphonse Milne-Edwards.

The fourth and concluding part of vol. xxviii. of the *Transactions of the Linnean Society*, is chiefly occupied by a supplementary paper by the Rev. O. P. Cambridge, on New and Rare British Spiders; but also contains some short papers of importance.—Prof. Oliver describes a new genus of Begoniaceæ from New Granada, under the name of *Begoniella*, a monotypic genus of great interest as respects the geographical distribution of the order; and three new genera of Malayan plants from the herbarium of Dr. Maingay—*Pteleocarpa* and *Ctenolophon* (Olacineæ), and *Maingaya* (Hamamelideæ).—Dr. M'Nab publishes his important paper on the Development of the flowers of *Welwitschia mirabilis*. Dr. M'Nab considers that in the male flowers of this very remarkable plant we have a very close approach to the Angiosperms, the axis of the flower ending in a mass of tissue which, in the female flower, is the terminal ovule; while, in the female flower, we have the truly gymnospermous condition, there being no carpels, but a terminal ovule, the modified end of the axis of the flower, with a single ovular integument, the pollen grains being applied directly to the naked nucleus.

SOCIETIES AND ACADEMIES

Royal Society, Nov. 20.—"Note on the Electrical Phenomena which accompany irritation of the leaf of *Dionæa muscipula*," by Dr J. Burdon Sanderson, F.R.S.

1. When the opposite ends of a living leaf of *Dionæa* are placed on non-polarisable electrodes in metallic connection with each other, and a Thomson's reflecting galvanometer of high resistance is introduced into the circuit thus formed, a deflection is observed which indicates the existence of a current from the proximal to the distal end of the leaf. This current I call the *normal leaf-current*. If, instead of the leaf, the leaf-stalk is placed on the electrodes (the leaf remaining united to it) in such a way that the extreme end of the stalk rests on one electrode and a part of the stalk at a certain distance from the leaf on the other, a current is indicated which is opposed to that in the leaf. This I call the *stalk-current*. To demonstrate these two currents, it is not necessary to expose any cut surface to the electrodes.

2. In a leaf with the petiole attached, the strength of the current is determined by the length of the petiole cut off with the leaf, in such a way that the shorter the petiole the greater is the deflection. Thus in a leaf with a petiole an inch long I observed a deflection of 40. I then cut off half, then half the remainder, and so on. After these successive amputations, the deflections were respectively 50, 65, 90, 120. If in this experiment, instead of completely severing the leaf at each time, it is merely all but divided with a sharp knife, the cut surfaces remaining in accurate apposition, the result is exactly the same as if the severance were complete; no further effect is obtained on separating the parts.

3. *Effect of constant current directed through the petiole on the leaf-current.*—If the leaf is placed on the galvanometer electrodes as before, and the petiole introduced into the circuit of a small Daniell, a commutator being interposed, it is found that on directing the battery-current down the petiole (*i.e.*, *from* the leaf), the normal deflection is increased; on directing the current *towards* the leaf, the deflection is diminished.

4. *Negative variation.*—*a.* If, the leaf being so placed on the electrodes that the normal leaf-current is indicated by a deflection *leftwards*, a fly is allowed to creep into it, it is observed that the moment the fly reaches the interior (so as to touch the sensitive hairs on the upper surface of the lamina), the needle swings to the right, the leaf at the same time closing on the fly.

b. The fly having been caught does not remain quiet in the leaf; each time it moves the needle again swings to the right, always coming to rest in a position somewhat farther to the left than before, and then slowly resuming its previous position.

c. The same series of phenomena present themselves if the sensitive hairs of a still expanded leaf are touched with a camel-hair pencil.

d. If the closed leaf is gently pinched with a pair of forceps with cork points, the effect is the same.

e. If the leaf-stalk is placed on the electrodes, as before, with the leaf attached to it, the deflection of the needle due to the stalk-current is *increased* whenever the leaf is irritated in any of the ways above described.

f. If half the lamina is cut off and the remainder placed on the electrodes, and that part of the concave surface at which the sensitive hairs are situated is touched with a camel-hair pencil, the needle swings to the right as before.

g. If, the open leaf having been placed on the galvanometer electrodes as in *a*, one of the concave surfaces is pierced with a pair of pointed platinum electrodes in connection with the opposite ends of the secondary coil of a Du Bois-Reymond's induction apparatus, it is observed that each time that the secondary circuit is closed, the needle swings to the right, at once resuming its former position in the same manner as after mechanical irritation. No difference in the effect is observable when the direction of the induced current is reversed. The observation may be repeated any number of times, *but no effect is produced unless an interval of from ten to twenty seconds has elapsed since the preceding irritation.*

h. If the part of the concave surface of the leaf which is nearest the petiole is excited, whether electrically or mechanically, the swing to the right (negative variation) is always preceded by a momentary jerk of the needle to the left, *i.e.* in the direction of the deflection due to the normal leaf-current; if any other part of the concave surface is irritated, this does not take place.

i. Whether the leaf is excited mechanically or electrically, an interval of from a quarter to a third of a second intervenes between the act of irritation and the negative variation.

"On the Algebraical Analogues of Logical Relations," by Alexander J. Ellis, F.R.S.

The object of this paper is to examine the "mathematical theory of logic," thus laid down by Dr. George Boole in his "Laws of Thought," p. 37:—"Let us conceive of an Algebra in which the symbols x, y, z, &c. admit indifferently of the values 0 and 1, and of these values alone. The laws, the axioms, and the processes of such an algebra will be identical in their whole extent with the laws, the axioms, and the processes of an Algebra or Logic. Difference of interpretation will alone divide them." For this purpose, first the laws of such an algebra have been investigated independently of logic, and secondly the laws of primary and secondary logical propositions as laid down by Dr. Boole, have been developed in an algebraical form, and compared with the former. The main results presumed to be established are:—

1. That there is a fundamental difference between such an algebra and logic, inasmuch as the algebra admits of only *two* phases, 0 and 1, and logic admits of *three* phases, namely, not only *none* and *all*, corresponding to 0 and 1, but also *some*, "which, though it may include in its meaning *all*, does not include *none*" (*ibid*, p. 124), and hence has no analogue in such an algebra; that is, an algebra of 0 and 1 can correspond only to a logic of *none* and *all*.

2. That, notwithstanding this difference, there are certain formal relations of equations which allow the algebra of 0 and 1 to be used as an *algorithm* for the purpose of arriving at certain logical forms, which, however, have then to be interpreted on a basis which has not even any analogy to the algebraical.

3. That the introduction of this algorithm introduces theoretical difficulties, adds to the amount of work, and is entirely unnecessary even for the purposes of the theory of probabilities founded upon it by Dr. Boole.

Mathematical Society, Nov. 13.—Prof. Cayley, and subsequently Prof. Sylvester, in the chair.—The following gentlemen have been elected officers of the new council:—President, Dr. Hirst; Vice-Presidents, Prof. Cayley, and Messrs. Spottiswoode and Sylvester. The retiring members were Prof. Crofton and Mr. J. Stirling, in whose room Mr. Sylvester and Lord Rayleigh were elected.—Mr. Sylvester then gave a description of a new instrument for converting circular into general rectilinear motion, and into motion in conics and other plane curves. (A brief sketch of the historical aspect of the communication, from the pen of Mr. Sylvester, forms the subject of a paragraph in NATURE of Nov. 13.) Several instruments were placed on the table for inspection.—Mr. W. Marsham Adams exhibited his Mensurator and Cœlometer, and gave a short account of the objects to which they could be applied. The Mensurator is an instrument designed primarily for the instantaneous solution of triangles, but capable, from its construction, of many other uses; such as illustrating most of Euclid's theorems with regard to the triangle, of performing addition, subtraction, rule of three, and extraction of square roots, of solving quadratics and simple binomial equations, and of reducing to mechanism some part of analytical geometry. The Cœlometer is an apparatus consisting of a stand carrying a globe mounted somewhat like a sea compass, and illustrates celestial longitude and latitude, the phenomena of the seasons, the correspondence of the calendar with the solar year, the precession of the equinoxes, the times of sunrise at any place on any day, the position of the principal stars during the night, and the general relations between the conceptions necessary for nautical astronomy. Medals were awarded for both instruments at the Vienna Exhibition. —Mr. S. Roberts (treasurer) read a short note "On the expression of the arc of a Cartesian by elliptic functions." The author showed that the hyper-elliptic part of the integral which gives the value of an arc of a Cartesian, is reducible to the form which Jacobi has shown to depend on elliptic functions.

Zoological Society, Nov. 18.—Dr. A. Günther, F.R.S., vice-president, in the chair. Mr. Sclater exhibited and pointed out the characters of two new species of birds obtained by Mr. Salmon during his expedition to the State of Antioquia, Columbia. These were named *Chlorochrysa nitidissima* and *Grallaria ruficeps*.—A letter was read from Mr. R. Swinhoe, H.B.M, Consul at Chefoo, containing a note on the White Stork of China, and stating that he had recently obtained a live Pitta in China, which appeared to be *Pitta nympha* of the Fauna Japonica.—Mr. A. H. Garrod exhibited and pointed out certain peculiarities in the cæcum of a Crab-eating Fox (*Canis cancrivorus*).—Mr. Sclater exhibited and made remarks on a pair of horns of the new Bubaline Antelope from the Bogos country, lately named *Alcelaphus tora* by Dr. Gray.—A paper was sent by Dr. Edward L. Moss, Surgeon in charge R.N. Hospital at Esquimalt, on a singular Virgularian Actinozoon taken at Burrard's Inlet, close to the northern mouth of the Fraser River.—A communication was read from Dr. O. Finsch, containing the description of a most remarkable and interesting new Passerine Bird which he had received from Mr. T. Klinesmith of Levuka, Ovalou, Feejee Islands. This little bird, which was not only new as a species, but also the type of a new genus, he proposed to call *Lamprolia Victoria*.—A communication was read from Mr. W. S. Atkinson, of Darjeeling, containing the descriptions of two new species of Butterflies from the Andaman Islands, which were named respectively *Papilio mayo* and *Euplœa andamanensis*.—Dr. Cobbold communicated the first of a series of papers entitled "Notes on the Entozoa;" being observations based on the examination of rare or otherwise valuable specimens contributed at intervals by Messrs. Charles Darwin, Robert Swinhoe, Charles W. Devis, the late Dr. W. C. Pechey, Dr. Murie, and others.—Mr. Edwin Ward exhibited and gave the description of a new Bird of Paradise, of the genus *Epimachus*, which he proposed to call *E. ellioti*.—A communication was read from Surgeon-Major Francis Day, containing remarks on Indian Fishes, mostly copied from the original manuscripts of the late Dr. Hamilton Buchanan.—Mr. J. W. Clark read a memoir on the Eared Seals of the Auckland Islands, one of which he recognised as *Otaria hookeri*, thus fixing the locality of this species.

Linnean Society, Nov. 20.—Mr. G. Bentham, president, in the chair.—Prof. Dyer exhibited a specimen of the fruit of *Luffa ægyptiaca*, a gigantic species of gourd, grown in this country.—An account of the flora of Monte Argentaro, on the borders of Tuscany, by Mr. Henry Groves, of Florence, was read.—On the Algæ of Mauritius, by Dr. Dickie. The total number of species recorded is 155. These include 17 well-known European species, most of which are cosmopolitan; 23 South African species; 12 Australian; 15 East Indian; 14 species found in the Red Sea; 12 being peculiar to the island.—On a peculiar embryo of Delphinium, by the Rev. C. A. Johns. The interesting point in the structure was the non-separation of the two cotyledons, the plumule forcing itself through a chink in the undivided cotyledon. Dr. Masters stated that this peculiarity is well known to occur occasionally in Ranunculaceæ, as well as in some other plants.—On the buds of Malaxis, by Dr. Dickie. This is supplementary to the paper already published in the "Journal" of the Society. —On the Algæ of St. Thomas and Bermuda, by Mr. H. N. Moseley. These were the results of the explorations on board the *Challenger*, one marine flowering plant being also found in flower for the first time.

Chemical Society, Nov. 20.—Dr. Odling, F.R.S., president, in the chair.—A paper on "the coefficient of expansion of carbon disulphide," by J. B. Hannay, was read by the secretary. —Dr. Russell then communicated his researches on the action of hydrogen on silver nitrate, giving an account of the precipitation of metallic silver in the crystalline state by means of hydrogen. —There were also a note on the action of zinc chloride on codeine, by Dr. C. R. A. Wright; on the chemical properties of ammoniated ammonia nitrate, by E. Divers, M.D.; and on the analysis of a meteoric stone and the detection of vanadium in it, by R. Apjohn.

Paris

Academy of Sciences, Nov. 17.—M. de Quatrefages, president, in the chair.—The following papers were read: —An answer to M. Tarry's remarks on the theory of the sun's spots, by M. Faye. M. Tarry's objection to M. Faye's theory was that, instead of a down-rush, he ought to have employed an up-rush as the cause of the spots, as a terrestrial cyclone rushes up, and not down. M. Faye answered the objections in detail.—Second memoir on the way in which water intervenes in chemical reactions, and on the connection between electro-motive force and affinity, by M. Becquerel.—Studies on beer; a new method of brewing it and rendering it unchangeable, by M. L. Pasteur. The author considers the spoiling and souring of beer to be due to germs, and suggests methods for preventing their access or destroying them during the processes of brewing.—An answer to M. Oudemans' observations on the influence of refraction, &c., during the transit of Venus, by M. E. Dubois.—On the use of the prism for the verification of the law of double refraction, by Prof. G. G. Stokes.—On certain metallic spectra (lead, chloride of gold, thallium, and lithium) by M. Lecocq de Boisbaudran. The author found that the combination of a metal was attended with the loss of some of the lines it exhibited when in the free state.—On the maximum density of water, by M. Pierron de Mondesir.—On the cooling effects of the joint actions of capillarity and evaporation, by M. C. Decharme.—On the quantity of ammonia contained in atmospheric air at different altitudes, by M. Truchot. The author stated that the ammonia increases as the cloud region is approached, and gave tables of determinations in support of his views.—Remarks on the paper of Pelouze and Audouin on the condensation of liquifiable matters held in suspension by gases, by M. D. Colladon.—Remarks on the paper of M. Derbès on the *Pemphigus* of *Pistacia terebinthus* compared with the *Phylloxera quercûs*, by M. Balbiani.—On the swellings produced on vine rootlets by the *Phylloxera*, by M. Max. Cornu.—On triple planes tangent to a surface, by Mr. W. Spottiswoode.—On the direction of the propagation of electricity, by M. Meyreneuf.—An answer to M. Mercadier's last note on the study of the vibratory movements of an elastic wire, by M. H. Valerius.—Observations on the molecular structure of meteoric iron and on solid ferrous chloride in meteorites, by Mr. J. Laurence Smith.—On the tertiary supranummulitic formations of the department of Hérault, by M. Rouville.—The death of M. Cl. Burdin, correspondent of the mechanical section, was announced.

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THURSDAY, DECEMBER 4, 1873

DR. MEYER'S EXPEDITION TO NEW GUINEA

BEFORE giving to the readers of NATURE a brief account of my own voyage to New Guinea, from which island I have just returned, I shall say a few words about some other expeditions to this "far-east" and interesting island, undertaken within the last two years by various governments and private individuals.

In 1871 the Dutch Government sent out a steamer and specially appointed officials, to circumnavigate the entire island of New Guinea, land from time to time, and take formal possession of those parts of the country which did not yet belong to the Dutch. It is known that this nation has hitherto had only a certain right over the western part of New Guinea as far as 141° E.; and before going farther the expedition had to erect sign-posts in the name of the Dutch Government at different parts of western New Guinea. But this first expedition did not attain its end; a few posts were set on the north coast, the farthest east being on Humboldt Bay, on the frontier of the Dutch territory; the expedition tried to go still farther east, but was obliged to return in a very bad condition, without having fulfilled its task.

In 1872 the Dutch sent out a second expedition in a small steamer for the same purpose; but this one did not succeed in going even so far east as the first, and was likewise obliged to return, after a very short time, without any result.

It is now proposed to try another expedition on a larger scale in 1874, which will go first through Torres Strait to the east, and afterwards, along the north coast, to the west; but I am not sure that this plan will be carried out.

In 1870 some Italians, under the guidance of M. Cerruti, who had been several times in New Guinea before (1861, 1865, and 1866), visited in a little schooner a part of the south-west coast, for the purpose of looking out for a convict settlement for their Government, and explored chiefly the straits between the island of Salwatty and the mainland. They were attacked in MacCluer Gulf, and the combat that took place was much spoken of in Dutch India during my sojourn there.

In 1872 two Italian naturalists, M. Beccari and M. d'Albertis, endeavoured to visit the mainland of New Guinea at a place on the south-west coast, called Utanate, which had been previously visited by the Dutch; but they could not reach it, on account of currents and winds. They remained for some time longer to the westward of the south-west coast and at Sorong, between the island of Salwatty and the mainland (where in the beginning of 1873 eighteen Europeans from an Australian pearlfisher were murdered by the Papooas*), then proceeded to Dorey, in the north, and made a station on the Arfak Mountains. They returned in November 1872, an Italian man-of-war being sent by their Government to look after them.

During the same time a Russian traveller, Mr. Maclay, had been on the north-west coast of New Guinea, in Astrolabe Bay. He was brought thither, and fetched away after more than a year's stay, by a Russian man-of-war; his plan to cross the mainland of New Guinea in any direction could not be effected, as was to be foreseen by those who know something about the special difficulties of travelling in New Guinea. He only moved about the district extending some miles round his station. Before I started on my expedition I met, in the beginning of this year, in Ternate (Molukkas Islands), the Russian man-of-war, coming back from New Guinea. Although she had only stayed in Astrolabe Bay for five days to take in water and wood, still, two months later, more than eighty of the ship's officers and crew were attacked by fever.

Finally, the news which reached Europe from Australia in respect to New Guinea, and which had already in 1871 made the Dutch send a man-of-war round that continent, to inspect how far the plans of the "New Guinea Prospecting Association" were ripening, this news, as well as the end of the expedition of this Association in 1872, are better known in England than the other undertakings which I have roughly sketched above.

The proposed and partly effected settlement of missionaries of the London Missionary Society on the islands of Torres Strait, and that proposed to be made on the south coast of New Guinea itself, are likewise known.

Whether the news, published in an Australian paper, that the English had taken possession of the extreme south-east shore of New Guinea, be true or not, I am not able to say. But from what has been said it may be concluded that the eyes of civilised nations are now fixed more on New Guinea than ever, and that results will be sure to follow. What these results may be, and what prospects all these exertions may present in respect to the character of the country and its inhabitants, I will not take into consideration now, but proceed to a sketch of my own voyage.

On my previous travels, which went as far as the Island of Celebes and the Philippine Islands, I had gathered sufficient information to know which would be the easiest way to reach New Guinea, and how to find the best place for a station. I was aware of the impossibility of penetrating into the interior of the larger part of this large continent, if my expedition were not of much larger dimensions, and if it could not command much ampler means than are available to a private person like myself, and chiefly if it would not lose its character of a natural history expedition, and become solely a geographical exploration. On another occasion I shall show how such a geographical expedition through New Guinea could be started by one individual or by a company of travellers, and to what parts it would be most advantageous to proceed at first. The peculiarities of the country and the natives are such, that there are even more difficulties for travellers here than there are in Australia, where the best-fitted-out expeditions and the most able and courageous men have failed. In New Guinea it is even a question, whether the scientific or practical results are likely to be at all equal to the expenditure and the great danger connected with such an undertaking.

Being obliged, therefore, to resign the eastern and larger half of the island, I had to choose for a starting-point only between the south-west coast, from opposite the Aru Islands to Salwatty, and the coasts of Geelvinks Bay. I preferred the latter, for several strong reasons which weighed against the south-west coast, and in favour of Geelvinks Bay; among others were the unhealthiness of the swampy shores of the south-west coast, the fact that the natives of these parts have been more influenced by Malay traders for centuries than those of the northern regions, and therefore are less original, and that the south-west coast has been visited oftener by Europeans.

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* I write Papooas, and not Papuas, because the Malays pronounce the word "Papooa," and not "Papua."

In consequence of the time consumed in making all necessary preparations, I only reached New Guinea in the beginning of March of this year, and anchored the little schooner, which I had hired in Ternate—(I preferred this place as a starting-point to Amboina)—in the harbour of Dorey, in the north-west corner of Geelvinks Bay, the only part of all New Guinea where any Europeans, German and Dutch missionaries, are settled. With me, and in my service on board the ship, over which I had full disposal, were, besides about fifteen Malay sailors and a Malay captain, twenty natives from different parts of the Malay Archipelago.

Dorey has been described, among others, by Mr. Wallace, but he has, in my opinion, not given a correct impression of the natives of the surrounding hills and mountains, separating them in some way from the inhabitants of the coast, as smaller, uglier, not mop-headed, &c. As I afterwards spent a long time among the natives of the Arfak Mountains, near Dorey, and the inhabitants of the different parts of the coast of Geelvinks Bay, the islands in the north of it, and the interior of this whole north-west part of New Guinea, I may state, that there is no generic difference at all between the Papooas of the mountains and the Papooas of the coast—except such differences as we find everywhere between the highlanders and coast inhabitants of the same race.

The changing of the West into the East Monsoon, to be expected in April, obliged me to visit first the islands in the north of Geelvinks Bay, if I wished to visit them at all; and therefore, after a three days' stay at Dorey and neighbourhood, I immediately started for Mafoor, a smaller island, only about sixty miles from Dorey. It took me more than forty-eight hours to reach it. Mafoor offered me nothing particular, besides its zoological productions; it is only interesting, for having been at an earlier period the chief seat of the Mafoor tribe, which now inhabits all the coasts of the western part of Geelvinks Bay.

The island of Mysore (Willem Schouten's island), the furthest north and a larger one, was far more important for my purposes. The natives were at first of a hostile disposition, but we soon became friends, and I spent here a most interesting time, in consequence of the results of my collections and what I saw of the Papooas, wild and nearly unmodified tribes, with all the virtues and vices of such. I commenced to make a collection of Papooan skulls here, in which at last I was so successful, that I hope to be able now, by means of my large materials, to trace at least the limits of the variation which the skull of this race undergoes, and finally to fix the type of the Papooan skull,—important questions, which craniology can solve.

On Mysore, Birds of Paradise, as well as the Kasuary, are unknown; but it as well as the large island of Jobie, which I now visited, is the home of the fine Crown-pigeon (*Goura victoriæ*). This beautiful bird occurs in such quantities, that it furnished us with our daily meals during nearly the whole of our stay on Mysore and Jobie. The flesh is most tender and delicate, preferable to any fowl I know of.

Jobie has for long had the reputation of being the home of many species of rare birds of Paradise; I am sorry to be obliged to rob it of this glory. I only got *Paradisea papuana* (but with more splendid, deeper orange and longer side feathers than from the mainland of New Guinea), *P. regia* and *Diphyllodes speciosa*. *P. papuana* is not rare, but very local, so that one may hunt for weeks in the mountains, without finding a single specimen (females and young males are seen and heard much oftener than males in their plumage); but if once a tree is found where they feed and "dance," a lot of them can be got together. *P. regia* is rare, and *D. speciosa* very difficult to get here.

I am sure that no other species of Birds of Paradise come from Jobie, as no other species are in the hands of the Papooan traders. I am convinced of this, because I stayed a long time at the chief place of the island, Ansus, a very populous settlement, where I saw and heard everything; more than two thousand Papooas are living here together, all in those large, curious houses, standing entirely on the water. But other species of Birds of Paradise which I showed (I had obtained some in Ternate and Dorey on my way), were unknown even to the inhabitants here, except to those who had been on the mainland. I mention this fact, notwithstanding that I had not the intention of giving any zoological details in this account, because it may be seen, how erroneous it was to give credit to the natives of New Guinea, who pretended that some rare birds of Paradise came from Jobie, certainly with the purpose of withdrawing attention from their own country, where these birds are to be found.

In general the fauna of Jobie is very poor, as well in respect to different species as to the mass of individuals of one species.

The inhabitants of the mountainous parts of Jobie are known to be cannibals, as well as the tribe of the Karoans, in the mountains of the north coast, between Amberbaki and the two small islands Amsterdam and Middelburg,[*] and the tribe of the Taringarays on the east coast of Geelvinks Bay. Here on Jobie, as everywhere on New Guinea, the coast Papooas are in perpetual war with the mountain tribes. Perhaps because some individuals of the latter believed us to be more friendly to the inhabitants of Ansus than to themselves, or that they required no special reason for fighting, once without any warning or provocation we were attacked with spears and arrows; but I afterwards took such precautions and frightened them to such a degree, that during our whole stay here we experienced nothing further disagreeable. The Papooas of Jobie have everywhere the bad reputation of being wild and quarrelsome.

After having left Jobie, from which I set out with a heavy heart—I should have liked to remain longer—I went as far to the east as the river Amberno, in fact to the north-east corner of Geelvinks Bay, and then shipped to the south-west, along the coast, landing and hunting from time to time, and trying to find a favourable place for a longer stay, and a spot from which it would be possible for me to penetrate into the interior, or to cross the island.

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[*] When back at Dorey in July, the natives here were very much frightened, because the Karoans near Amberbaki had just robbed and destroyed a trading vessel, and killed or enslaved the crew.

I did not succeed till I reached the south point of the bay. Here I found a little tribe of Papooas, who treated me from the beginning to the end in the most friendly way. On this account, and because I enjoyed a very favourable hunting-ground (immediately after going ashore I got four different species of birds of Paradise), I remained here for some weeks. Shortly after having anchored, even the young girls came on board the ship together with the men, and remained there for hours, whereas, in other parts, the women generally are very shy and keep aloof.

Seeing that I could trust the natives here, I tried to carry out my project of crossing the country to the coast opposite the Aru Islands. But even if I had not come so far, for reasons which it would be out of place to give in this brief account of my journey, I got some interesting additions to our geographical knowledge,* and was very much satisfied with my zoological collections.

But I would not give up my plan of crossing New Guinea, and therefore proceeded near the coast to the north-west, looking out everywhere for a convenient starting-point, and gathering every possible information from the natives. But the island was still too broad here; the Papooas knew nothing of the opposite coast, as they do not migrate so far.

The natives of these western coasts of Geelvinks Bay are all afraid of the Wandamman tribes, whereas those of the eastern coasts are afraid of the Waropin tribes; but generally the vast country here is very poorly populated, there being few settlements, and these few small ones.

The point where I crossed the island at last into MacCluer Gulf of the south-west coast was situated 134° 18′ E., 20° 38′ S. I went first to the north-west, and then, after having passed several mountain chains (2,000 ft.), to the west, down a fine river called the Jakati, through the country of Onim. It was, perhaps, lucky for me that I could only get a very small native prau here, else I would have proceeded farther west by sea, (the swamps render it impossible to go by land), and possibly encountered dangers from the natives of the MacCluer Gulf, who have not the best reputation, and who certainly would have felt inclined to revenge their countrymen, killed by the Italian Cerruti and his company, some years ago.

I need not say that this journey from one side of New Guinea to the other has never been made before; and I should hardly myself attribute any importance to the fact, were it not for the reason that till then we did not know exactly whether there existed a communication by water from Geelvinks Bay to MacCluer Gulf or not. We may be convinced now that it does not exist.

From Geelvinks Bay I tried to ascend the Arfak Mountains from the south, but did not succeed, because I ran short of provisions. The country seemed uninhabited, or, without Papooan guides as I was, and with only some of my Malay companions, I missed the few native houses and small plantations in the neighbourhood, scattered here and there, so that it was not advisable to go too far into the country. Besides, I did not find in the forests on the southern slopes of the Arfak Mountains those gems of the bird-world which I hoped to find, and therefore left these regions and penetrated from the north with better success. It will be interesting for English ornithologists to learn that I succeeded in finding here (3,000—6,000 ft) all the known Birds of Paradise of New Guinea (except the species from the islands), besides a new one, and a quantity of other most curious and rare specimens, the ornis of the mountains being quite different from that of the seashore. But here also, as on the whole of New Guinea, I was astonished to see that the fauna generally is not rich. The forms found in the country certainly are curious and characteristic, but, in comparison with the enormous mass of forest, they are everywhere very scarce, and it is an exception to find a hunting-ground where much is to be got in a short space of time. It is the same in New Guinea as I found it in Celebes, where more of the life of nature is to be seen and heard near the seashore and the settlements than in silent virgin forests.

I hope that now, since the interior of New Guinea is opened, at least as the way is known how to penetrate into one part of it, other naturalists will soon succeed in reaping more important results than it was my lot to obtain.

ADOLF BERNHARD MEYER

MICROSCOPIC PETROGRAPHY

Mikroskopische Physiographie der petrographisch wichtigen Mineralien ein Hulfsbuch, bei mikroskopischen Gesteinsstudien. H. Rosenbusch. With 102 woodcuts and ten coloured plates. (Stuttgart.)

SINCE we last called attention to this subject in the columns of NATURE it has been making steady progress, chiefly among our German, that is, of course, German-speaking, brethren of the hammer and lens. The various serials which treat of Geology and Mineralogy bear witness to this progress, and to the wonderful activity of some of the workers, such as our good friends Zirkel and Tschermak, to whom it is so largely due. And now here comes a goodly octavo of some four hundred pages as a further contribution to our knowledge, and a fresh proof of the strong hold which the microscopic study of minerals and rocks has taken of the German geognostical mind. This activity need not be matter for wonder when one considers the chaos into which matters petrographical had got even in Germany. Those who studied rocks in that country had become a sort of bound thralls to chemists and chemical analysis. They dared not trust their eyes to discriminate the differences of species and varieties. The specimens must be handed over to the laboratory, and on the judgment thence obtained depended the names by which the compounds should be known thenceforward throughout Christendom. By this means, as the composition of a rock often differs considerably in different, and even in closely-adjoining, parts—variations resulting partly from original discrepancies, and partly from internal changes due to the subsequent infiltration of water or other metamorphic influences—it was not difficult to make out half-a-dozen distinct varieties of rock from the same mass and even from the same quarry. And so analysis of rocks grew and multiplied, chemists became more and more nice in their discrimination of the veriest fractions of a per cent., petrography seemed in a fair way of being annexed as a dependent province of chemistry, and the petrographers, who ought to have been geologists, and to have set themselves strenuously to find out what had been the history of the rocks as parts of the architecture of the globe, came gradually to accustom themselves to the notion that, after all, it was really true that rocks were merely so many chemical compounds to be analysed and labelled accordingly.

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* The geographical part of my expedition will be published very soon, accompanied by a chart, in Petermann's *Mittheilungen*.

In the midst of the darkness wherein the poor petrographers ticketed their specimens, carefully arranged their cabinets, and elaborated their dreary treatises, there fell among them (not from heaven, but from the hands of a worthy citizen of Sheffield) a microscope and a few glass slides, with a description of what could be done therewith. Eyes which had seen no light for so long could not at first make anything of the apparition, but after a few years it began to take shape before them; and now the microscope promises to do as much in comparison for mineralogy and petrography as it has done for the biological sciences.

From town to town this new light has spread, or rather rushed, all over Germany. There is now a sort of neck-and-neck race who will make the most slices of rocks and minerals. A cutting or rubbing-machine and a microscope have become as necessary implements as a hammer or a lens. Every month brings to light some new "mikromineralogische" contribution, insomuch that if the fever lasts we shall ere long be as over-weighted with microscopic analysis as we used to be with chemical. Both are excellent and necessary, and yet we may be allowed to believe that neither singly nor together do they disclose to us anything like the whole history of the rocks, and that they cannot by themselves yield a sufficiently broad foundation for a truly philosophical classification in petrography.

The advantages of microscopic analysis applied to rocks are so many and obvious that we cannot be surprised that they should have been so widely recognised and put in practice. This method of investigation throws a direct light upon the construction of rocks in a way which chemical analysis can never do. Moreover, it is easily adopted. Anybody can make microscopic sections, and with due care and experience may become a skilful analyst. And then this mode of research is so cheap. Even if the observer does not care to give the trouble and time necessary for the construction of his own sections, he can get them made for him at small cost. And once in possession of them and his microscope, he obtains his results at once. No need to wait for days upon a solution, or to weigh and re-weigh his precipitates.

It is plain that as rocks are composed of aggregates of minerals in many various combinations, the first preliminary step in our investigation of their minute structure should be devoted to the study of the microscopic character of the minerals which compose them. We must know how these minerals are built up in themselves before we can adequately comprehend the manner in which they are mingled together to form rocks. Besides, in a crystalline rock, such, for example, as basalt, the component minerals are crystallised on so minute a scale, and often so imperfectly, that their ordinary and characteristic peculiarities may be so veiled that, unless from previous experience, we could not with certainty recognise them. Hence every student who sets himself, microscope in hand, to find out how the materials of the rocky crust of the earth have been put together, ought unquestionably to begin the search by accustoming his eye to the variations which the simple minerals present when viewed in different positions under a strong magnifying power. It will not be necessary for him to cut slices of every known mineral. He will have done enough for his immediate purpose if he has sliced in all directions, and examined with polariscope and otherwise the comparatively few simple minerals which are of prime importance, as those of which most of the visible rocks of the globe have been formed.

A text-book which will guide him in this most interesting and important research has never hitherto appeared. Descriptions of the methods to be employed in the preparation of translucent sections have been published both in this country and in Germany. Indeed, it was Nicol, of Edinburgh, who, besides giving us the prism named after him, invented and made known more than forty years ago this most ingenious method of investigation. Abundant notices have also been published during the last dozen years, chiefly in Germany, regarding the microscopic characters of many minerals and rocks, so that a student who had time and opportunity to consult this very scattered literature, might gain amply sufficient knowledge to start him in his research. But none the less has a general text-book been required to save such needless expenditure of time, and to give the student those practical hints which he is not likely to meet with in mere scientific communications on special subjects. It is this want which Mr. Rosenbusch endeavours to supply in the volume now before us.

From his preface we gather that living at Freiburg he caught the microscopic fever, and has had it for a number of years. Anxious to communicate the infection as safely and effectually as possible to the younger mineralogists, he has compiled a text-book which ought to serve its purpose well. It is well arranged, neatly printed, excellently illustrated, and cheap. After some introductory pages which skim over the history of his subject, the author proceeds, in the first or general part of his treatise, to give the student directions how to cut and prepare his slices of mineral or rock for microscopic examination. Then, having the slices prepared, he shows how they are to be used, and what may be looked for in them. With characteristic German completeness he speaks of the general morphological peculiarities of minerals crystallised and amorphous, and shows how singular and varied are the anomalies in internal structure revealed by the microscope even in what seem to be the most regularly built crystals. The optical properties of minerals are discanted upon with a fulness perhaps hardly in keeping with the other parts of the book, but the importance of this branch of the subject, particularly in reference to the analysis by means of polarised light, may well be pleaded in excuse by the author. The third section of the general descrip tion of minerals deals with their chemical peculiarities. It occupies not quite four pages, and has evidently been inserted for the sake of completeness, that the learner, even though specially intent upon microscopic work, should not be left wholly in the dark as to what he can accomplish himself in the way of chemical analysis.

The second and by much the more important and useful part of the treatise deals with the microscopic characters of minerals, and more particularly of those which enter largely into the composition of crystalline rocks. Considerably over a hundred minerals are treated in this way, and these, of course, include all those which are of prime consequence to the petrographer. For example, the felspars, augite, hornblende, calcite, quartz, pyrite, and other common ingredients of rocks are fully described. The author has worked hard at the subject himself, though he has not hitherto published much. One excellent feature of his volume is the full references which he gives to the papers of previous writers on the same subject. Not only at the beginning of the description of each mineral does he quote, in legible print, the titles of the papers in which information about the microscopic characters of that mineral may be found, but at the end of the volume he inserts a long alphabetical list of authors, with the names and dates of their papers. This is a most welcome boon to all those who, especially in our own country, have the courage to attack the voluminous, but hitherto hardly known or accessible literature of the subject. Two sorts of illustrations are given—woodcuts and coloured plates. Of the former rather more than 100 occur, mostly illustrative of the crystalline forms or optical characters of the minerals. They do not call for special remark, except that they might with advantage have been more numerously inserted to explain the internal peculiarities of some of the numerous species described. The coloured plates are singularly effective. Ten in number, they contain sixty figures of the microscopic structure of upwards of thirty more or less common minerals. We have seen nothing so good since Vogelsang's large and admirable drawings published six years ago at Bonn. It appears that it was originally intended to have included more plates, but that the cost proved so great that the number had to be restricted to ten. This, no doubt, is the reason why some not very important minerals have a place on the plates, while others of greater consequence have been left out.

This volume, even had it been less painstaking than it is, would have deserved commendation as an introduction to a study for which no text-book at all previously existed. But, as its author frankly acknowledges, it will not and is not intended to supply the place of actual personal work—"he who would learn microscopic mineralogy must to the cutting-lathe and the microscope." The greater the number of observers who can be induced to betake themselves to this pursuit, the sooner may we hope for some definite and broad well-established results. At present the work accomplished, most excellent and praiseworthy though it be, belongs rather to the hewing-of-wood and drawing-of-water order. The facts are weekly accumulating out of which, in the end, a flood of light will unquestionably be cast upon the genesis of rocks, and consequently upon the history of the earth itself. All honour, therefore, to the enthusiastic workmen by whom this labour is so cheerfully and actively undertaken, and none the less to Mr. Rosenbusch for publishing a most useful volume, which will, no doubt, increase their numbers.

OUR BOOK SHELF

Solid Geometry and Conic Sections, with Appendices on Transversals, and Harmonic Division, for the Use of Schools. By J. M. Wilson, M.A. Second Edition. (Macmillan and Co., 1873.)

Elementary Geometry, Books i. ii. iii., following the "Syllabus of Geometry," prepared by the Geometrical Association. By J. M. Wilson, M.A. Third Edition. (Same publishers, 1873.)

THE portions of the title-pages we have above given sufficiently indicate the scope of the two works under review and the measure of acceptance they have met with. As we have already given an account of the former work it will not be necessary to give any detailed account of it now. It has been considerably improved by the addition of some eighteen pages of new matter, consisting of a slight rearrangement of Section I., which treats of planes, the addition of a section (IV.) on the sphere, which is almost entirely new to the work, and some slight changes in the articles on the Ellipse and Hyperbola. The result is a close approximation to the views we expressed in our previous notice, and the book can be recommended as an excellent, if not the only English, treatise suited to the requirements of candidates for the first B.A. Pass Examination of the London University. We point out an obvious slip of *inscribed* for *circumscribed* circles, on p. 55; in the fifth paragraph, p. 56, all the A's but one should be accented; the last exercise, on p. 68, is misplaced, and repeated in its proper place, as Exercise 29 on p. 71; other minor slips can be easily corrected.

The "Elementary Geometry" is to our mind a vast improvement upon the first edition; the changes are all, we believe, in the right direction. We never took kindly to that first edition; the most confirmed euclidophilist must be led by a perusal of this to a more favourable view of the aims of anti-euclidean agitators. Seeing that the aim of teachers of both parties, if they are in earnest, should be the *improvement* of geometrical teaching, we trust that neither party will lose sight of this high mark through intervening clouds of dust raised on irrelevant grounds.

The "get-up" of both books is excellent, the printing of the "Elementary Geometry" most accurate (we have detected but one or two slight errors). We wish to add a closing remark on this subject of *errata*: we consider that an author is bound to bestow every care in this matter, and it is with regret that we find some works of recent date have been brought out, it is reasonable to suppose, in such haste to meet a possible demand for them that they may be said to teem with mistakes. This entails great waste of time and trial of patience upon junior students and appears to us unfair treatment. R. T.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Southern Uplands of Scotland

To the able articles on this subject contributed to your pages by Prof. Harkness, I should like to be permitted to make an addition. He has referred to some opinions and observations of mine, but I am anxious that it should be generally known to what an extent the results obtained by the Geological Survey are due to the zeal and ability of my colleagues. Thus, Mr. R. L. Jack has the merit of detecting and tracing the Caradoc basin of the Leadhills, and of working out the structure of that region which has been of so much service in the subsequent progress of the Survey. Mr. John Horne has carried the lines far into Galloway, and Mr. D. R. Irvine has traced them across a great part of Wigtownshire. Mr. H. Skae has mapped them across Dumfriesshire into Selkirkshire, while Mr. B. N. Peach, besides doing excellent service in the west, is now running them across the rest of the country towards the sea on the east.

Allow me also on the part of my colleagues, as well as for myself, to take advantage of this opportunity to thank Prof. Harkness for his most valuable and welcome papers, and to express our gratification that the labours of the Survey should have found so courteous an exponent, and one whose knowledge of the country which we have mapped is so minute and extensive.

ARCH. GEIKIE

The Huemul

IN Vol. viii. p. 253 of your valuable journal, I find it noticed that the Chilian Exploring Expedition has taken a specimen of the Huemul, an animal which had altogether been lost sight of, and first described by Molina under the name of *Equus bisulcus*. This notice is not correct, as the animal has been described already in 1846 by Messrs. Gray and Gervais, in the *Annales des scient. natur.* iii. Ser. Tom. v. page 91, under the name of *Cervus chilensis*, and compared with *C. antisiensis* of D'Orbigny, as the species most nearly allied to the Huemul or Guemul or Guamel, different names for the same animal in different parts of the country. This first description was repeated the following year in the "Historia fisica y Politica de Chile," Zoology, vol. i. page 159, and accompanied by the figure of the animal (pl. 10, and its skull pl. 11), from the only known specimen of a young male of half-grown size, brought to Paris by Mr. Gay. On the same specimen Mr. Pucheran has founded his description in his valuable monograph of the genus Cervus, published in the *Archives du Museum*, vol. vi. page 965 (1862), and from these two descriptions Mr. A. Warner has given a combined extract in his "Sangethiere," &c. Tom. v. (supplement), page 382, under the same name of *Cervus chilensis*. Meanwhile Dr. J. E. Gray had described a species of deer, received by the Earl of Derby from Chili as *Cervus leucotis* (Annals of Nat. Hist. ii., Ser. Tom. v. page 324, 1840, and Proceed. Zool. Soc., 1849, page 64, pl. 12), which name he soon changed to *Furcifer Huamel* (Annals ser. iv. 427), and at last to *Xenelaphus huamel*, adding to his first description new notices, with the figures of the horns of the male (Proceed. Zool. Soc., 1869, page 496), and the skull of the female, and stating that his *Cervus leucotis* is identical with the *Cervus chilensis* of Gay. This exposition proves that the Huemul or Guemul is already a very well-known animal, and has by no means been overlooked by naturalists.

A young collector in Buenos Ayres, Mr. Franc Moreno, has lately received a pair of these animals from Patagonia, where they were caught by the Indian Pehuelches, who live on the western foot of the Cordilleras, near the sources of the rivers Negro and Colorado. These two specimens have been brought to the public Museum, where I have examined them carefully. The male is a young one, with horns still covered by the skin, and only 3 in. long, without branches. I regret that therefore I can say nothing about the figure of the adult's horns, which are according to the drawing given by my dear friend, Dr. Gray, very like that of the roebuck, although the specimen he has figured may be regarded as in an abnormal state, from the great difference between their two sides. Both sexes of the animal are of equal size—3 ft. high on back, and 4½ ft. long, the head being 10 in. long, the ears and the neck 7 in. every one, and the body 3 ft. without the tail, which measures 7 in. with its hairs, but only 4 in. in the axis. Great naked lachrymal pits are seen below the eyes. The fur is of the same quality in each, but very different in the cold and in the warm seasons; then both skins are in the time of hairing, the female with the prevailing hair of the winter, and the male with the prevailing of the summer. Each hair is not entirely straight, but some are undulated, principally on the under half, and this undulated portion has a clear greyish-brown colour; over this clearer portion comes a broad dark-brown or black ring, and the end is clear reddish yellow, with a fine blackish tip, generally broken off in old fur. For the winter dress the hairs are 2 in. to 2½ in. long, and of a less characteristic colour, being over the whole skin of an undistinguished greyish-brown colour; but in the summer dress the hairs measure no more than 1¼ in. or 1½ in., and all their colours are cleaner and better pronounced. Therefore the animal is darker and more distinguished in colour in the summer than in the winter. The hairs on the face are very short, as are those on the outside of the ears, somewhat longer on the legs, but nearly as short on the under half part of the extremities. The breast and the tail have the longest hairs. Different in colour are the naked nose and upper lip, both entirely black; the breast is dark blackish-brown, the genital region to the tail, with the inside of the hinder upper legs being white. The same colour also pervades the inside of the ears, which are coated with long hairs; the hoofs are black. No tinge of the particular stripe of longer hairs on the tarsus of the hinder legs is conspicuous in either sex; but I find, with Dr. Gray, a large tuft of longer hairs on the hock, on the inside behind, which makes this part of the legs very thick.

The animal lives principally in the valleys of the Cordilleras, but on both sides, the eastern and the western, and rarely goes down to the flat country of the Argentine pampas. Its proper range is between 35° and 45°. It is well known by the Indians, who not only make use of its strong skin for war-dress, and its meat for food, but also tame young animals, using them for domestic employment, like the Guanaco, which lives in the same territory, but is much more common, and therefore almost the only animal used for hunting by the same people.

Buenos Ayres, Sept. 20

DR. BURMEISTER

The Diverticulum of the Small Intestine considered as a Rudimentary Structure

I MUST claim the opportunity of reply to the article which appeared in your number of October 16 (vol. viii. p. 509), entitled "On the Appendix Vermiformis and the Evolution Hypothesis," which the writer offers as a commentary on my little paper at the recent meeting of the British Association, "On the Diverticulum of the Small Intestine considered as a Rudimentary Structure."

The writer seems to have been misled by newspaper reports. None of these were furnished by me, or submitted to me before publication, and in those which I saw after their publication both the anatomy and the argument were grossly and indeed absurdly blundered. This applies not only to my paper and remarks, but to the remarks made by those who spoke on my paper. It was, perhaps, too much to expect newspaper reporters not to get confused among scientific terms, and I may have erred in not having the usual abstract of my little paper ready to hand to the reporters.

Newspaper reports may be passed without notice. but I cannot allow an article in a scientific periodical to pass in which the writer uses such language as the following, with which the article in your columns concludes:—

"To quote the words of one of the greatest of our physiologists, it can only bring ignominy on the body of scientific workers if they are supposed to countenance such an argument as that of Prof. Struthers, which assumes that because one or two individuals have died from the impactation of cherry-stones in the appendix vermiformis, therefore there is no God!"

The "no God" was certainly not in my paper or in anything I have ever written or spoken, and the accusation is to me so offensive that I repudiate it with indignation. How anyone should suppose that the evolution hypothesis implies that there is "no God" I am at a loss to understand. I supposed it to be well understood that, on the contrary, that great hypothesis enables us to rise to higher conceptions, the only question being the mode of proceeding.

As to the scientific argument, it seems hopeless to attempt to unravel the confusion into which newspaper reports and my critic have brought it, except by re-stating my argument. But this is for the most part unnecessary after your publication of my abstract in the number following that in which the article of which I complain appeared. It cannot be absolutely proved that the appendix vermiformis is useless, though a survey of the facts in comparative anatomy and development leads to the inference that it is a rudimentary structure. But my paper was on the diverticulum, the appendix being referred to only collaterally, and more for the sake of clearing away the most unnecessary teleology with which it has been encrusted, than with the view of resting the argument on it. The diverticulum, like the appendix, has glands and muscular layers, secreting and expelling; it has villi, actively absorbing; and it is large, which the appendix is not. Yet, notwithstanding all this elaborate construction and this activity, who will maintain that this unclosed bit of the vitelline duct has been left unclosed in some of us for use? But one is sometimes met with the remark that, if these rudimentary and variable structures are useless, they are at any rate not injurious. But is it so? May they, and do they, not become injurious under disease or accident? There is the male mamma, for instance, which we have sometimes occasion to excise for disease. Whatever may be the law which regulates the evolution of the sexual organs, no "use" theory can account for the presence of that rudimentary organ. But the diverticulum is a possibly injurious structure not merely as a tissue, but in addition, specially, as forming, if I may use the word, a kind of trap, by lodgment or by strangulation. Thus we find that we have, whether we will or no, reached the conclusion that there are parts in the animal body which are not only useless but worse than useless because dangerous.

I do not see any reply to this in my critic's remark that it proves too much for the argument, that, for instance, because some people have died from wounds of the scalp, therefore the head might be dispensed with. For, however much the head may vary among us, it is not a rudimentary structure. No argument can affect the fact that the diverticulum is not only a useless structure, but worse than useless because dangerous. The object of putting it thus emphatically is both to establish and to call attention to the conclusion that there are such things in animal bodies as rudimentary structures, things which are of no use to the animal body which contains them, and which can be understood only by referring to other animal bodies in which they are in full play; and that we must therefore rise to higher conceptions of the mode in which these things are regulated. It was carefully stated in my paper that the consideration of such parts as the diverticulum does not carry us further than to clear away the old argument, but that, on taking a survey of rudimentary structures generally, we are led on to the conclusion that the evolution hypothesis is the more probable one in regard to the mode of origin of animal bodies.

The nature of the diverticulum and its sources of danger are well known to the readers of Meckel, Monro, Lawrence, Rokitanski, and Cruveilhier. I may be allowed to mention that nearly twenty years ago I published (Edin. Med. and Surg. Journal, April 1854) twenty cases of diverticulum, with a drawing of each. In three of these it was the cause of death, and I referred to some other cases in which it caused death as reported by previous writers. Anyone in London who is desirous of seeing a case in which it caused death, may do so by looking into the museum of St. Bartholomew's Hospital. There is, I may mention, a diverticulum, at the usual place, in a subject now being dissected in my anatomical rooms. If my critic will come to Aberdeen I will show him a large collection of them, and also of specimens showing the various positions and conditions of the appendix vermiformis, and, indeed, many other interesting rudimentary structures and variations which, I infer, he has not yet seen.

My critic's objection that such discussions are unnecessary, that the true theory will ultimately prevail from its own intrinsic value, might be urged against all discussion; and I differ from him very much if he thinks that the question does not require to be stirred among and by the teachers of human anatomy in this country. The cause of my little paper, in fact, was my having, not long before, heard a teacher of human anatomy, at a similar meeting, call in question the whole argument from rudimentary structures. I attributed no importance to my paper further than that, in bringing forward the diverticulum, it submitted an illustration for the argument which does not admit of cavil.

Aberdeen, Nov. 22 JOHN STRUTHERS

The Atmospheric Telegraph

WILL you permit one of your subscribers who is interested in the credit of the English telegraphic system, to supplement your article of November 27 by a few remarks?

The distribution of telegraphic messages by means of air was introduced by Mr. Latimer Clark, and had been employed by the Electric Telegraph Company long before it was adopted either in Berlin or Paris.

The *Times* article of November 15 deals with the undertaking of the Pneumatic Despatch Company for the conveyance of parcels and goods, not messages. The writer incidentally mentions the transmission of messages, but scarcely seems to have been aware of the extent of the London message system.

If I might encumber your valuable space by statistics, I could show that the pneumatic system of the Postal Telegraphs, or even that of the Electric Telegraph Company at the time of the transfer of their undertaking to the State, will bear comparison, both as to extent and efficiency, with that of Paris, effective as the latter is.

The system is employed in Manchester, Liverpool, Birmingham, Glasgow, and Dublin. R. S. CULLEY

Engineer-in-Chief of Postal Telegraphs

General Post Office, Nov. 29

SENSATION IN THE SPINAL CORD

THE principle which I endeavoured some years ago to get recognised as the directive principle of research in Nerve Physiology, was that everywhere identity of Tissue carried with it identity of physiological Property, and that similarity in the structure and connections of Organs involved corresponding similarity in Function. Although these premisses were almost truisms, the conclusion drawn—that all nerve-centres must have a *common* Property, and *similar* Functions—was too much opposed to the reigning doctrine, to find general acceptance. Especially was it resisted in its application to the functions of the Spinal Cord; and this because of the two hypotheses current, namely, that Reflex Action did not involve Sensibility, and that the Brain was the sole Organ of the Mind. Following in the track so victoriously opened by Pfluger, I brought forward what seemed to me decisive evidence of the sensational and volitional functions of the Spinal Cord; but this evidence has not been generally deemed conclusive by those whose verdict is authoritative. Neither in Germany nor in England have the majority of physiologists consented to regard the actions determined by tthe Spinal Cord in the absence of the Brain as sensitive actions.

This is not the place to examine the insufficiency of the evidence which is held to exclude sensation from Reflex Action, nor to exhibit the irrationality of the conception of the Brain as the Organ of Mind—which, as I have elsewhere said, is not more acceptable than would be the parallel conception of the Heart as the Organ of Life. The purpose of the present paper is restricted to the examination of the most striking experimental evidence *against* the sensational function of the Spinal Cord, which to my knowledge has hitherto been advanced. I had intended reserving the criticism for its appropriate place in the "Problems of Life and Mind," but an article by Mr. Michael Foster which has just appeared (*Journal of Anatomy and Physiology*, November), on the Effects of rise of Temperature on Reflex Action, induces me to bring the subject before the readers of NATURE, in the hope that some of them may re-investigate it and record their results.

I will merely remark that the microscopic investigations which have recently been made with greatly increased powers and better methods of preparation, have more and more confirmed my assertion of the histological identity of Spinal Cord and Brain. On the other hand the experiments of Goltz (*Functionen der Nervencentren des Frosches*, 1869, p. 128) seem to supply direct evidence against the identity of property; and this evidence cannot be ignored.

Goltz observed that a frog, when placed in water the temperature of which is slowly raised towards boiling, manifests uneasiness as soon as the temperature reaches 25° C., and becomes more and more agitated as the heat increases, vainly struggling to get out, and finally, at 42° C., dies in a state of rigid tetanus. The evidence of feeling being thus manifested when the frog has its brain, what is the case with a brainless frog? It is absolutely the reverse. Quietly the animal sits through all the successions of temperature, never once manifesting uneasiness or pain, never once attempting to escape the impending death. "The spinal soul sleeps, perhaps; it takes no heed of the danger. One must waken it. I touch with acid the skin of its back in that part which is raised above the surface of the water. *Swiftly and surely the hind paw is brought to bear on it, and the acid on the irritated spot is wiped away;* then the leg resumes its comfortable position."

The water grows hotter and hotter, but the brainless frog never moves, till, at 56° C., it expires in a state of tetanus.

This contrast is assuredly marked enough, and most readers will be disposed to admit that if the brainless animal can endure, without manifesting even uneasiness, what in the normal animal produces every sign of intense pain, the conclusion that the brainless animal feels nothing, and therefore that his Spinal Chord is not a sensational centre, is irresistible. This conclusion I altogether reject. Not that I question the facts, for I have verified their accuracy; and Mr. Foster, who has repeatedly verified them, only points to the new difficulty which they raise, namely, why the brainless frog is not excited to reflex action by the stimulus of hot water? It is, therefore, the interpretation of the facts to which attention must be drawn; and to make this complete, let me here note counter facts which my experiments presented.

The brainless frog is *not* insensible to the heat, *unless* the insensibility be gradually produced. If its foot be dipped into the hot water the leg is violently retracted; and if the whole or greater part of the body be immersed, the frog struggles vehemently, and rapidly passes into a state of tetanus. The difference between the behaviour of a normal frog and a brainless frog when *suddenly* immersed in very hot water is not greater than might reasonably be anticipated between animals uninjured and animals with one great sensitive centre removed.

These facts are substantially confirmed by the facts brought forward in Mr. Foster's paper. He also finds the legs of a decapitated frog withdrawn by reflex action, as soon as the temperature of the water reaches a little over 30° C. "However slowly the water be heated, the feet are always withdrawn at a temperature of 35° or earlier." But he observes that when the whole body is immersed and the water gradually heated, no movement, or only the very slightest spasm of the muscles of the legs takes place. The point to which he draws attention is, that whereas the stimulus of hot water applied to the *foot* causes reflex action, applied to the whole leg or the whole body it causes none; his explanation is that the depressing influence of heat on the Spinal Cord destroys its reflex powers. This explanation seems to accord very well with all his observations, but is not in accordance with the fact mentioned by Goltz of the frog's wiping away the acid which is dropped on its back; a fact clearly manifesting the presence of reflex sensibility.

It is this fact which I should urge against Goltz, and all who share his views. It proves, to my mind, that although the frog remains motionless in the heated water and shows no sign of pain from the stimulus of heat, this is assuredly not because Sensibility in general is gone, but simply because Sensibility to *temperature* is gone. It is not necessary to refer to the many well-authenticated cases of analgesia without anæsthesia, of insensibility to pain or temperature without insensibility to touch; I will parallel Goltz's case of the brainless frog suffering itself to be boiled without moving, by the case of the frog with its brain and other nerve centres intact, allowing its legs to be burnt to a cinder without moving. In a paper read at the Aberdeen Meeting of the British Association, I brought forward some experiments on frogs after their skins had been wholly or partially removed. (There were small patches of skin left on the head wherewith to compare the effects of stimuli). These frogs assuredly had not lost their Sensibility; they responded, as usual, to any stimulus applied to the patches of skin which remained; and as these responses were the responses of animals in possession of a brain, no one would explain them away as *mere* reflexes. Yet these sensitive frogs allowed their flayed limbs to be pinched, pricked, cut, burnt with acids, and even burnt to a cinder with the flame of a wax taper, yet remained motionless under all these stimuli, though a touch on the patch of skin would make them wince or hop away.

I did not try the experiment of boiling one of these frogs, but who can deny that the insensibility they presented with their brains and without their skins, is even greater than that presented by brainless frogs with their skins? The point urged is that the frog without its brain is incapable of feeling the stimulus of hot water, which, when the brain is intact, is felt intensely; and the conclusion drawn is that the spinal cord is not a sensational centre. But this point is blunted when we find that the frog is equally insensible to the heat, when its brain is intact and only the skin removed. Ought we to conclude that the skin is the sensational centre? The one conclusion would be as logical as the other.

Mr. Foster, who is only treating of the influence of temperature, asks why the sensations and *cerebral* processes are not dulled in the same way as he supposes the *spinal* processes to be dulled by heat? "The answer," he says, "is that a less intense sensory impulse is needed to call forth a movement of volition, that is, a movement carried out by the encephalon, than an ordinary reflex action, that is, a movement carried out by the spinal cord alone. The water as it is being warmed suggests a movement to the intelligent frog long before it is able to call forth an unintelligent reflex action. The very first movement of the frog, the removal of any part of his body out of the water, increases the effect of the stimulus; for the return of the limb to the water already warm gives rise to a stronger stimulus than contact with the water raised to the same temperature while the limb is still in it; and thus *one movement leads to another* and the frog speedily becomes violent. It is nearly the same with the brainless frog when a movement has for some reason or other been started; only in the observations we have been dealing with this initial movement is wanting."

Let us compare the energetic movements of the normal frog and the absence of movement in the brainless frog, with the energetic movements of a waking man in a suffocating atmosphere, and the absence of movements in the sleeping or stupefied man in the same atmosphere, and all the phenomena are clear. The waking man and normal frog are alert and alarmed. The sleeping man and brainless frog remain motionless. Instead of our being surprised at the brainless frog manifesting so little Sensibility when the gradually-increasing heat is threatening its existence, we ought to be surprised at its manifesting so much Sensibility as a thousand experiments disclose; especially when we see that if the heat be suddenly applied the Sensibility is manifested as *equally* energetic in normal and in mutilated frogs.

In conclusion, let it be observed that unnecessary obstacles are thrown in the way of rational interpretation when connotative terms such as Spinal *Soul* (*Rückenmarkseele*) are adopted. It is one thing to assign a general physiological Property, such as Sensibility, to the nervous centres; another thing to assign a term which is the abstract expression of the connexus of sensibilities, to any one centre. In saying that the Spinal Cord is *a* seat of sensation, it is not meant that it is *the* seat, nor that the sensations are *specifically* like the sensations of colour, of sound, of taste, of smell; but they are as like these as each of these is like the other.

GEORGE HENRY LEWES

THE ARTISTIC REPRESENTATION OF NATURE*

THE late autumn of every year introduces to the public a large supply of gorgeous volumes, "got-up" in lavish fashion with handsome plates and lightly-written letter-press, which are generally spoken of as Christmas Books, and are intended to be the means for the material expression of the generous feelings which that season is supposed to evoke. The work to which we wish to call attention is not intended to be one of these, though its exterior appearance might, at first sight, be thought to warrant the supposition. It is a special work brought out under special circumstances, and, as we are told in the preface, the plates have been engraved for nearly seven years. We refer to it, and shall speak of some of the pictures in detail, as showing the service which Art can render to Science by a faithful representation of Nature. The more scientific Art is, the more successful and the more impressive she will be; only by a thoroughly scientific study of his subject and its surroundings can an artist hope to achieve complete success.

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* "The Life and Habits of Wild Animals" Illustrated by Designs by J. Wolf. (Macmillan, 1873.)

The book derives a special, though a painful interest, from the fact that it contains the last series of illustrations which will be drawn by a highly-talented German artist—Mr. Wolf—the previous productions of whose pencil are so well known to all who find pleasure in the study of the animal world. The volume is illustrated by twenty plates, beautifully engraved by Messrs. J. W. and E. Whymper, each of which depicts some stirring scene in the life of "our four-footed friends," or puts before us some picture of the life of birds, some of them representing in a terribly graphic manner the struggles which pervade the existence of beasts, and render its tenure so precarious. Witness the subject of plate iii.—one of the most powerful in the whole series—the death-grip of the crocodile's cruel jaws upon the handsome head of the tiger drawn slowly and resistingly beneath the stream where the conqueror will make his banquet. There is no one who would not feel, in gazing at this picture, a strong sympathy with that most splendid of the feline tribe in this his death-agony. We do not select this plate as superior in draughtmanship to its fellows; they are all of the same high order of merit, though some naturally arrest the attention more forcibly than others, in proportion as the feelings which connect man *quâ* animal with his fellow-animals find fuller expression with regard to the nobler and higher specimens of animal life.

And here we would say that pictures like these—not mere passive delineations of the outward shapes, but illustrations of the habits of wild animals—have an instructive and suggestive value. They are pictures which set one thinking. There is a dramatic reality about them which leads the mind into the by-paths of contemplation as no still outline can—they irresistibly compel us to compare with ourselves these denizens of the forest and the prairie, of the river and the sea. We seem at once to be impressed with the consciousness of their irresponsibility, of their independence of ethical restraints, obeying as they do but one law—the law of their kind—which incidentally leads them to the destruction of other kinds inferior to their own. The half-human looking ape does not allow us to predicate the conception of morality of any of its actions; the care of its young which it evinces is but an exhibition of the instinct of self-preservation which pervades all species of the higher animal forms; it is difficult to realise that the gap between man and monkey is anything less than a so-called difference of kind. Many other reflections are suggested by a sympathetic survey of such animated drawings as these, but we will not weary our readers with subjective digressions, which must necessarily vary with the individuals who indulge in these reflections; we are only eager to impress the superiority in this regard of delineations of active life and habits over mere portraiture, however well executed, of individual forms of life.

We are glad to be able to reproduce one of the most pleasing of the plates which adorn Mr. Wolf's work—"The Island Sanctuary." There is a peaceful lonely beauty about this representation of the osprey's haunt, which at once arrests the attention and forms a strong contrast with the depictions of the more savage warfare of species against species, of panther against doe, of lion against deer, of wolf against boar, which are contained in the same volume with it. The siesta of the jaguar (plate ix.) and the bath of the large pachyderms, elephants and hippopotami (plate x.), are two of the most striking drawings in this volume, the former especially we think inimitably excellent. There is an idyllic completeness in the representation of the largest of the American cats taking its ease during the midday heat on the branches which overhang the river. Without going into further detail concerning the separate plates, which require to be seen to be appreciated, we would mention one more, Catching a Tartar (plate xviii.), the most sensational in the series, very forcibly drawn, the dead or dying owl's wings have lost their motive power, but in their outstretched hugeness serve to break the rapidity of the descent and save the weasel, whose "cunning has proved more than a match for the strength of the more powerful" bird.

We speak in a somewhat popular strain of Mr. Wolf's work, not with any intention of treating it as one of the hastily concocted products of the winter season, which, as we have said, it is not meant to be, but rather from a belief that it will appeal to those who, without a special scientific or zoological training, have yet a genuine love of contemplating the varied phases of life in beast and bird, who believe with Coleridge, that

> "He prayeth well who loveth well
> Both man and bird and beast,"

and to such as these we can say that this volume is of no common sort; the pictures are such as stir the imagination and please the taste, while, as justly remarked by Mr. Whymper in his preface, their value is greatly enhanced by the "power of delineating specific characters" which is displayed.

We must not omit to mention, in connection with Mr. Wolf's plates, the letterpress which accompanies them, and which is from the pen of Mr. Daniel G. Elliot, of the United States. It is, of course, in this case subservient to the drawings which it interprets. In his outspoken preface, to which we have already referred, Mr. Whymper tells us that Mr. Elliot has laid aside the scientific treatment of his subject, for which he is fully capable, as bearing in mind that "the book is intended for the general public, and not for a class." Our American cousins are always masters of the art of depicting in animated and picturesque fashion all that is of interest in life and action, whether in man or in beast; and Mr. Elliot has not failed in the task set before him; he has steered clear of fulsomeness, and cannot be accused of padding; his writing is instructive with respect to the habits of animals, and is not of that ejaculatory kind which too often accompanies pictures and seeks to impress the character of eloquence by a copious interlarding of interjections. We can give in one quotation a fair example of his portion of the work. Speaking of the gorilla he says:—"In the pathless tracts of those ancient woods, distant even from the primitive abodes of hardly less savage men, in company with the fierce inmates of the jungle, the gorilla dwells, surrounded by his family. Peacefully they pass the day, seeking the various fruits that in many a cluster hang from the lofty trees, paying generally but little attention to what is passing below them. But if any unusual sound breaks the stillness of the woods, or a strange form be seen approaching their vicinity, then the females, bearing their young clinging fast to them, flee away into the still deeper recesses of the forest; while the father and protector of the small community, swinging himself rapidly from tree to tree, tearing loose the vines that stretch across his passing form, advances towards the object of their fears, and before imitating the rest in their speedy flight, satisfies himself in regard to its presence, and then with many a hideous grimace, and short hoarse call, demands to know, in impatient tone, Who comes here?"

THE ISLAND SANCTUARY

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE*

IX.

IN the comparison of standard weights, the difficulty and risk of error in determining the weight of air displaced by them is to be avoided by weighing them not in air, but in a vacuum. Two methods are employed for this kind of weighing.

In the first and simplest method, when an ordinary balance of precision is used, each standard weight is placed in an exhausted receiver just large enough to hold it, and is weighed separately against a counterpoise by Borda's method. Sensible discordances have, however, been found in the results of this method of weighing in exhausted receivers, which render its use inexpedient when scientific accuracy is required. These discordances are perhaps attributable to a small quantity of air being present in the receiver during the weighings, the amount of which cannot be accurately determined. Another probable cause is a change in the temperature and atmospheric pressure affecting the balance itself and the weights in the pans during the long time necessarily occupied in the whole process of this method of weighing. Indeed it may be generally stated as a rule that the risk of discordances in the results of weighings is in proportion to the time occupied in the operation. Such discordances are not found when the weighings are made by the second method, when a vacuum balance is used, that is to say, when the balance case itself is made an exhausted receiver.

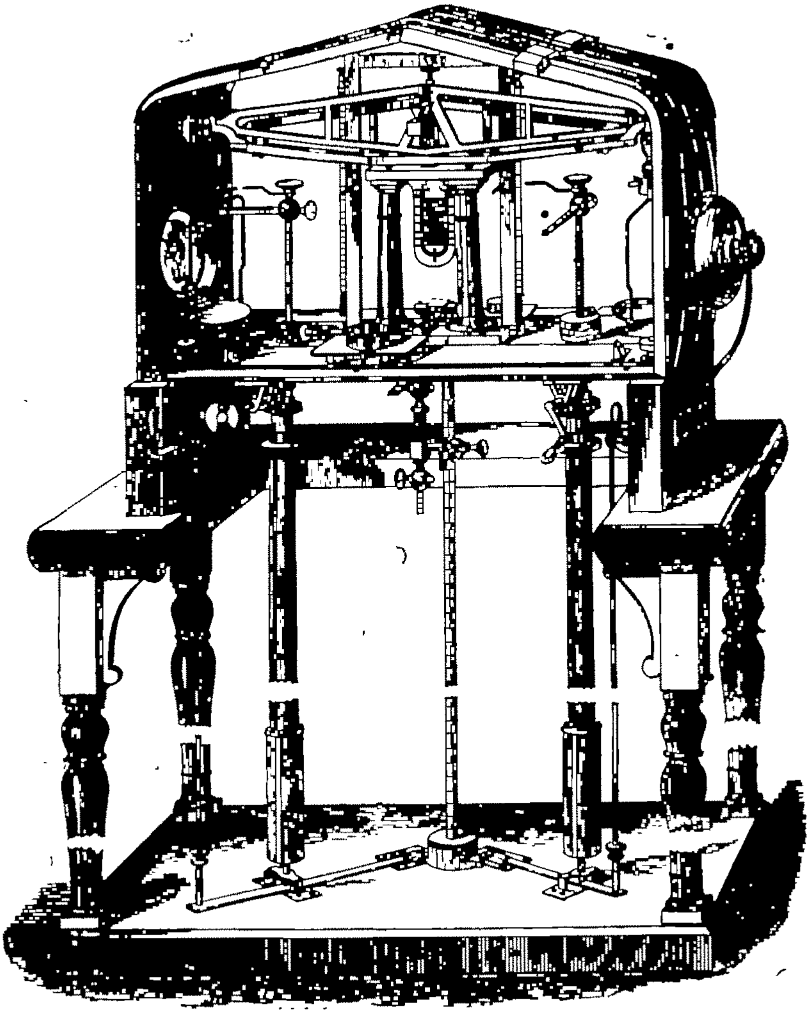

FIG 19.—New Vacuum Balance of the Standards Department.

A vacuum balance has been constructed at Paris by M. Deleuil, and is now used at the Conservatoire des Arts et Métiers, consisting of a balance of the best construction placed in a very strong cast-iron case that can be made perfectly air-tight. This case has four circular openings for giving admittance and light to the inside, which are closed with strong glass covers. It has a stuffing box for the handle of the lever by which the balance is put in action and arrested. This balance has been found to give very accurate results of weighing in a vacuum. But the comparison of standard weights in this vacuum balance takes a considerable time from the necessity of opening the case and re-establishing a vacuum at least a second time in order to change the weights in the pans even when Borda's method is used; and occasionally this must be done again if a small additional balance weight is required to be placed in either pan, in order to obtain a sufficiently approximate equilibrium, so that the pointer may not exceed the limits of the index scale.

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* Continued from p. 49.

Some improvements on Deleuil's vacuum balance have been designed by Prof. W. H. Miller, and have been practically carried out in a vacuum balance constructed by Mr. Oertling for the Standards Department. The balance case consists of a strong brass frame cast in one piece, with a rectangular base, two sides, and an arched top. Two solid glass plates, each 1¾ in. thick, form the front and back of the case, being clamped to plane surfaces of the brass frame, and made air-tight by interposing thin india-rubber. They are thus removable when required, for instance, when any alteration is needed in the balance. There is a circular opening 4⅛ in. in diameter, on each side of the brass frame, similar to those on Deleuil's balance, to which glass covers are fitted. There is no stuffing-box, but when the Standard weights to be compared are placed in the pans, and the balance case exhausted, contrivances are arranged for putting the balance in action and arresting it, for adding any balance weights to either pan and removing them, and for interchanging the pans and weights by transferring them to the other end of the beam, without any disturbance of the vacuum, or necessity of opening the case.

These arrangements enable the weighings to be made by Gauss's method of alternation. The balance case is firmly placed upon a strong mahogany stand. Two iron tubes are fixed underneath and opening into the balance case. They rest in iron cups containing a sufficient quantity of mercury. Within each tube is a steel rod rising to the required height inside the balance case, and having at the top an arm of convenient form. By means of a simple lever handle outside the tube, either rod can be lifted about an inch, and it can also be turned round. By this rotary motion, when the left-hand rod is in its normal position, it acts upon two bevelled wheels, and thus lowers the supporting frame of the beam and puts the balance in action; and by reversing the motion, the action is stopped. By raising either rod to nearly its full height, it can be made to take up one of several small balance weights riding on a little rail fixed to the pillar of the balance, and transfer it to a similar rail at the top of the pan, or to transfer it back again. Again by raising either steel rod to an intermediate height, and turning its arm under the arched rods of one of the pans, and then raising it a little, the pan and weight can be lifted off the hook of the beam and transposed to a small carriage standing upon a railroad near and parallel to the front of the balance-case. In a similar way the other pan and weight can be transferred to a second carriage at the back of the case. By means of a cord and pulleys, one of which is upon the right-hand steel rod and can thus be turned round with the hand, the two carriages can be moved to the other ends of the case, and then each pan with its weight can be attached to the hook at the other end of the beam. The desired results are all thus attained, and the whole action of the balance is open to view.

The construction of this new vacuum balance may be seen from Fig. 19.

The balance itself is similar in construction to the other Standard balances made by Mr. Oertling. It is constructed to weigh a kilogram in each pan. There are two Standard thermometers inside the case, one fixed to each pillar, and adjustible as to height, so that its bulb may be on the same level as the centre of gravity of the weight. A mercurial gauge is fixed between the pillars, and there is the same arrangement of three tubes and stopcocks communicating with air-pumps and with a mano-barometer, as in Deleuil's vacuum balance. Two glass vessels containing chloride of calcium, are also introduced for absorbing any moisture in the balance case.

There are two ways of comparing and verifying standard measures of capacity. The first and most accurate, as well as scientific method, is by weighing their contents of distilled water; the second method, which is simpler and more ordinarily used, consists in comparing the measure of water contained in them, with the contents of a verified standard measure.

In weighing the contents of distilled water contained in a standard measure, when quite full to the brim, and with the surface of the water made accurately level by a disc of plate glass slid over it, Borda's method of weighing is employed. The measure with its disc is placed empty in one of the pans of the balance, and is accurately counterpoised. A verified standard weight equal to the legal weight of water contained in the measure is then added to the pan containing the measure and disc, and is accurately counterpoised, and a sufficient number of weighings is taken until the mean resting-point of the balance is determined and noted. The standard weight is then removed. The measure is exactly filled with distilled water, and its temperature, together with the reading of the barometer noted. Any difference in the actual temperature and barometric pressure from the normal temperature and pressure is to be compensated by equivalent weights placed either in the measure pan or weight pan as required. If an equipoise is not now obtained, additional weights are placed in the pan until an equilibrium is produced, and any difference from the normal correcting weight for temperature and barometric pressure either plus or minus, shows the error of the measure in relation to its legal weight of water at the standard temperature and barometric pressure.

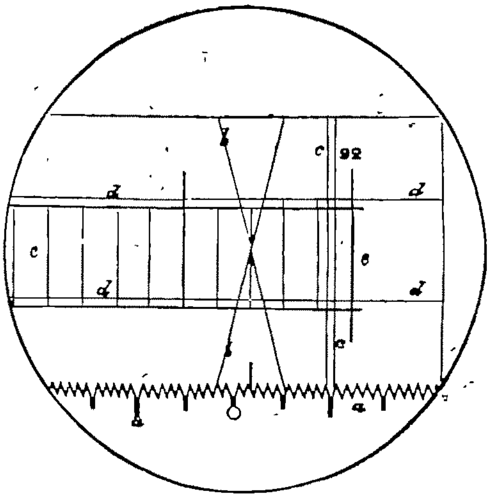

FIG. 20.—Field of Micrometer of Microscope.

For ascertaining the exact amount of the proper corrections for temperature and barometric pressure, authoritative tables are computed both for Imperial and for Metric Measures. Such tables will be found in the Papers appended to the Fifth Report of the Standards Commission, published in 1871 (pp. 81, 193, and 196), and to the Sixth Annual Report of the Warden of the Standards, published in 1872 (pp. 49 and 51).

With regard to comparing instruments for standard measures of length, their construction has necessarily varied according to the form of the standard measure. As has been already stated, the earlier scientific standards of length were defined by two points, and all comparisons were made by means of a beam compass.

The introduction by Mr. Troughton of the use of the micrometer microscope was a great step in advance towards the attainment of scientific accuracy in the comparison of our standard measures of length. It enabled optical observations to be made without injurious contact to the defining points or lines, and thus without interference with the permanence of the measures. Several descriptions of comparing apparatus with micrometer microscopes have been constructed at various times, but all are made upon the same principle. The microscope is fixed in a vertical position, and is provided with a spirit level and with screws for accurate levelling and focal adjustment. The defining marks of the two standard measures to be compared are brought successively under it, their height being adjusted to the focal distance of the microscope. Any difference of length between the defining marks of the two measures is read off from the graduated head of the micrometer. This part of the apparatus consists of an endless screw with the very finest threads, having a large head divided into 100 parts. The screw is placed in a horizontal position, and when turned carries with it a nut moving in horizontal guides, together with an open frame, which has cobweb lines stretched across it. Two of these lines (*b b* Fig. 20) cross each other at equal angles to the axis of the screw, and so that a line bisecting them is normal to its axis. Two other lines (*c c*) are placed nearly close, and parallel to each other and normal to the axis of the screw; and there are two longitudinal lines (*d d*) parallel to the axis of the screw, by means of which this axis is made parallel to the axis of the measure under observation. When turning the screw, the number of revolutions is read off by the aid of a pointer from a rack (*a*) placed at the edge of the open frame and parallel to the screw, whilst the number of divisions in one revolution is read off on the graduated head of the screw, from a fixed line marked on the upper surface of the microscope. Looking through the eye-piece of the microscope at the magnified first ten hundredths of the inch 36—37 marked on the subdivided standard yard of the Standards' Department (here inverted), the field of the microscope is seen as represented in Fig. 20.

In this figure the cross lines are used for observation, and are seen adjusted to the 0·03 in. line. The pointer at the rack shows the screw to be turned between one and two revolutions from the middle of the field.

All micrometer microscopes used for the comparison of standard measures of length are constructed upon the principle thus described. But there are various kinds of arrangements for supporting the standard measures in a proper position, and for more conveniently bringing their defining marks under the microscopes. Under one of the arrangements, a single micrometer microscope is used, and fixed over the supporting apparatus, which, for the purpose of comparison, has both a transversal and a longitudinal displacement.

The two standard measures (denoted as A and B) being placed with their axes exactly parallel, and their defining marks as nearly as possible in the same line normal to the axes, the left-hand defining mark of A is brought under the microscope, and the position of the micrometer read off on the index scale and noted. By the transversal displacement, the left-hand defining mark of B is next brought under the microscope, and the reading of the index scale noted. The two measuring bars are then moved their whole length by longitudinal displacement, and the right-hand defining marks of A and B successively read off and noted, thus affording the means of ascertaining the difference of length of the two standard measures. The temperature of the bars at the beginning and end of the observations must be determined by thermometers, and the mean temperature noted, and allowance must be made by computation for any difference of length arising from unequal expansion or contraction of the two bars, when this temperature differs from the standard temperature. For this purpose it is absolutely necessary that the coefficient of expansion of each standard bar must be previously determined.

This method of comparing with a single microscope is used in France, but not in England, where the risk of error arising from the longitudinal movement of the bars is avoided by using two microscopes, and only a transversal displacement of the bars during the observations, although there are also means of longitudinal displacement for the purposes of adjustment. The objection raised against the use of two microscopes, that the distance between them may vary during the period of observation by the expansion or contraction from alteration of temperature of the material which unites them, is obviated by fixing them firmly and independently upon a solid stone support.

Placing measuring bars directly upon a plane support is objectionable. It has been proved that there is a risk of discordances in comparisons being caused by almost undiscoverable inequalities in planed surfaces, as well as by a difference of temperature in the plane surface and the under surface of the measuring bar, when thus placed. To guard against this risk, the bars are supported upon rollers, and the measuring bars ought to be stiff enough to bear to be supported upon a few points at which rollers can be conveniently applied. For a short bar two rollers are sufficient; for a longer bar more supports are required. The standard yard bars are supported upon eight rollers, and it is always requisite that each support should exert the same vertical pressure upwards, in order that the interval between two points upon the surface of the bar may not be altered by the flexure. This object is attained by a proper arrangement of levers; and it is easily seen that an arrangement of levers by which equal pressure upwards may be exerted at four or eight points is very simple. Each bar rests upon two brass lever-frames.

It has been shown by the Astronomer Royal, in his paper printed in the Royal Astronomical Society's Memoirs, vol. xv., that the value of the intervals (supposed equal) which ought to exist between different supports of a bar, each support exerting the same vertical pressure upwards, is as follows: n being the number of supports, the resulting intervals of supports is:—

$$\frac{\text{length of bar}}{\sqrt{(n^2 - 1)}}.$$

In order to ascertain with scientific precision how far the results of comparisons of standards obtained by the use of weighing and measuring instruments are to be depended upon for their accuracy, a calculation is to be made of the probable error of every such result, whether it be the result of a single comparison, or the mean result of any number of comparisons. And when other elements are to be taken into account, it is necessary that the probable error of each computation should be determined and allowed for before the final results of comparison can be determined and allowed for.

The mode generally adopted for calculating the probable error is based upon the method of least squares, and is fully stated by the Astronomer Royal, in his "Theory of Errors of Observation," pp. 44-7.

H. W. CHISHOLM

EARTH-SCULPTURE [*]

II.

YOU are aware that the revival of the half-forgotten doctrines of the early Scottish School of Geology has not been without vehement protest on the part of the older geologists, who have been inclined to treat them rather as novelties and departures from the older and purer faith. No one resisted them more determinedly than my much-missed friend and benefactor, the late Sir Roderick Murchison. He looked with regret, and even, perhaps, sometimes with a little alarm, upon their advance, and to the last he battled against them. He was, indeed, in this country the leader of his party, which has been called the "Convulsionist School," and his death has, doubtless, been a severe blow to that school, as it has been a loss to all who admired a straightforward, courteous, and undaunted antagonist.

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[*] Opening Address to the Edinburgh Geological Society, by Prof. Geikie F.R.S. (continued from p. 52).

Other members of the party have, however, in more or less direct ways, lifted up their voices of protest. I select this evening one of these antagonists, partly because he has spoken more and more energetically than any other, and partly because a good deal of his speaking has been directed against myself. And here I am sorry that I must begin by a reference to a matter of personal history. In the summer of 1865 I published a little volume, now out of print, on "The Scenery of Scotland, viewed in connection with its Physical Geology." The object of that work was to show how completely the Huttonian doctrine of earth-sculpture was borne out by the mountains and valleys of the northern part of this island. I distinctly disclaimed any novelty or originality on my own part in the broad doctrine which I tried to enforce. My veneration for Hutton and Playfair had been from boyhood profound; again and again in the pages of my book I quoted them, and spoke of them as the founders of the school to which I professed myself a loyal adherent, and in which I could boast such friends and colleagues as Jukes and Ramsay.

I was well aware, and stated in the preface, that the views to which I had been led "ran counter to what are still the prevailing impressions on this subject," and that I was prepared to find them disputed, or thrown aside. Convinced, however, of their essential truth, I looked forward to a time when what might then be regarded as mere dreaming would be established as a recognised part of the groundwork of geology. The views put forward in the volume met, indeed, with an amount of general acceptance which I could hardly have anticipated. But at length the expected opposition made its appearance.

On February 3, 1868, his Grace the Duke of Argyll read to the Geological Society of London a paper, entitled "On the Physical Geography of Argyleshire, in connection with its geological structure." Although that title was chosen, the paper proved really to be from beginning to end a criticism of my little book, which, indeed, the author candidly acknowledged to have served him as "the best text he could find."

To that paper I made no reply. It seemed to me that the noble author had failed to perceive the bearing of the whole argument from geological waste, as proved by geological structure. His objections being already, in my opinion, anticipated in the book which had called them forth, I did not see how I could make my case plainer by any amount of additional argument. But further, his Grace had begun his communication with a sentence in which he stated that the views set forth by me "seemed to be gaining ground with the younger school of geologists,"—fatal admission, as it occurred to me, for I felt that what was called the younger school must eventually take the place of that which styled itself the older, and that if it remained true to its belief, the views which were now called in question would carry the day without any battling of mine. Every month shows more fully the justice of this anticipation.

I was content to let the matter rest; nor would I recur to it now, but for the following reasons. Since that time the Duke of Argyll has become President of the Geological Society of London. In his recent address, and in a separate communication to the Society, he has returned to the subject of the origin of the present features of the land, referring to his former paper as "an argument which had not been met by any answer in detail," and adhering, therefore, to the views there expressed. As to the non-appearance of any "answer in detail" from myself, I can give no other explanation than that I considered my little book sufficiently detailed for its purpose, and believed that it already anticipated and answered the argument of my opponent. That is still my belief.

But a broad challenge addressed to the general body of geologists by the President in the official Address which he annually gives to the Society and the world, is not the same thing as a criticism from one member of the Society upon the work of another member. In the interests of science, therefore, it seems to me that some protest is now called for against doctrines promulgated at this late date in the century from so high and honourable position as the Chair of the Geological Society of London; and as I have been especially singled out for attack, it appears to me to be only an act of duty to vindicate, not my own position merely, but the reputation of that "younger school" which is accused of seeking to pervert the geological mind from the ancient and true creed. If these doctrines maintained by the President were to become generally diffused, which, happily, is now impossible, they would suffice to paralyse research in one important branch of the science; for, as far as relates to the history of the configuration of the land, they would assuredly bring down upon us again the pre-Huttonian darkness.

No one whom the Geological Society of London has chosen as its President can fail to command the respectful attention of geologists all over the world. And while I gladly acknowledge this right, I would also express the gratification which is widespread among the brethren of the hammer in this country that the Duke of Argyll, in the midst of so many and so onerous, as well as honourable duties, should find time to take a lively and active interest in the progress of geology. I admire, too, the vigour with which he wields his pen, and the boldness with which he gives his judgment among disputed questions. He has once more thrown down his geological gauntlet, and if I venture to take it up, and accept his battle, it is in the full consciousness of the presence of an adversary who, while dealing hard blows himself, will take in good part such buffets as the fortunes of war may bring to him.

I have already alluded to the natural impression that when we look at a region of rough mountains formed out of hardened and contorted rocks, we behold in the external outlines the direct results of the subterranean force by which the rocks were altered and crumpled. This obvious inference is far older than the days of geological inquiry. But surely its mere obviousness is no argument for its truth, any more than the rising and setting of the sun prove the earth to be the centre of the universe. In the volume already referred to I spoke of it as "dealing with that dreamland of conjecture and speculation lying far beyond the pathways of science, where one has no need of facts for either the foundation or superstructure of his theory. It thus requires no scientific knowledge or training; it can be appreciated by all, and may be applied to the history of a mountain chain by one to whom the very name of geology is unknown." But to recognise that this common and instinctive notion is yet a misleading one, requires an acquaintance with geological structure which comparatively few have an opportunity of obtaining, and which appears to be not always readily acquired at second-hand. I have watched the current geological literature on this question during the last decennium, and the result has been to convince me that the notion, or rather the prejudice which I am combating, is in some minds so deeply rooted that it cannot be got rid of by the reading of any number of books or treatises, and, of course, still less by the writing of them. Simple as may be the statement of the leading principles and facts relative to that waste of the earth's surface to which the term Denudation is applied, there is yet, I firmly believe, no part of geology more difficult adequately to realise. So striking are the difference and contrast between the magnitude of the results adduced and the apparent insignificance and impotence of the forces which are alleged to have produced them, that the mind not unnaturally hesitates to associate the one with the other in the relation of cause and effect. And yet it is only in proportion as one is enabled to master this subject that he is prepared to understand, far less to discuss the origin of the present contours of the land.

In the volume which the Duke of Argyll has singled out to bear the brunt of his attack, I carefully stated at the commencement that I proposed to consider the problem only "in so far as it relates to the history of the scenery of Scotland." I laid down no universal law or dogma by which the hills and valleys of every other part of the world were to be explained. I knew the mountains and glens of Scotland; I had wandered over them and studied them from boyhood; trained in the severe and laborious school of the Geological Survey, I had mapped many hundreds of square miles of their surface, across some of the most complicated pieces of geological structure in the kingdom. It was not, therefore, in any spirit of rashness, or novelty, or dogmatism, but with the growing convictions of many years of experience and in the belief that a service to the cause of geological inquiry in this country could be done, that I ventured to launch my little book upon the world. I was well aware that other regions exhibited features not seen here, and that for these other explanations might require to be found. But it was then no part of my subject to travel beyond my own domain. When the principles for which, in common with my able colleagues in the Survey, I contended were firmly established in relation to the scenery of this country, it would then be time to consider how far they were applicable elsewhere. That they would be found to be not merely of local but of wide general import I then held to be probable, and I now know to be profoundly true.

One main object of my chapters was to show how the present hills and valleys of Scotland had come into existence gradually, one by one, during an enormously protracted period of geological waste in the manner to which I have already referred this evening. I adduced copious proofs from all parts of the kingdom in support of this view, similar proofs having been already triumphantly accumulated by Mr. Jukes in Ireland, and by Prof. Ramsay and others in England.

Far from ignoring the influence of geological structure upon external form, I might even have been charged with having brought forward a needlessly ample accumulation of evidence to show how constantly the resulting contours of the country have been determined by the arrangement of the rocks. I showed how ancient, in a geological sense, the denudation of the country had been, and how thoroughly it had done its work upon the surface, no matter whether the rocks had been originally formed as mere soft mud or had been once in actual fusion. I dwelt on the remarkable fact that as a rule the valleys do not run along lines of fracture, and quoted in support of this assertion the published maps of the Geological Survey of the three kingdoms. To these and similar statements of sober fact which are now part of the common stock of geological knowledge, his Grace opposes such phrases as these: "The facts assumed are, in my opinion, to a large extent purely hypothetical," "This assertion is erroneous," "extravagant demands," "inventions and imaginations," and so on.

(*To be continued.*)

NOTES

THE annual meeting of the Fellows of the Royal Society was held on Tuesday at Burlington House. The retiring President, Sir George Biddell Airy, K.C.B., delivered the inaugural address. The presentation of the medals followed. The Copley Medal was awarded to Professor Helmholtz, the distinguished physiologist, physicist, and mathematician, of Berlin, "whose memoirs have ranged through nervous physiology, hydrodynamical theory, instruments (as the ophthalmometer and the ophthalmoscope) for exact measurement and for medical examination of the eye, and other important subjects, and have been generally recognised as giving real additions to our knowledge." A Royal Medal was awarded to Prof. Allman, F.R.S., "for his numerous zoological investigations, and more especially for his work upon the Tubularian Hydroids. The subject of these labours is one upon which few persons are qualified to enter; and the Council are impressed with the delicacy of the work and the value of the scientific results." A Royal medal was awarded to Professor H. E. Roscoe, F.R.S., of Owens College, Manchester, "for his various Chemical Researches, more especially for his investigations of the Chemical Action of Light, and of the Combinations of Vanadium." Dr. Joseph Dalton Hooker, C.B., was elected President of the Society.

THE alleged reply of the Government on the subject of an Arctic Expedition as reported in the daily papers (*Daily Telegraph* and *Pall Mall Gazette*) is calculated to convey a very erroneous impression. Mr. Gladstone has requested that he may be furnished, in writing, with the reasons for the despatch of an Arctic Expedition, before receiving a deputation on the subject. Those reasons, which we believe to be quite conclusive as showing the propriety of despatching an expedition next year, will at once be furnished to the Prime Minister.

PROF. A. W. WILLIAMSON has been elected a Correspondent of the French Academy.

THE Duke of Northumberland has been unanimously elected President of the Royal Institution, in succession to the late Sir Henry Holland.

THE probable arrangements for the Friday Evening Meetings of the Royal Institution before Easter 1874, are as follows:— Jan. 16: The Acoustic Transparency and Opacity of the Atmosphere, by Prof. Tyndall, F.R.S. Jan. 23: Recent Discoveries in Mechanical Conversion of Motion, by Prof. Sylvester, F.R.S. Jan. 30: Weber and his Times, by Sir Julius Benedict. Feb. 6: The Heart and the Sphygmograph, by Alfred H. Garrod, Fellow of St. John's College, Cambridge. Feb. 13: The Opponents of Shakespeare, by Dr. Doran, F.S.A. Feb. 20: The Autotype and other Photographic Processes and Discoveries, by Vernon Heath. Feb. 27: Men of Science, their Nature and Nurture, by Francis Galton, F.R.S. March 6: Venus's Fly-trap, by Dr. J. S. Burdon-Sanderson, F.R.S. March 13: Graphic Representations of Musical Sounds, by M. Cornu. March 20: The Temperature of the Atlantic, by Dr. W. B. Carpenter, F.R.S., Registrar Univ. Lond. March 27: The Physical History of the Rhine, by Prof. A. C. Ramsay, F.R.S., Director of the Geological Survey of Great Britain.

SIR SAMUEL BAKER has quite recovered from his recent indisposition, and will on Monday next address the Royal Geographical Society upon his adventures in Africa.

WE regret to announce the death of M. De La Rive at Marseilles on Nov. 27, on his way to Cannes. He had had an apoplectic fit about a fortnight previously, from which he seemed to be slowly recovering, though greatly shattered in intellect.

WE rejoice to learn that at a convocation held at Oxford on November 27, the grant alluded to in NATURE a fortnight ago in connection with Dr. De La Rue's gift of astronomical apparatus to the University, was acceded to in a manner creditable and gratifying to all concerned. Thus the University has, we believe, established the foundation of what ought to become a very useful Observatory for Astronomical Physics. One immediate result, we hope, will be to excite Cambridge into vigorous action. Oxford deserves great credit for the efforts she has made during the past few years to encourage the study of physical science; we hope the results will lead her to do so to a still greater extent.

THE fund being raised for the purpose of providing a suitable memorial to the late Prof. Sedgwick, of Cambridge University, reaches nearly 10,000*l.* The form of the testimonial will be, some new and suitable buildings for the schools of geology, and a full-length statue of the late professor.

THE Cape mail brings word that the *Challenger* has arrived at Simon's Bay. On her voyage from Bahia she touched at Tristan d'Acunha, and made a survey of the groups of islands to which it belongs. Two Germans were found who had lived there for a couple of years, and who gladly availed themselves of the opportunity of leaving.

THE annual course of lectures of the Brown Institution, under the Government of the University of London, will be delivered in the theatre of the University by Dr. Burdon-Sanderson, F.R.S., on successive Tuesdays and Fridays during the present month, at 5 o'clock in the afternoon. The first lecture will be given on Tuesday next the 9th inst.

PROFESSOR E. WEISS, of the Vienna Observatory, we learn from the *Bulletin International* of the Paris Observatory, has identified the comet recently discovered by Coggia, with the first comet of 1818, discovered by Pons at Marseilles.

WE understand that the Lords of the Privy Council on Education have decided to unite the Professorships of General and Applied Chemistry in the Royal College of Science, Dublin, and that this joint professorship will be conferred on Mr. Galloway, for many years the Professor of Applied Chemistry to the College. The only vacancy to be now filled up in the college staff is therefore that of the Professorship of Zoology.

PROF. N. L. SHALER, Geologist of the State of Kentucky, in a recent letter to the *Frankfort Yeoman*, makes a rather novel suggestion for improving the navigation of the Ohio River, and at the same time preventing the enormous destruction of property which its floods now occasion at intervals, by washing away its banks. In what has hitherto proved a vain endeavour to accomplish the former object, a large amount of money has been already spent under appropriations of the United States Congress, for wing-dams and other structures to concentrate the flow during the season of slack water; and schemes have been considered with more or less favour that involved the expenditure of from ten to forty million dollars. The waste by floods, of property bordering the river, is estimated by Prof. Shaler at 400,000 dols. per annum. He thinks that both objects could be accomplished by simply planting willows upon the banks, as he finds that wherever such a plantation has been effected, the resulting growth not only holds the soil in which it is rooted, but accumulates that which is brought down by the river. When the banks have been sufficiently strengthened and extended by means of such plantations, a deepening of the channel must result, which will improve navigation. The entire cost of planting the banks of the river from Pittsburgh to its mouth is estimated by Prof. Shaler at 100,000 dols.

ON Monday, Nov. 24, a meeting of the Royal Geographical Society was held in the theatre of the University of London, Burlington Gardens; Sir Bartle Frere in the chair. Two papers were read—one by Capt. J. Moresby, R.N., "On recent discoveries at the eastern end of New Guinea," and the other by the Rev. W. Wyatt Gill, on three visits to New Guinea. Capt. Moresby's paper entered at much length into the configuration and aspect of the country, which the author described as not unlike that of Australia. From all he saw of the people, the old idea that they were the most savage of all races must be abandoned. Capt. Moresby's paper described the utensils used by the natives, and looked forward to a better future for them in consequence of their connection with England. The Rev. Mr. Gill then related his experience, which in general confirmed that of Capt. Moresby.

THE late Mr. Robert M'Andrew, F.R.S., of Isleworth House, Middlesex, has bequeathed to the University of Cambridge a very large and valuable collection of recent shells. The collection is one of great scientific interest, and is well known to persons engaged in the study of this branch of natural history. Mr. M'Andrew also bequeaths to the University "such of the purely conchological works in my library as the Vice-Chancellor or any Professor or other official nominated by him shall select, provided they are works which the said University does not already possess (otherwise than in the Public Library of the said University), and such works are to be placed in the Natural History Museum or some library connected with it."

A CORRESPONDENT asks whether any of our readers can inform him if there exists any description of a fine section of Rhætic beds which is to be found about half a mile outside the town of Newark-upon-Trent?

WE have received copies of the *New York Tribune* for October 29, 30, 31, containing full reports of the recent meeting of the American Academy of Sciences, in New York. The reports are very detailed, and have evidently been prepared with great care for the *Tribune*, which, moreover, to judge from the numbers referred to, seems to devote something like one-third of its space to matters more or less connected with Science, not to mention literature. We fear this would not pay in this country; it evidently does in America. The American Academy, appears to be a kind of select upper Association for the Promotion of Science. It started with fifty members, and adds only five new members each year; there seems to be but little pre-arrangement as to the meetings.

THE earthquake on the 9th November, in Western Asia Minor, was rather remarkable. It was felt at 10 A.M. at the Dardanelles and Broosa. It reached to Ak Hissar, Phocæa, and the islands of Samos and Nisyros, in fact from N. to S. At Smyrna a first shock was felt at 9.49 P.M., and another at 3.20 A.M. [of the next day?]. After the first shock a strong smell of sulphur pervaded the atmosphere and entered the houses. A thick mist which had hung about for days dispersed, and the night was clear. Nisyros was supposed to be the centre. At the Dardanelles the shock was preceded by a rumbling noise. At Broosa there was a second shock at 1 P.M. An earthquake was felt on October 10, at 4.45 A.M., at San Salvador, in Central America. It was slight.

THE naturalists connected with the U.S. Yellowstone Expedition of the summer of 1873 have all returned from the field, and are at present engaged in preparing reports for transmission to the Secretary of War. The opportunities furnished by the occasion were not so good as had been hoped for, the region proving to be much more destitute of animal and vegetable life than anticipated. Everything was done, however, by them that the circumstances would allow. The collections embrace a full series of everything met with in the form of animal and vegetable life. The collections of butterflies and of plants were especially rich; of fossils not many were obtained, but among them will doubtless be found some new species. Among these was a large ammonite, 3 ft. in diameter, presented to the party by Lieut. P. H. Ray. A few uncharacteristic bones of fossil vertebrates were picked up, but the expedition failed to reach any of the great bone deposits of the Mauvaises Terres, as they had hoped to do.

WE have recently had occasion to notice the fact that the plans of the new observatory at Cincinnati, U.S., had been approved, and were about being carried into execution. It gives us pleasure to record the rapid progress that has been made in this work, as evinced by the fact that on the 28th of August the corner-stone of the new building now in process of erection on Mount Lookout was laid with becoming ceremonies. The site chosen for the new observatory is about four miles north-east of that on Mount Adams, where the original observatory, founded by Prof. O. M. Mitchell, was established. The corner-stone that was laid in 1843 on that elevation by John Quincy Adams has been carefully removed to the new site, and appropriately forms the corner-stone of the new equatorial pier. The observatory has, by means of a tripartite agreement with the city and the heirs of Nicholas Longworth, now passed into the hands of the Cincinnati University. The proceeds, amounting to 50,000 dols., realised on the sale of the property on Mount Adams have been invested for the support of the art department of the university. The city, however, has pledged itself to maintain the observatory when once established, and the establishment has itself been hastened by the liberality of Mr. John Kilgour, who has given four acres of ground as a site for the new building, and added 10,000 dols. for the latter. The site is admirably adapted for the purpose of the institution. It is one of the highest points in the county, commanding a beautiful and extended view, and it is not likely that the difficulty experienced at the old site from the smoke and vapours of the city will for a long time if ever, trouble the astronomers on Mount Lookout. The new edifice faces south, having a width of about sixty feet, a depth of ninety feet, and two wings, making the breadth through the wings about one hundred feet. One of the wings will be used for the meridian instruments; and in the centre of the building, on a brick pier thirty-six feet high and seventeen feet in diameter, will rest the big telescope. The building will be two stories high, except in the centre, where the revolving turret of iron for the equatorial will add half a story. The structure is to be of pressed brick, with freestone trimmings.

THE additions to the Zoological Society's collection during the last week include an Arabian Baboon (*Cynocephalus hamadryas*) from Arabia, presented by Miss Sandon; a Wild Cat (*Felis catus*) from Scotland, presented by Sir T. Riddell, Bart.; three Gray's Terrapins (*Clemmys grayi*), and some Moorish Tortoises (*Testudo mauritanica*) from Persia, presented by Hon. E. Ellis; an African Goat (*Capra hircus*) from Bedah, presented by Mr. J. A. Croft; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Lady Stirling; two Blue-throated Parrots (*Pionus sordidus*) from Venezuela; an Active Amazon (*Chrysotis agilis*) from Jamaica, and a Blackish Sternothere (*Sternothoerus subniger*) from Madagascar, purchased.

SCIENTIFIC SERIALS

THE *Journal of Mental Science*, October 1873. This journal is still occupied with only medico-psychological subjects. The Morisonian Lectures on Insanity for 1873 begun in this number are of great interest, and mark the advance of Science in this painfully important branch of knowledge. Nothing, we think, can be more evident than that Dr. Skae proceeds on a scientific principle when he attempts to classify the various forms of insanity according to the bodily disease or condition, as far as it can be ascertained, which proceeds or accompanies the insanity. And it is surprising that even Dr. Maudsley should be found among those who cavil at Dr. Skae's classification, instead of adopting his principle and making the most of it. With insanity Science has made a beginning, but that is all.—In an article by Dr. J. T. Dickson on "The Functions of Brain and Muscle Considered in Relation to Epilepsy" we have a rather singular hypothesis concerning the functional relation of the brain to the muscular system. We cannot afford to indicate this curious theory; we doubt if we quite understand it; but we can inform the scientific world generally, on the authority of Dr. Dickson, that what they have been in the habit of believing on this subject "is not only improbable, but impossible." Dr. Hughlings Jackson has, it seems, been at the pains to quote against Dr. Dickson some passages from Herbert Spencer's Psychology; but he could have little known with whom he had to deal. Dr. Dickson quietly remarks—"From this it would seem that Spencer holds somewhat the same, though the untenable view." Was there ever a finer example of how completely original ideas can free a mind from the degrading thraldom of authority? Does Spencer differ from me? why then that is the worse for Spencer.—The article of most general interest is "The Morbid Psychology of Criminals," by Dr. D. Nicolson, continued from last number, and still unfinished. It abounds in valuable observations, and good practical common sense. When in prison criminals offer good opportunities for observation, but we do not perceive that their "emotional displays" can with strictness be said to mark anything specially morbid. From all that is said, we cannot gather more than that criminals are like the much larger class to which they generally belong, namely people of a low type of mind. The unfortunates that find their way into our prisons are, we regret to think, far from the only people who cannot help insanely accusing others of wicked designs against them; whose minds are lawless and undisciplined; who must have their "breakings out;" and for whom, when they become intolerably insolent and violent, "a good drubbing on the spot" would be the most appropriate medicine. People, when inclined to what they ought not to do will not be deterred by the fear of punishments that are not painful, or which are too distant to act on their dull imaginations. This leads to large considerations, but we can only say that it would be a great matter for social progress if our tender-hearted philanthropists—those who busy themselves with theories of home, school, and prison discipline, distributing gratis wonderful receipts for the painless cure of all bad habits—could be brought to understand a little better than they do the real nature of the material on which they have to work. The review of the Lunacy Blue Books will be found interesting; also "Antiquarian Scraps relating to Insanity," by Dr. T. W. McDowall.

Journal of the Royal Geological Society of Ireland, vol. xiii., Part 3, for the session 1872–73, contains E. T. Hardman on the occurrence of gypsum in the Keuper Marls, near Coagh, Co. Tyrone.—Prof. T. Rupert Jones, on some Foraminifera from the chalk of the North of Ireland.—P. S. Abraham, notes on the geology of the Hartz.—Prof. Macalister, a description of two Veddah skulls, and Presidential address (which latter gives an able summary of the work done by German petrologists with the object of determining the mineral constitution and structure of plutonic, metamorphic, volcanic and other rocks by the aid of the microscope).—Prof. E. Hull, on the microscopical structure of the Limerick carboniferous Trap Rocks, and on the microscopical structure of Irish granites.—Col. Meadows Taylor, the Coal fields of Central India.—R. J. Cruise, Analysis of the Leitrim coal, remarks on the coal area of the district.—Dr. Studdert, on the Lough Allen coal from the Arigna District, Co. Leitrim.—G. H. Kinahan, on the carboniferous ingenite rocks of the County Limerick.—E. T. Hardman, on the occurrence of siliceous nodular brown Hæmatite (Gothite) in the carboniferous limestone beds near Cookstown, Co. Tyrone, &c., and on an analysis of white chalk from the County of Tyrone, with notes on the occurrence of zinc therein.—Rev. Dr. Macloskie, on the silicified wood of Lough Neagh.—Dr. Tichborne, on the formation of crystalline minerals having the spherical form.

THE 2nd and 3rd numbers of the 7th volume of the *Canadian Naturalist* commence with a paper by Dr. Dawson on impressions and footprints of aquatic animals and imitative markings on carboniferous rocks, those considered being invertebrate. The paper originally appeared in *Silliman's Journal*.—Mr. G. F. Mathew continues a description of his impressions of Cuba, and enters into detail respecting the botany of the island.—Mr. Whiteaves gives an account of a deep-sea dredging expedition round the island of Anticosti, in the Gulf of St Lawrence, in which upwards of 100 species of marine invertebrata new to the Gulf of St. Lawrence were added to the previously recorded fauna.—Dr. Dawson also contributes a paper on the geological relations of the iron ores of Nova Scotia, considering first the bedded ores of the Lower Helderberg series, and of Nictaux and Moose River; next the veins of iron ore of the East River of Pictou, Shubenacadie, and other parts.—Dr. Nicholson, of Toronto, describes some new fossils from the Devonian rocks of

Western Ontario, including *Zaphrentis fenestrata* (n.s.) *Blothrophyllum approximatum* (n.s.); *Heliophyllum colbornensis* (n.s.); *Petraia logani* (n.s.); and *Alecto canadensis* (n.s.).—A detailed report is given of the meeting of the American Association for the Advancement of Science, of which an abstract has already appeared in our pages.

Journal of the Franklin Institute, Oct. 1873.—We have here the second portion of Prof. Thurston's valuable paper on the molecular changes produced in iron by variations of temperature. He comes to the conclusion that at temperatures above 600° and below 70° F., iron conforms to the general law for solid bodies, that increase of temperature diminishes tenacity but increases ductility and resilience, while decrease of temperature has the opposite effect. Below 70° the tenacity increases with diminishing temperature at the rate of 0·02 to 0·03 per cent. for each degree F., while the resilience decreases in much higher ratio. Between ordinary temperatures and a point somewhere between 500° and 600°, on the other hand, iron shows marked deviation from the law, the strength increasing to the extent of about fifteen per cent. with good iron. The practical result is, that as iron does not lose its power of sustaining "dead" loads at low temperature, but greatly loses its power of resisting shocks, the factor of safety in structures need not be increased in the former case, where exposure to severe cold is apprehended; but that machinery, rails, and other structures which have to resist shocks should have large factors of safety, and be protected, if possible, from extremes of temperature.—Mr. Lowe communicates "something new concerning the physical properties of steam," viz., that the external work given out by steam in expanding from the temperature (t') to the temperature (t), bears a constant ratio to the difference; that is, to ($t' - t$). He considers the latent heat performs the internal work, while the sensible heat only is available for external work; in which case that vapour whose latent heat is the smallest, other things equal, would be the best agent for converting heat into work.—A paper on statistics of coal, is compiled from Mr. James McFarlane's "Coal Regions of America."—Mr. Bilgram furnishes an "Elementary treatment of Zeuner's slide-valve;" and Mr. Murphy has a paper on "Bridge building considered normally."—There are descriptions of machinery for utilisation of coal waste, a stone-cutting machine, and a machine for making paper boxes. The latter produces match-boxes at the rate of 3,000 in an hour. Paste is dispensed with, the slips of wrapper being fastened by delicate staples of iron wire.

American Journal of Science and Arts, November, 1873. In this number we find two contributions in chemistry from the Massachusetts Institute of Technology, in one of which it is shown that by solution of cast-iron in an acid, there may be obtained, besides gaseous bodies, which escape with the hydrogen, volatile hydrocarbons, boiling between 93° and 155° C., and probably belonging partly to the saturated, partly to the non-saturated series. Of the latter, considerable quantities may be condensed by combination with bromine, after having passed through a freezing mixture.—Prof. H. L. Smith gives a series of investigations made in the Queen's Chamber of the Great Pyramid, as supporting the view that a high degree of geometrical and astronomical knowledge must have been possessed by the builders, but without superhuman accuracy. In a paper on rocks of the Helderberg era, in the Connecticut Valley, Prof. Dana endeavours to show that Staurolitic slate, hornblendic rocks, gneiss, mica schist, &c., are extensively developed in a *formation of Helderberg age*, and probably the Upper Helderberg or Lower Devonian. There is a letter from Dr. B. A. Gould, Director of the Cordoba Observatory (date Aug. 5), giving an account of work recently done there. Zone observations had been begun in September last year, and were nearly half completed, some 50,000 stars having been observed. From a note on the hypsometric work of the U.S. Geological and Geographical Survey of the Territories, we learn that four stations were established: at Denver, 5,000 feet above the sea; Cañon City, 6,000 feet; Fair Play (in the South Park), 10,000 feet; and Mount Lincoln, 14,000 feet; the observations at each being taken three times daily. The U.S. Signal Service have recently established a permanent meteorological station on the summit of Pike's Peak, about 14,000 feet high; the observations will be published daily by telegraph, and will doubtless be of high scientific and popular interest.—Of the remaining matter we may note suggested improvements in filter pumps, and in the arrangement of shutters in a dome for an equatorial telescope.

Poggendorff's Annalen der Physik und Chemie. No. 7. 1873. In this number, M. Quincke continues his "Optische Untersuchungen," investigating at some length the behaviour of polarised light on its passage through gratings.—M. Riess enunciates thus a new kind of reaction of currents: a wire circuit, part of which is traversed by a given (Leyden) battery current, remaining unaltered, various secondary currents, produced in it successively, react on the primary, so that the weaker secondary corresponds to the stronger primary.—Dr. Voller has examined the influence of temperature on electromotive force of galvanic combinations, and finds that with salt solutions in contact with copper or zinc, the force is diminished by rise of temperature, whereas with acids it is increased.—An interesting paper by Prof. Villari treats of the time flint glass takes to be magnetised, demagnetised, and to turn the plane of polarisation. He rotated a glass cylinder between the poles of an electro-magnet, where it acted like a cylindrical lens to polarised light passing through the poles. When not magnetised, the cylinder, whether in motion or at rest, was neutral to the light; but when magnetised, its plane-rotating power considerably diminished with increasing velocity of rotation; the reason being that, in such quick revolution, each diameter remained too short a time in the axial direction to acquire all the magnetism it would otherwise have. To give flint glass such diamagnetic intensity, as became observable by rotation of the plane, required at the least 0″·001244, while to give it all the diamagnetism it is capable of taking under a strong magnet, at least 0″·00241 was necessary.—"A contribution to the theory of thermal currents," by M. Avenarius, appears to be an appropriation of results published by Prof. Tait in 1870, and which are incorporated in the professor's Rede Lecture for this year. A similar remark will apply to M. Topler's application of air-friction to the deadening of galvanometer needles, &c., which is simply Sir W. Thomson's dead-beat principle.—M. Raye criticises unfavourably M. Zöllner's theory of sun-spots and protuberances; his own theory represents, in the sun, something like what occurs in our cyclones, in which there is an *upward* air-current carrying with it aqueous vapour, which forms above into a cloud. He thus differs from Faye, who supposes a *descending* current, in the solar cyclones.—M. Hennig describes an apparatus for quantitative spectrum analysis, and M. Schneider continues his account of salts of sulphur. We find also notes on galvanic reduction of iron under the influence of an electromagnetic solenoid, and on the reflection and refraction of sound; from the St. Petersburg and Vienna academies respectively.—An abstract of an instructive paper by M. Vogel on the spectra of comets we hope to give shortly.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, Nov. 19.—Prof. Ramsay, F.R.S., vice-president, in the chair.—The following communications were read:—"Supplemental Note on the Anatomy of *Hypsilophodon Foxii*," by Mr. J. W. Hulke, F.R.S. The material for this note was a slab from Cowleaze Chine, containing portions of two individuals of *Hypsilophodon Foxii*, one consisting of a skull with a great part of the vertebral column, the other of a portion of the vertebral column. The author described some details of the structure of the skull, and especially the palatal apparatus. In connection with the question of the generic rank of *Hypsilophodon*, the author stated that in *Hypsilophodon* the centra of the sacral vertebræ are cylindroid and rounded below, whilst in *Iguanodon* they are compressed laterally and angulated below.—"The Drift-beds of the North-west of England, Part 1, Shells of the Lancashire and Cheshire Low-level Clay and Sands," by Mr. T. Mellard Reade. The author gave a list of the localities in which shells were found, and stated that in all forty-six species had been met with distributed through the clay-beds, those found in the sand-seams being rare and generally fragmentary and rolled. He contended that the admixture of shells in the boulder-clay was due to the tendency of the sea to throw up its contents on the beach, whence changing currents and floating ice might again remove them, and to the oscillations of the land bringing all the beds at one time or another within reach of marine erosive action. He maintained that it is in the distribution of land and sea at the period of deposition of the Lancashire deposits, and not in astronomical causes, that we must seek the explanation of the climate of that period, the conditions of which he endeavoured to explain by a consideration of the proportions of the species and the natural habitats of the shells found in the drifts.—"Note on a deposit of Middle Pleistocene Gravel near Leyland, Lancashire," by Mr. R. D. Darbishire. The bed of gravel, about forty feet thick, and about 240 feet above the level of the sea, is covered by yellow brick clay, and overlies an untried bed of fine sea-sand. The shells and fragments occur chiefly at the base of the gravel. The author considered the Leyland deposit, like those on the west of the Derbyshire hills, to be more probably littoral and truly climatic than that of the Liverpool clays, the subject of Mr. Reade's paper, and hazarded the conjecture that the latter were sea-bottom beds, into which, during some process of degradation and redistribution, the specimens found and enumerated by Mr. Reade had been carried down from the former more ancient retreating coast-lines.

Geologists' Association, Nov. 7.—Mr. Henry Woodward, F.R.S., president, in the chair.—At this, the first meeting of the session 1873–74, the president delivered the opening address of the new session, in which he gave a review of the progress of geological science during the past year. Mr. Woodward referred to the progress made in the acceptance by botanists and zoologists of the doctrine of evolution. "Darwin's theory has already passed through the fire like crude ore, it has been roasted, crushed, sifted, washed, and after all the pure metal remains. Our speculations, however, bring us no nearer to the discovery of the origin of life itself."

Meteorological Society, Nov. 19.—Dr. R. J. Mann, president, in the chair.—The following papers were read:—The thunderstorm at Brighton on Oct. 8, 1873, and its effects, by F. E. Sawyer, and some considerations suggested by the depressions which passed over the British Islands during September 1873, by F. Gaster.—A discussion took place on the best form of thermometer stand. It was resolved that the following conditions should be fulfilled:—(1) The contained thermometers must at all times be shielded from the direct rays of the sun; (2) The stand must be so arranged that even when its own external temperature is raised, the thermometers shall not be thereby affected; (3) As reflected heat must diminish the accuracy with which thermometers indicate air or shade temperature, these disturbing causes should be excluded; (4) The temperature of the air alone being desired, it is necessary that the readings of the thermometers be not affected by radiation to the sky; (5) It being desirable that one pattern of stand be used in all localities, it follows that it should be absolutely independent of all surrounding objects; (6) There must be free access of air round the thermometers; (7) No rain should ever reach the dry-bulb thermometers, for if it does, it improperly lowers their temperature, making them read even lower than the wet bulb; (8) The stand must also be unaffected by snow, both as a direct fall or from obstructed circulation of air; (9) It is very desirable that the stand require no attention between the hours of observation; (10) It is desirable, but not absolutely necessary, that room be provided for a duplicate set of instruments; (11) The stand should not be costly; (12) It should be capable of easy transmission by rail or otherwise. Mr. Prince gave an account of some experiments he had made, and was of opinion that the true temperature of the air could be obtained without a stand. Mr. Symons thought that a stand constructed on the Kew and Stevenson pattern combined, but smaller than the former and larger than the latter would be the best form of stand to adopt. The meeting not having the results of the comparison of the observations made with the different stands at Strathfield Turgiss, the discussion was adjourned till after these are published.

Anthropological Institute, Nov. 25.—Prof. Busk, F.R.S., president, in the chair.—Mr. F. W. Rudler read a report on Anthropology at the meeting of the British Association at Bradford.—Dr. G. W. Leitner, Principal of the Government College of Lahore, gave an account of the Siah Posh Kafirs, a race of people inhabiting Kafiristan, on the south-eastern slope on the Hindu Kush. Kafiristan may be said to form a triangular tract of country lying between 35° and 36° N. lat., and 70° and 72° E. long., and is bounded on its sides by Kábul, Badakshan, and Kashmir. The name of Siah Posh Kafirs was given to them by the Mohammedans, "Siah" meaning "black," "Posh" clothing, and "Kafir" infidel; for in fact a Kafir, according to the Mohammedans, was any one who did not follow the teaching of Mahomet. The Kafirs claimed to be a sort of country cousins of the British. Slavery existed within their own country, and also within five miles of Peshawur, where the Kafirs were sold in the open market. The consequence was that the Kafirs in retaliation, kept the roads leading to Central Asia in a state of insecurity, and murdered all travellers coming within their reach. Dr. Leitner, referring to the asserted Macedonian origin of the Kafirs, said that that supposition was founded on very loose and vague data, and that they themselves knew nothing of Alexander. The Tunganis, another of those races, claimed direct descent from Alexander's soldiers. Another theory was that the Siah Posh Kafirs were Zoroastrians, who were supposed to have been forced into the hills by the Arabs, and the existing customs among the Kafirs certainly seemed to support the idea that they were ethnologically connected with the Parsees. He inclined to the opinion that they were Aborigines; and if they were not descended from the same stock as the "Arian" race, they were certainly, as far as language was concerned, equally related to the Sanscrit.

Entomological Society, Nov. 17.—Prof. Westwood, president, in the chair.—Mr. Higgins exhibited *Deilephila euphorbiæ* and *Sphinx pinastri*, bred from larvæ taken in June 1872, near Harwich.—Mr. Champion exhibited several rare Coleoptera taken at Braemar and other places during the past season.—Mr. Boyd exhibited a Trichopterous insect, *Brachycentrus subnubilus*, a species which constructs quadrangular cases, which had been reared from the egg state.—Mr. Müller remarked on some galls found by Dr. Masters on the roots of *Deodara*, which he considered identical with the galls of *Biorhiza aptera*, Fab., usually occurring on the roots of oak.—Mr. Bird exhibited *Chilo gigantellus* from Horning Fen, and Mr. Vaughan *Pempelia davisellus* reared from Furze.—Mr. Stevens exhibited some rare Lepidoptera taken on the South Coast.—A paper was read, entitled "Notes on the Habits of *Papilio merope* Auct., with a Description of its Larva and Pupa," by J. P. Mansel Weale, B.A. Also a paper entitled "Observations on *Papilio merope* Auct., with an account of the various known Forms of that Butterfly," by Roland Trimen, F.L.S., &c.—Some remarks were communicated by Mr. Miskin, of Brisbane in Queensland, respecting *Mynes guerini* of Wallace, which he considered identical with *M. geoffroyi* Guerin, and directing attention to the singular habit of the pupæ, which were suspended in groups of three or four individuals, united at the tails.

Royal Horticultural Society, Nov. 12.—Scientific Committee.—A. Grote, F.R.S., in the chair.—The Rev. M. J. Berkeley sent a Capsicum from Transylvania with two small fruits produced from the placenta.—Mr. Anderson Henry sent fruit of *Tacsonia quitensis*, produced in a cool greenhouse.—Mr. Wheble sent wood and bark of *Sequoia sempervirens*, the latter being extremely similar to that of the large tree exhibited at the Crystal Palace.—Prof. Thiselton Dyer exhibited preparations of the buds upon the leaves of *Malaxis*, prepared by Prof. Dickie.

General Meeting.—H. Little in the chair.—Prof. Thiselton Dyer called the attention of the meeting to the fine plant of *Vanda cærulea* with four panicles; a plant of the recently introduced *Batemania Burtii* from Costa Rica; specimens of a species of *Stylidium* (probably *S. ciliatum*), an Australian genus with the radical leaves in a Crassula-like tuft; flowering specimens of *Cunonia capensis* from Syon House; and a "grape-rail," a contrivance by which grapes could be preserved through the winter. The pieces of cane to which the grapes were attached were inserted into holes in long zinc rod-like boxes which contained a mixture of fuller's earth, starch, sugar, charcoal, and water. It was remarked by Mr. Jennings that *Vanda cærulea* was fast disappearing from its native localities. At the present rate the ruthless removal of the plant must determine its extermination at any rate in the Khasia hills.

Anthropological Society, Nov. 18.—Dr. R. S. Charnock, president, in the chair. Extracts from letters from foreign correspondents were read, one of which announced an alleged discovery of a Phoenician inscription of the 4th century, B.C., near Rio de Janeiro, and one from Captain Burton, mentioning the discovery at Maeshowe, in Orkney, of Scandinavian inscriptions, in Arabic letters.—Personal observations of the Sae-lies or Flathead Indians of North America, by J. Simms, M.D., of New York. The discourse treated of the manner of fashioning or deforming the head, the customs, dress, diet, disposition of the dead, &c. Dr. Simms also gave a brief description of the Quatsino Indians, who inhabit the north-western coast of Vancouver Island, the mode of fashioning their peculiar, sugar-loaf form of heads, their superstitions, food, &c. He also gave a very interesting account of the Digger Indians of California, the ircolour, form, dress, manner of living, general habits, including badges of mourning, food, &c. The Snakes, Utes, Piutes, Foxes, Sioux, and other tribes were briefly described.

CAMBRIDGE

Philosophical Society, Nov. 17.—"On a suspected forgery in the Vatican Manuscript Record of the Trial of Galileo before the Inquisition," by Mr. Sedley Taylor, late Fellow of Trinity College. The object of the paper was to show, in accordance with the views of recent German and Italian authorities, that the sentence pronounced against Galileo in 1633 was based on a spurious document fabricated for the express purpose of securing his condemnation. The evidence adduced to support this conclusion was taken partly from the works and letters of Galileo, and partly from the contemporary records of the trial preserved in the Archives of the Inquisition, portions of which have been lately published for the first time. The result of the paper was to exonerate Galileo completely from the charge of contumacy which all his biographers have hitherto either advanced or tacitly admitted.

MANCHESTER

Literary and Philosophical Society, Nov. 4.—R. Angus Smith, F.R.S., vice-president, in the chair.—"On the Bursting of Trees and Objects struck by Lightning," by Prof. Osborne Reynolds, M.A. The results of the experiments referred to in this paper were exhibited to the meeting. The suggestion thrown out by Mr. Baxendell at the last meeting—that the explosive effect of lightning is due to the conversion of moisture into steam—seemed to him to be so very probable, that he was induced to try if he could not produce a similar effect experimentally. He tried various experiments by sending a discharge through pieces of damped wood, and through glass tubes with and without water. The pieces of wood, which varied in size, yielded various results, and the glass tubes, which also were of various sizes, were shivered to pieces.—The Rev. W. N. Molesworth, M.A., brought under the notice of the Society some Roman and Celtic antiquities, to which he thought that sufficient attention had not been given in this country.

Nov. 18.—E. W. Binney, F.R.S., vice-president, in the chair.—"On the Bursting of Trees and Objects struck by Lightning," by Prof. Osborne Reynolds, M.A. In a paper on this subject read at the last meeting I stated that the tube which was burst by a discharge from a jar would probably withstand an internal pressure of from 2 to 5 tons on the square inch; and I made use of the expression the tube might be fired like a gun without bursting. These statements were based on the calculated strength of the tube, and with a view to show that there was no mistake, I have since tried it in the following manner.—I made 3 guns of the same tube. No. 1, which was 6 inches long, had its end stopped with a brass plug containing the fuze hole. No. 2 and No. 3 were 5 inches long and had their breeches drawn down so as only to leave a fuse hole. These tubes were loaded with gunpowder and shotted with slugs of wire which fitted them, and which were all ¾ inch long. No. 1 was first fired with ¼ inch of powder, the shot penetrated ¼ inch into a deal board, and the gun was uninjured. No. 2 was then fired with 1¼ inches of powder, and the shot went through the 1-inch deal board and ¼ inch into some mahogany behind, thus penetrating altogether 1¼ inches; the tube, however, was burst to fragments. Some of these were recovered, and although they were small they did not show cracks and signs of crushing like those from the electrical fracture. No. 3 was then fired with ¾ inch of powder, and the shot penetrated ¼ inch into the deal board. It was again fired with 1 inch of powder, and the shot penetrated 1 inch into the deal. Again it was a third time fired with 1¼ inches of powder, when it burst, and the shot only just dented the wood. These experiments seem to me to prove conclusively the great strength of the tube and the enormous bursting force of the electrical discharge.—On the colour of Nankin cotton by Edward Schunck, Ph.D., F.R.S.—An improved method for preparing Marsh Gas, by C. Schorlemmer, F.R.S. The author found that by heating an intimate mixture of anhydrous sodium acetate with more than twice its weight of lime and sodium carbonate, a very regular and quiet evolution of marsh gas took place. The gas thus obtained always contains some acetone, which is easily removed by shaking it with water, or, better still, with a solution of acid sodium sulphite.

DUBLIN

Royal Geological Society of Ireland, Nov. 12.—Prof. E. Hull, F.R.S., president, in the chair.—Mr. J. E. Gore, C.E., read a note on a bed of fossiliferous kunkar in the Punjab—The president read a series of notes on the Microscopic Structure of Irish Granites:—1, Granite of Aillemore, Co. Mayo; 2, Granitoid Quartz Porphyry of Attithomasreagh, Co. Galway; 3, Granite of Ballynockan, Co. Wicklow.—Prof. Reynolds exhibited specimens of the new minerals Uranotine and Walpurgine.—Prof. Traquair exhibited specimens for the Rev. J. Emerson, of some coal fossils from the Jarrow Colliery, Co. Kilkenny, among which were noticed portions of the skeletons of *Urocordylus wandesfordii* and *Ichthyerpeton bradleyi* described some time since as from a neighbouring colliery, by Huxley and Wright; also the palate tooth of *Ctenodon cristatus*, patches of scales of *Megalichthys hibberti*, and some vertebræ and scales of a Rhizodopsis.

Royal Irish Academy, Nov. 10.—Rev. Prof. Jellett, president, in the chair.—A paper was read by Messrs. Draper and Moss on some forms of Selenium, and on the influence of light on the electrical conductivity of this element.—Prof. Macalister read a paper on the anatomy of a species of Aonyx from the Upper Indus. The species had been sent by the late Earl of Mayo to the Royal Zoological Society of Ireland, but differed in no marked degree from the one described by Horsfield as *A. leptonyx*.—Mr. H. W. Macintosh read a paper on the myology of *Arctopithecus blainvillei*.

EDINBURGH

Royal Society of Edinburgh, Dec. 1.—Sir Robert Christison, vice-president, in the chair.

The following communications were read:—

1. Laboratory Notes, by Prof. Tait.—(1) First Approximation to a Thermo-electric Diagram. (2) On the Flow of Water through Fine Tubes.

2. Note on the use of ∇ in Curvilinear Co-ordinates, and on the Transformation of Double and Triple Integrals, by Prof. Tait.

2. On the Physiological Action of Ozone, by James Dewar and Dr. M'Kendrick.

4. On a Compound formed by the addition of Bromacetic Acid to Sulphide of Methyl, and on some of its Derivatives, by Prof. Crum Brown.

5. Note on the Expression for the Action of one Current-element on another, by Prof. Tait.

GLASGOW

Geological Society, Nov. 13.—Mr. E. A. Wunsch, vice-president, in the chair. A paper on the Post-tertiary Beds (Kyles of Bute), by the Rev. H. W. Crosskey and David Robertson, was read to the meeting. The succession of beds, as found at various parts of the Kyles, in proceeding from high to low water mark, is as follows:—(1) Boulder-clay, hard, compact, unfossiliferous, and red in colour; (2) A highly laminated clay, precisely similar to that which occupies the same position at Paisley and many other localities, has been found to contain the remains of some species of Foraminifera; (3) A bed of clay and sand, exceedingly rich in characteristic Arctic shells; (4) The *Pecten maximus* bed, has been found cropping out in various localities.—Mr. Jas. Armstrong read a paper on the Fossils found in the Carboniferous Shales of Gare and Westerhouse, illustrated by a series of finely-preserved specimens collected from these localities, about three miles to the north-east of Carluke.—The Chairman exhibited some interesting specimens of the junction of granite and slate from the island of Arran, and made some remarks on the various theories which had been propounded regarding its origin.

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THURSDAY, DECEMBER 11, 1873

THE ARCTIC EXPEDITION

WE were able to announce, in our last number, that the version of the reply of the Government with regard to the despatch of an Arctic Expedition, which had appeared in the daily newspapers, was inaccurate, and that the subject was still under consideration. But the grounds for abandoning Arctic discovery, which were attributed to the Government, have no doubt occurred to the official mind; and they involve fallacies which would be so fatal to the best interests of this country, that we cannot allow them to pass without remark.

It was said that the Government hold that survey operations have a stronger claim than those of discovery; and that if Ministers were inclined to augment charges for such purposes, they would incline to do so for survey, rather than for a new voyage of discovery.

We have here an attempt to separate scientific expeditions into two branches, survey and discovery. The originator of this fallacy does not appear to be aware that all surveying voyages are voyages of discovery in the strictest sense. Their operations are intended to explore, and accurately lay down, unknown or little known coasts or harbours. Captain Cook's voyages were surveying operations, and it will scarcely be denied that they were also voyages of discovery. The Arctic voyages of Ross and Parry included surveys which have been of the utmost value to the whaling fleets ever since.

Arctic discovery is now advocated by naval and scientific men for the very reason that it will include marine surveys and hydrographical investigations of the most undoubted importance. Few operations have "benefited commerce and promoted international intercourse" more than Arctic voyages of discovery. One of our earliest Arctic expeditions discovered the White Sea route to Russia, and opened a flourishing trade. The Spitzbergen voyages led to the establishment of a fishery which added millions to the wealth of these islands. The discovery of Davis's Straits did the same. Ross's first voyage showed the way for the whalers into Baffin's Bay. Parry's voyages pointed out new ground in Prince Regent's Inlet. The handling of steamers by Osborn and Cator, as Capt. Penny declared at the time, caused a revolution in the system of ice navigation by whalers. Arctic voyages are surveying operations, and they have benefited commerce as much as any other surveying work whatever. As to promoting international intercourse, Arctic achievements have always excited friendly sympathy and interest throughout the civilised world.

We must also notice the shocking insincerity of the reply that is imputed to the Government. An Arctic Expedition cannot be undertaken, we are told, because Ministers are anxious to provide funds for ordinary surveys. Now it is a fact that no Government has ever more persistently neglected the surveying branch of the service; which has been so starved and pared down as to cause anxiety to those acquainted with the subject. If the Hydrographer's official position did not seal his lips, he could give an account of the way in which the surveying department has been treated of late years, which would excite indignation throughout the country. Some idea may, however, be obtained of the way in which surveys are neglected, from the following figures. From the year 1849 to 1853, the proportion of each 1000*l.* of naval expenditure spent on surveying averaged 15*l.* 5*s.* It is now 9*l.* In 1871—72 the total effective naval expenditure was 7,807,946*l.*, and the expenditure on the surveying branch was 70,456*l.* The total tonnage of the British mercantile marine in 1871—72, was 7,142,894, so that the total naval expenditure per ton of British naval shipping, was 1*l.* 1*s.* 11*d.*; and the proportion of expenditure for surveying and discovery, by far the most useful and important work of the navy in time of peace, was 2*d.* Not only has surveying and Arctic work been rendered inefficient by extreme parsimony, or wholly neglected; but, while the wealth of the country has enormously increased, the expenditure on the best work of the navy has been cut down to a third less than it was twenty years ago.

It may be that the official notion of surveying is confined to the revision of work on comparatively well-known coasts. Even such work is done inefficiently; and its renewed efficiency would be no argument for the neglect of Arctic exploration. At the time when Arctic expeditions of discovery were despatched, the more ordinary surveying operations were not neglected. Officers were surveying the coasts of these islands, Capt. Graves was at work in the Mediterranean, Collinson in China, Kellett in the Pacific, and their vessels were properly equipped. Assuredly the Government are bound to restore the Surveying Department to efficiency; and such a reformation would include the despatch of a thoroughly well-equipped Arctic Expedition for survey and discovery. We understand that a further most able and carefully-considered letter has been addressed to Mr. Gladstone on this subject; and we earnestly trust that, after further consideration, the Prime Minister will see that his plain duty points in the same direction as political expediency. The country feels strongly on the subject; and the resolution to despatch an Arctic Expedition of discovery in 1874, will meet with the hearty approval of all classes of the community.

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*LOCAL SCIENTIFIC SOCIETIES**

III.

IN the ten years succeeding 1860 the number of local scientific societies formed throughout the country was more than double that of the previous decade, amounting altogether to fifty-six, of which no less than forty-five are field-clubs. Many of these are well known for producing excellent work, but we must refer our readers to the list at p. 521 of vol. viii. for details. The Quekett Club of London was formed during this period, as were also a number of clubs in the Severn Valley, the Eastbourne Natural History Society, and others which have done good work, but which are far too numerous to mention. Two or three very excellent societies were formed in North Britain during this decade, including the Perthshire Society of Natural History, which, at any rate as represented by a few of its members, is one of the hardestworking societies in the kingdom. Under its auspices the *Scottish Naturalist* is published, and a *Flora* and *Fauna* of the extensive and varied county of Perthshire is being brought out; recently we noticed a proposal issuing from one of the members for the establishment of a British Naturalists' Agency. A very laudable though somewhat Scotch appendage has just been added to the Society, in the shape of a "Perthshire Mountain Club" for the exploration of the Perthshire mountains, more especially those that have been neglected by naturalists, with the following office-bearers:—A cairn-master, a scribe and naturalist, a geometer, a bard, and, to crown all, a quaigh-bearer, a quaigh being a two-eared drinking-cup from which to quaff the "mountain-dew" withal.

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* Continued from p. 40.

Another Scottish club that we deem worthy of special mention is the Alva Society of Natural Science and Archæology, whose history has been one of continued success. There can be no doubt, the secretary informs us, that this Society has tended to foster a taste for natural history in the neighbourhood, and encouraged the observation of local phenomena. It was founded in 1862, and now numbers 110 members belonging to all classes of society; the patron being the Earl of Kellie, the president the sheriff-substitute of the county, the vice-presidents a medical practitioner, a grocer, and a wine-merchant; the councillors a clergyman, a bank agent, a hairdresser, an architect, and an ironmonger; the treasurer a druggist, the secretary a medical practitioner, the curator a blacksmith, and the librarian the governor of the prison. The object of the Society is the study of natural science and archæology by the exhibition and preservation of specimens, the reading of communications, by lectures, excursions, and the formation of a library and museum. The number of members has become so large, and the collections of the Society have so accumulated, that their present place of meeting has become too small, and the Society has therefore contracted to have a special building erected for its own use, at a cost of about 1,600*l*., raised by subscription from among the members and the noblemen and gentlemen of the neighbourhood. The papers read at the monthly meeting are printed in one of the local papers, the type being afterwards broken into pages, and a small volume of transactions thus published for each year. One of these volumes we have before us, and its contents are varied and exceedingly creditable, though we miss a list of the fauna and flora of the small county of Clackmannan, in the county town of which the Society has its head-quarters. We hope this excellent Society will make the compilation of such lists part of its work in the future.

Our space only permits us to name the Largo Field Naturalists' Society, on the north shore of the Frith of Forth, a society founded in 1863, and which, to judge from the papers read and the secretary's report to us, is doing excellent service in connection with the natural history of the county of Fife; it appears to have a valuable collection of specimens. We mention these three societies because, in some respects, they are worthy of imitation by other similar associations, and because, we regret to say, Scotland is not represented in the list of field-clubs in anything like the proportion, even considering its size, that England is; very large districts, which we are sure would yield abundant fruit of a rare and interesting kind, being entirely unworked by any club. We hope in the course of a very few years to see this defect remedied.

In the three years 1871-2-3, at least twenty-seven new societies have been formed; there may have been more of which we have not heard. Fourteen of these have had their origin during the present year; and if field-clubs continue to multiply during the remaining years of the decade in the same proportion, we may expect to see very few districts in England and Scotland at least, without its local field-club. We had hoped that the inquiries of the British Association Committee on this subject might have given an additional impetus to the spread, as well as to the usefulness, of such societies; but we fear that hitherto this committee has done absolutely nothing.

We cannot conclude this part of the subject without referring to the field-clubs of Lancashire and the west of Yorkshire. In Lancashire there are a number of field-clubs * composed almost exclusively of working-men, some of which have been in existence for many years, and all of them, we believe, in excellent working condition. In Lancashire there are at least eleven of such clubs, one of which is among the most efficient field-clubs in the kingdom. This is the Todmorden Botanical Society, which may be taken as a specimen of these Lancashire clubs, and of which Sir Walter Elliot thus speaks:—

"One of the most successful of the above is the Todmorden Botanical Society, established in 1852, principally through the exertions of Mr. Stansfield, who has always been its president. The bulk of the 185 members are working-men, who pay a subscription of 6*s*. a year, meet on the first Monday of every month, and in the winter, on the intermediate fortnights, for lectures and papers; and make six field excursions, four within ten miles, and two longer ones, extending into neighbouring counties, and even as far as Scotland. They have a good herbarium, and have prepared a flora embracing a space of six miles round Todmorden. They have also acquired a library of 600 volumes, chiefly botanical."

We can only briefly refer to the West Riding Consolidated Naturalists' Society, which at present, as will be seen from our list, consists of an amalgamation of twelve local clubs, belonging to various towns in the West Riding, and all of them, like the Lancashire Societies, composed mainly of working-men. Each of these societies has, we understand, its own district in which to carry on its field-work, and the united societies have stated meetings, but so far as we have ascertained, they have not yet decided upon a satisfactory *modus operandi*. The amalgamated societies have, however, a journal in common, "The Yorkshire Naturalists' Recorder," in which their proceedings are published, we believe monthly. There is no doubt that if their united societies could devise a satisfactory organisation in which to carry on their work in co-operation, great good would be the result. Their example might, we think, be followed with advantage by other contiguous small societies, which we fear are often apt to get disheartened from the paucity of working members, and a feeling of isolation. This is the only instance, so far as we know, in which a number of contiguous societies have united into a connected group, though other societies occasionally have excursions in common.

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* We regret that these were omitted from our list, as we got no information from them, and Sir W. Elliot does not give them in his list, only referring to them for some reason in his address.

We regret to say that since our list was published, we have ascertained that two of the Yorkshire Societies named therein, are now defunct, viz. the Halifax Naturalists' Society, once a member of the West Riding Union, and the Leeds Natural History Society. We have been told that the Wigan Field Naturalists' Scientific Society, given in Sir Walter Elliot's list, with 150 members, is also dead. We hope that in reality these are not dead, but only sleeping; and that means may soon be taken to rouse them again into activity.

Altogether, then, including the Lancashire Societies not in our list, and others of which we have heard since our list was published, one of which was founded at Ballymena, County Antrim, the result, we believe, of some lectures there last winter, there are at the present time in Great Britain and Ireland at least 169 associations established solely or partly for the pursuit of science in one form or another. Of these 104 are professedly field-clubs, while a considerable number of the remainder do field-club work in so far as the publication of lists of the natural productions of their surrounding districts are concerned. Only 22 of these 169 societies were founded previous to 1830, while all the field-clubs were formed after that year, and by far the greater number of them within the last twenty-three years. We do not reckon among these the scientific societies which have been formed in connection with our public schools, to which we shall refer afterwards.

Of these societies the English ones are mainly grouped in the North of England, along the Welsh border, and in the southern counties, the midland district being but sparsely represented, and Bedfordshire,[*] Derbyshire, Essex, Hertfordshire, Huntingdonshire, Lincolnshire, Rutlandshire, not at all, not to mention the Channel Islands and the Isle of Man, which would afford opportunities to field-clubs which cannot be attained in the main island at all. Glamorganshire is the only Welsh county represented by a society, while all but three of the Irish counties are unrepresented. Scotland, the birthplace of field-clubs, we have already referred to as being far behind England in this respect. Ireland, and even Wales, cannot perhaps at present be blamed for their backwardness in regard to associations of this kind, though each country, in its own way, offers a magnificent field of investigation to local naturalists. With regard to the unoccupied districts of England and Scotland, we can only hope that the scientific contagion may rapidly spread, as no doubt it will when all the conditions are present for its taking effect. Meanwhile, the rapid spread of scientific societies, and especially field-clubs, and the valuable results that have already followed from the labours of a number of them, must be exceedingly gratifying to all who desire to see the triumph of science, and, indeed, to all who are earnestly seeking after the elevation of their fellow-men. Is it not one more sign that "the old order changeth, yielding place to new?"

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[*] By a misprint in our last article the Woolhope was said to be in Bedfordshire instead of Herefordshire.

MARSHALL'S TODAS OF SOUTH INDIA

A Phrenologist amongst the Todas; or, the Study of a Primitive Tribe in South India. By William E. Marshall, Lieut.-Col. of H.M. Bengal Staff Corps. (Longmans, 1873.)

THE Todas are a pastoral hill-tribe in the Nilagiri region of Southern India, whose singularly interesting social condition fairly entitled them to be described in a volume by themselves. Colonel Marshall succeeds in communicating to his readers the lively interest he felt in his work, and several points of ethnology will be perceptibly advanced by it, notwithstanding much of the theoretical part of the book which will hardly meet with acceptance.

Especially from the moralist's point of view, the condition of these secluded herdsmen deserved to be put on record while still little changed under influences from without. They show perfectly how the milder virtues naturally prevail among men in an intellectually childlike state, if only society is undisturbed from without, and finds its equilibrium within. "The general type of the Toda character is most unvarying; singularly frank, affable, and self-possessed, cheerful yet staid;" theft and violence are almost absent among them; their quiet domestic life is "undisturbed by the wrongs of grasping, vindictive, overbearing natures;" their engagements to support their wives and children, though resting on mere promises, are kept through utter guilelessness and want of talent to plot. Toda society is simply held together by the strength of family affection. "It is a quiet, undemonstrative, but intensely domestic people; domestic in the wider sense of viewing the entire family, to the last cousin, much as one household, in which everyone is everywhere entirely at home; each one assisting, with the steadiness of a caterpillar, in the easy, progressive task of emptying his neighbour's larder; no one exerting himself by one fraction to raise the family. The great feature in Toda organisation, is the all-absorbing power of his domestic attachments, which, like Pharaoh's lean kine, swallow up all other qualities." The points where the moral code of these easy-going folk differs from that of modern intuitive moralists, are especially polyandry and infanticide. Their marriage-relations within the family have perhaps more nearly approached than those of any other known tribe that promiscuity which several modern ethnologists have supposed to belong to a primitive state of society; "it was formerly their almost universal custom—in the days when women were more scarce than they are now—for *a family of near relations to live together in one mand, having wife, children, and cattle all in common.*" Here, indeed, is socialism of an extreme order, prevailing among a low race, in whose general condition its evil and good are alike visible. As need hardly be said, to the Toda mind polyandry seems part of the natural order of things. So it was with infanticide, till about fifty years ago an English officer, Mr. Sullivan, mounted the Nilagiri plateau and visited the homes of the Todas. Since then all the events of Toda history have been dated from the visit of "Sullivan Dore," as we date from the Christian era, and thenceforward the Government put down infanticide, and its former prevalence is now only to be traced in the census, and learnt from the memory of old people.

An aged Toda gave his account of the practice :—"I don't know whether it was wrong or not to kill them, but we were very poor, and could not support our children. Now every one has a mantle ('putkuli'), but formerly there was only one for the whole family, and he who had to go out took the mantle, the rest remaining naked at home, naked all but the loin-cloth ('kuvn'). We did not kill them to please any god, but because it was our custom. The mother never nursed the child—no, never! and the parents did not kill it. How could we do so? Do you think we could kill it ourselves? . . . Boys were never killed, only girls; not those who were sickly and deformed—that would be a sin ('papum'); but when we had one girl, or in some families two girls, those that followed were killed."

Perhaps the ablest part of Colonel Marshall's work is his tracing out of the social forces which brought about this condition of society, the enforced equilibrium between population and means of subsistence, leading a tender-hearted people to systematic female infanticide, and then causing a huddling together of the endogamous polyandrous clans to keep themselves alive. It is no doubt true that the entrance of new conditions, such as a state of war or an advance in the arts, would have altered not only the relation of the sexes but also the moral laws of the people. Colonel Marshall's researches were especially suggested and guided by Mr. M'Lennan's "Primitive Marriage," and if a new edition is brought out of that important treatise (now out of print and scarce), the Todas will supply some items of valuable evidence to it, bearing on ancient social conditions of mankind.

Care must be taken, however, to interpret with proper reservation the word "primitive," as used in these inquiries. Colonel Marshall calls the Todas a "primitive tribe," and argues from their customs to the condition of "primitive races," nor is this objectionable if the word be meant only to signify a comparatively early stage of society. But the Todas are by no means primitive as representing the earliest known grades of civilisation: they are not savages, but a pastoral tribe in a condition much above savagery, belonging to the great Dravidian race of South India. Among them, moreover, may be noticed certain curious customs, to be accounted for on the principle of "survival in culture," and being apparently relics of a former condition of the race different from the present. The Todas are not now hunters, nor do they use bows and arrows. But, at a certain time after marriage, the Toda husband and wife go into the village wood, and kneeling before a lamp at the foot of a tree, the wife receives from the husband a bow and arrow made by him, which she salutes by lowering her forehead to them. Taking up the weapons, she asks, "What is the name of your bow?" each clan apparently having a different name for its bow; he tells her the name, and afterwards she deposits the bow and arrow at the foot of the tree. Colonel Marshall can hardly be wrong in his supposition that this custom has come down from a former period when the Todas actually carried such weapons. This is also confirmed by their funeral rites, where among the articles burnt for the dead man are a flute (an instrument they never use), and a toy bow and arrows, which they get made for the purpose by their neighbours the Kotas. When the author got a man to buy him one, the Kota who made it asked "Who is dead?" The inference is obvious, that the Todas were hunters before they took to their absolutely pastoral life. Nowadays, their cattle are all in all to them; not only their life but their religion turns on buffalo; the milkman is a divine personage too holy to be touched; the most sacred objects are certain ancient cow-bells, and the dignity of the sacred bell-cows is handed down from mother-cow to daughter-cow. The keeping up of this sacred heritage in the female line leads Col. Marshall to infer, at any rate ingeniously, that he has found here a relic of ancient days when the rule of kinship on the mother's side (which he considers with Mr. M'Lennan to characterise primitive society) still prevailed; it only now holds good of bulls and cows, while among men and women relationship is on the male side, thus following the rule which is considered to belong to a higher stage of society. It is not a new idea that the worship of the cow in Egypt and India had its origin not in myth but in practical expediency, being craftily devised to prevent the lives of such valuable creatures being wasted. But nowhere does this argument look so complete and rational as among those thoroughgoing devotees of the milk-can, the Todas.

It is to be feared that the title of Col. Marshall's volume may prevent its having all the popularity it deserves. Not that this title is misleading, for he accepts and uses confidently the now discredited phrenological system of bumps and organs, and tabulates his series of Toda skulls according to their Concentrativeness, Amativeness, Veneration, &c. On this classification by phrenological organs he founds a theory as to the relation between civilisation and the shape of the skull. It appears, from his description, that the Todas are a uniformly long-skulled race, though, among his dimensions, I fail to find anywhere the actual measurements of cranial length and breadth, and can only guess from the portraits (which, by the way, are beautiful autotypes), that the proportions of these two diameters may perhaps be something like 100 : 72 or 75. Now these dolichocephalic Todas being a kindly, harmless, indolent, unprogressive race, Col. Marshall proceeds to connect their narrowness of skull with their want of active energetic qualities, the phrenological organs of which are placed at the side of the head. Thus he comes to the conclusion that it is the brachycephalic tribes, with their skulls broadened by the fierce conquering and progressive organs, which come to the front in the march of civilisation. Well, no doubt there are various dolichocephalic tribes who have remained at low stages of culture, but how is it in the northern half of Asia, the abode of the broadest-headed tribes of man, whom nevertheless the comparatively long-headed Russians have for ages been beating with one hand and civilising with the other. Prof. Carl Vogt's treatment of the question is on a far broader basis, where in a few lines of one of his lectures he shows that both the extreme dolichocephalic and brachycephalic tribes are savages or barbarians, while the main work of civilisation has been done by people who are neither the one nor the other, the mesaticephalic or intermediate-headed races, such as ourselves. This is one of the points which make the reader regret that Col. Marshall did not keep his book waiting till he could bring his opinions under discussion at the Anthropological Institute or the Asiatic Society, which might have led him to modify his views in several ways. As it is, his preface is dated from Faizabad, and in it he describes himself as "a solitary Indian, far away from contact with men of science, but fresh from the actual and impressive presence of 'Nature's children.'" These words account for the freshness and vigour of his style, but they must not be taken to imply that his examination was made without want of knowledge of anthropology. So far from this, one of the great excellencies of the volume lies in showing how much more deeply an observer sees into the life of an uncivilised people, when he is engaged in examining evidence for and against current ethnological theories, than when he goes as a mere traveller, setting down at random anything that takes his attention.

EDWARD B. TYLOR

OUR BOOK SHELF

An Elementary Treatise on Geometrical Conic Sections. By G. Richardson, M.A. (Rivington, 1873.)

THIS is one of the volumes of the publisher's Mathematical Series, is very well printed, and has, if we are not mistaken, only three trivial misprints. There is quite a run at the present time on this subject, if we may judge by the number of treatises which have recently made their appearance, and this we are not altogether surprised at, as it is one of great interest; its theorems have great intrinsic beauty and almost boundless applications. The ordinary propositions are discussed not altogether in the usual order of consecution from the locus-point of view (the last chapter of four pages being devoted to the cone); the demonstrations are neat, and two or three are exceedingly concise as well. The only or chief novelty is the simultaneous treatment of the ellipse and the hyperbola, the corresponding propositions facing one another on the even and odd pages respectively. The discussion of the asymptotic properties of the latter curve pairs off against a series of propositions on projections. The book is a good working one for beginners, and embraces sufficient for the preliminary examination for mathematical honours at Cambridge, without having too much for school use. There is an extensive selection of exercises.

R. T.

Waste Products and Undeveloped Substances. A Synopsis of progress made in their economic utilisation during the last quarter of a century, at home and abroad. By P. L. Simmonds. (London: Hardwicke, 1873.)

MR. SIMMONDS'S book is seasonable in these days, when so much has been done in the utilisation of waste, as showing how very much yet remains to do.

In nearly 500 pages of close print he has drawn attention to a mass of matter almost bewildering in its vastness, and extending to nearly every kind of material in use in civilised communities. We cannot help noticing that Mr. Simmonds has been affected by the mass of subjects he has attempted, for the book very frequently displays a considerable lack of arrangement.

The author should look to this in a future edition, in which also the book might be easily and advantageously condensed to a considerable extent.

We must, however, thank the author for the service he does in calling the attention of civilisation to the extravagant, and we might say, "riotous" living with which its substance is wasted.

La Botanique de la Bible. Étude scientifique, historique, litteraire et exégétique des plantes mentionnées dans la Sainte-Ecriture. Par Frédéric Hamilton. 8vo. pp. 220, 25 photographs. (Nice: Eugène Fleurdelys, 1871.)

THIS interesting volume will possibly be unknown to the majority of our readers, and yet we venture to think that, from the beauty of its illustrations and the pleasantness of its style, it may to some of them prove a welcome addition to their knowledge of the subject on which it treats. Not stopping to discuss the nature of those mysterious trees said to have existed in the Garden of Eden, the author divides his subject into two parts. The first treating of the genera and species of which there can be little doubt, such as the pomegranate, almond, cedar, fig, &c.; and the second of those plants or portions of plants about which it is difficult to decide to what genus even they may belong, such as shittim-wood, hyssop, &c. In the first portion of the volume not only are the scientific characters of the plants given, but there is also added a series of references to them from the classics. The photographs are taken from living specimens growing chiefly in the neighbourhood of Nice and Mentone.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Effects of Temperature on Reflex Action

I DO not know if I quite understand Mr. Lewes's objections to my little article in the *Journal of Anatomy and Physiology*. He attributes the absence of movements in the case in question to a loss of sensibility *to temperature*. At first his statement reads as if the loss of sensibility to temperature were due to the removal of the brain. But he cannot mean this, because the whole of my paper starts from the fact that when the toes alone are exposed to gradually heated water, the leg is withdrawn. If he means that the sensibility to temperature alone is destroyed or depressed by the exposure of the whole body to the gradually heated water, and the other "sensibilities" left intact, I do not see how my argument touching the difference between the entire and the brainless frog is affected at all by a limitation of the stimulus to one particular kind. Moreover, in the last observation recorded in my paper it is expressly stated that in the later stages of heating the absence or diminution of reaction towards chemical as well as thermal stimuli was observed. Gradually heated water acts as a very slight stimulus, sulphuric acid (even dilute) is a very strong stimulus; and that the latter suddenly applied, as in the experiment of Goltz referred to by Mr. Lewes, should call forth a reflex action at a time when the former is unable to do so, in no way contradicts my explanation of the absence of movements. A red-hot iron might have been substituted for the sulphuric acid with identical results.

The paper in question had for its object simply the solution of the difficulty why the brainless frog allowed himself to be boiled without moving. In it I carefully avoided entering upon any discussion concerning Sensation (or Consciousness) in the spinal cord. The words "movement of volition, *that is*, a *movement carried out by the encephalon*,"—"ordinary reflex action, that is, a movement *carried out by the spinal cord alone*," were purposely chosen. I went so far as to speak of an "intelligent frog" and an "unintelligent reflex action," because we have means of measuring intelligence, and we can speak of a body as being conscious and yet not intelligent. I imagine that if Mr. Lewes and myself were to talk over the matter quietly, he would find that I am not so much at variance with him as he imagines. I feel with him the difficulty of refusing to the protoplasm of a white blood corpuscle, a something which may be evolved into (not out of) consciousness. That and like difficulties are not a little increased if, as Mr. Darwin seems to suggest, we regard inherited voluntary acts as the chief instead of the occasional source of reflex actions. Without entering into any long discussion, perhaps I may be permitted to say that in such matters as the movements of a brainless frog, it seems to me there are two things which ought to be kept separate: the investigation into the laws according to which those movements take place, *i.e.*, the study of the various nervous mechanisms of the spinal cord, and the question whether those movements, whether the working of those mechanisms, is or is not accompanied by consciousness. As a physiologist I am prepared to busy myself with the first, as I see prospects of success. With regard to the second, I am not prepared to say anything until we have obtained some better tokens of consciousness than the greater or less resemblance of the movements in question to such movements as our conscious selves are in the habit of executing.

M. FOSTER

Meyer's Exploration of New Guinea

FEW persons can have read Dr. Meyer's account of his recent adventurous and very successful journey with more interest than myself; but I confess I was surprised to find that the translator of my book should have misunderstood what I had stated, and so create a difference between us where none exists. He says (speaking of Dorey) that I "have not given a correct impression of the natives of the surrounding hills and mountains, separating them in some way from the inhabitants of the coast, as smaller, uglier, not mop-headed," &c.; and that he finds on the other hand, that "there is no generic difference at all between the Papoos of the mountain and the Papoos of the coast, except such differences as we find everywhere between the highlanders and coast inhabitants of the same race." Now I say exactly the same thing: "From these (sketches) and the captain's description, it appeared that the people of Arfak were similar to those of Dorey." ("Malay Archipelago," 3rd Ed. p. 505.) Dr. Meyer however, probably refers to what I say of the people of one hill village, close to Dorey: "The inhabitants seemed rather uglier than those at Dorey village. They are, no doubt, the true indigenes of this part of New Guinea, living in the interior, and subsisting by cultivation and hunting. The Dorey-men, on the other hand, are shore dwellers, fishers, and traders in a small way, and have thus the character of a colony who have migrated from another district. These hillmen, or Arfaks, differed much in physical features. They were generally black, but some were brown like Malays. Their hair, though always more or less frizzly, was sometimes short and matted," &c. (p. 499). I can only suppose that the word "differed" in the above passage was taken to mean "differed from the Dorey people," whereas the context shows that it means "differed among themselves," or varied, which would have been a better word. In the preceding page I have stated of the inhabitants of Dorey: "The majority have short woolly hair;" so that there is no difference from them in that respect. In all I have written about the Papuans I have maintained that the people of New Guinea and of all the immediately surrounding islands are of one race, with very unimportant local differences; and I do not think my remark, that the people of one village were "*rather* uglier" than those of another, three miles off, justifies the idea that I supposed there was any "difference," in an ethnological sense, between them. I cannot find that I have said a word about difference of stature.

The great success of both Messrs. D'Albertis and Meyer in penetrating inland in New Guinea will, it is to be hoped, induce other travellers to attempt the exploration of the far larger and less known southern portion. Two Europeans, with a small steam launch and a Malay crew, would, no doubt, be able to penetrate a long way up some of the larger rivers, and establish a station from which exploration of the central mountains might be effected. There is now no portion of the globe so completely unknown as this, or which promises such great results for every branch of Natural History.

ALFRED R. WALLACE

Deep-sea Sounding and Deep-sea Thermometers

WITH reference to the discussion which has recently been carried on in NATURE as to the deep-sea thermometers, I hope that perhaps the following statement may tend to put the matter at rest.

One of Negretti's thermometers was exhibited at the Royal United Service Institution at a lecture, March 11, 1859, by Admiral FitzRoy, who then spoke of them "as thermometers peculiarly constructed, self-registering," &c. The construction of these thermometers had been fully described in the "First number of Meteorological Papers, 1857," and was subsequently given in a "Treatise on Meteorological Instruments," published by Negretti and Zambra in 1864. The peculiarity of these thermometers was mentioned in the Hydrographic Instructions to Captain Dayman of the *Cyclops* Sounding Expedition, dated May 29, 1857. These facts are sufficient to show the ample publication of the device in question for protecting the bulbs against pressure.

I know from Dr. Miller himself that he did not know of Negretti's plan. In his paper in the Royal Society Proceedings, he calls the one which he describes a "simple expedient." I am not aware of any just claim on the part of Mr. Casella to the principle of the invention.

I consider that the practice of instrument makers designating by their names instruments which they have not *invented*, is most reprehensible.

ROBERT H. SCOTT

London, Dec. 9

[We have received a letter on this subject also from Mr. Casella, but as there is nothing in it bearing on the real point at issue, we do not print it. The above letter from Mr. Scott renders it clear to us, and it will doubtless be also clear to our readers, that the whole credit of the double bulb belongs to Messrs. Negretti and Zambra. We quite agree also with Mr. Scott's closing remarks. This correspondence must now cease. —ED.]

The Dutch Photographs of the Eclipse of 1871

ABOUT a year ago Dr. Schellen kindly sent me two paper copies of the Java photograph, one of them was stated to be of the size of the original negative and the other was an enlargement of about ten and a half diameters, with a delicately soft outline and much detail in the corona. On comparing this with the Indian photographs I found that though the outline of the corona corresponded depression for depression with the two Indian series, yet there was great difference in the detail of the lower parts. The question therefore arose, Was such difference to be regarded as proof of enormous change in the corona in the course of about an hour, during the passage of the totality shadow from India to Java?

I had carefully compared and catalogued the details visible upon the original negatives of the two Indian series, and had found no structure in the one that could not be traced in the other, but the details of the new Java photograph were quite of a different character, lumpy, and in more definite masses. On mentioning this to Lord Lindsay he informed me that he had other copies of the Java negatives which he had received directly from Prof. Oudemans and which were almost structureless. Mr. Davis undertook a critical comparison of the two Java photographs, and pointed out that in spite of the striking dissimilarity of the paper prints, they were evidently both taken from the same original, for they each showed a faint scratch and three minute photographic flaws in the same relative positions. It was impossible to assert that the one was a good print and the other a very bad one, for in the photograph with the delicate corona the moon's limb was soft and hazy, while with the poor corona the limb was perfectly sharp and definite. We had only one course left, and that was to infer that the softening and details had been produced artificially. Having detected manipulation in the corona, we naturally suspected it in the moon's limb, and thus arose my remark at the meeting of the Astronomical Society, that the sharp edges of the irradiation under the prominences might have been artificially produced by stopping out the moon, or rather by stopping out the hazy irradiation which presents so marked a feature, especially under the prominences in the Indian photographs, as well as in those taken in 1870.

There is still a little mystery which requires clearing up about the hazy irradiation. No trace of it is to be found in the copies of the Shelbyville photograph taken by Mr. Whipple in 1869, nor (as we now learn) in the Java photographs, although the action of the light has been greater in these than in some of the Indian and 1870 negatives, which show it as a very marked feature. We know that under ordinary circumstances hazy irradiation is produced by reflection at the hinder surface of the glass on which the photograph is taken, and that its amount may be greatly reduced by backing the plate, during its exposure, with wet paper, so as to produce a film of water instead of a film of air immediately behind the plate, thus causing nearly all the light to be transmitted instead of reflected at its back surface. Yet the Baikul photographs (and I understand also the Cadiz photograph of 1870) were backed with wet paper, and still show the irradiation very markedly.

The cause of the ellipticity of the dark moon touched upon by Prof. Oudemans seems to me to involve some very interesting questions. It is remarkable that the ellipticity does not occur in all eclipse photographs. After making allowance for the moon's motion during 40 seconds in the enlargement from the Cadiz negative, I may say that I have not been able to detect any difference between the polar and equatorial diameters in *any* of the 1870 photographs.

In No. 2 of the glass copies from the Ottumwa photographs, 1869, the moon is also apparently quite circular; but in No. 4, where the bright depths of the chromosphere are just appearing, the polar diameter is distinctly the longest. I have been led to conclude that the ellipticity is caused by an unequal eating over or irradiation at the polar and equatorial portions of the limb, and that in this lies proof that at the sun's equatorial regions the brighter layers of the chromosphere extend to a greater height than near the poles. We know from other sources that the corona generally, and probably also its lower portions, were not so bright in 1870 as in 1869 and 1871; hence the eating over between the prominences has been comparatively slight, and no detectable difference has been caused between the polar and equatorial diameters.

A. COWPER RANYARD

The British Museum

IT is strange that such a statement as that advanced by Mr. W. Stanley Jevons in NATURE, Nov. 13, has so long remained unchallenged, viz. "that the British Museum exists not so much for the momentary amusement of gaping crowds of country people, who do not understand a single object on which they gaze, as for the promotion of scientific discovery, and the advancement of literary and historical inquiry." No one will dispute the truth of these statements, but substitute the word "instruction" for "momentary amusement," and I very much doubt if his views would meet with public approval. I have always looked upon the British Museum as the National Museum, and *pre-eminently* the Museum of the people, and, as such, the arrangement and labelling of the specimens should be of the most simple and instructive nature: nor is such an object opposed to, but perfectly coincident with, the highest interests of science. No wonder the Museum is filled with "gaping crowds" when nothing is done to instruct them as to the nature of objects of which Mr. Stanley Jevons himself admits they are ignorant, nor to provide them with a suitable and educational guide-book, without which they are as sheep without a shepherd When the Trustees of this Museum can spare time, they may, perhaps, be able to direct attention to the fuller development of its scientific and educational functions; as regards the former, by the establishment of one exclusively British Department; and, as regards the latter, by carrying out the very obvious suggestions which I have advanced. The view that science, or rather scientific men, should have a monopoly of the benefits to be derived from this Institution is astoundingly selfish and narrow-minded. If such are the views of the Trustees, the British Museum had better be closed to the public.

S. G. P.

Moraines

I HAVE recently been visiting some of those spots which, according to Prof. Ramsay and other geologists, are marked by moraines of the ancient glaciers of North Wales, and several of which are supposed to form the retaining walls of lakes or tarns: and a question has arisen in my mind to which neither my own consideration nor any of the few books here at my command has afforded any answer.

A glacier which has retreated from its terminal moraine, is always the source of a stream of water, and this stream always cuts through the terminal moraine, and makes in it a gap often wide, and always reaching down to the level of the original soil. A terminal moraine from which a glacier has retreated is the rim of a saucer with a cleft in it, extending to the bottom of the saucer. It consequently cannot and does not act as a retaining wall, and the water from the glacier does not form a lake, but flows out as a stream. No better illustration of this fact occurs to me than the Rhone glacier, with its long series of terminal moraines, all intersected and cut through to the ground by the infant Rhone. How then can a terminal moraine ever form a lake? But if a terminal moraine alone cannot form a lake, a terminal moraine with a stopper put into its hole might. But how is the stopper to get there? Why should *débris* or stones or any other stopper stay in the one place in the whole line where there is no resistance?

Where the basin of the lake is supposed to be constituted by a rock basin and a moraine on its rim, what I have said has, of course, no application to the rock basin, but seems to me to apply to show that the moraine cannot constitute any part of the retaining barrier.

And again, where the retaining barrier is supposed to be constituted by a marine terminal moraine, *i.e.* by a moraine deposited under the sea, the observations I have made seem not to apply.

My questions apply to ordinary terrestrial terminal moraines. They are so simple and go so to the root of the whole notion that such moraines can form lakes that I presume they have been answered long ago by geologists. Can any of your readers tell me where such answers are given or what they ought to be?

Bryn Gwyn, Penmaenmawr, Oct. 13 EDW. FRY

The Elevation of Mountains and the Internal Condition of the Earth

I HAVE just read in NATURE, vol. ix. p. 62, Captain Hutton's letter to the Rev. Osmond Fisher on the "Elevation of Mountains and Volcanic Theories." I was also indebted some time since to the courtesy of Captain Hutton for a copy of his lecture on the Formation of Mountains, delivered at Wellington, New Zealand, November, 1872. Without entering at present into a discussion upon the particular theory which finds favour with him, I may be permitted to call attention to the fact that Sir William Thomson's views as to the rigidity of the earth have been distinctly called in question in a former number of this journal, which has probably not reached Captain Hutton. I refer to my communication entitled "The Rigidity of the Earth," printed in NATURE, vol. vii. p. 288. Captain Hutton expresses his belief that the theory of internal rigidity has probably a weak point somewhere. I venture to think that its weak points are so many as to make it a theory too brittle to form a support to any geological superstructure.

Dublin, November 28. H. HENNESSY

METEOROLOGIC SECTIONS OF THE ATMOSPHERE

THE primary object of meteorology is to record the pressure, the temperature, the moisture, the electricity, and the movements of the atmosphere. It is desirable, however, that observations on these subjects should be combined with the elements of time and distance. At the general meeting of the Scottish Meteorological Society on June 26, 1867, I proposed the method, since generally adopted, of reducing the intensity of storms to a numerical value by the calculation of barometric-gradients, or in other words by dividing the difference of reading of any two barometers by the distances between the stations where such barometers are placed, thus introducing a nomenclature of universal application, by which the movements of any aerial current, and particularly the wind force of storms, may in every part of the world be reduced to one standard of comparison; and the calculation of thermometric, hygrometric, and electric gradients was subsequently proposed. Since then I suggested to the same society the extension of this system by the establishment of a series of barometers placed at short distances from each other in one or more than one direction in azimuth, so as to give horizontal atmospheric sections for pressure. By means of such lines of section the maximum gradient during storms might, from the nearness of the stations to each other, be ascertained, and thus the phenomena of local storms and other local atmospheric disturbances investigated with some hope of success; and since then a horizontal section extending landwards from the sea-shore has been proposed for temperature and moisture, chiefly with the view of determining the extension inland of the influence of the sea on climate.

It would be important were the system of meteorological sections extended to the vertical as well as the horizontal plane. If a string of stations were placed at short horizontal distances from each other and extending from the bottom to the top of a high hill or mountain, the section thus obtained would show the relative distribution at different times, of pressure, temperature, humidity, &c., in the vertical plane. In Scotland, the existing station of Drumlanrig is 191 feet, and that at Wanlockhead 1,334 feet above the sea, so that the difference in elevation is 1,143 feet. The horizontal distance between them is 9 miles, and in all probability the necessary number of intermediate stations could be established. In Hong Kong the town of Victoria is 1,666 feet below that of Blockhouse Victoria Peak, while in Switzerland

and other mountainous districts many other suitable places might no doubt be found.

Would it not be possible to secure funds for establishing at least one such atmospheric section on the slope of some steep hill or mountain in connection with a station or two on an adjoining level district of country?

THOMAS STEVENSON

ON THE PHYSIOLOGICAL ACTION OF OZONE

AT a meeting of the Royal Society of Edinburgh on the 1st inst., a communication was read from Mr. Dewar and Dr. M'Kendrick on the physiological action of ozone. The authors, in the first place, pointed out that little was known regarding the action of this substance, except its peculiar smell and the irritating effect it had on the mucous membrane of the respiratory tract. Schonbein had shown that a mouse died in five minutes in an atmosphere highly charged with ozone; and it was this distinguished investigator who asserted that there was a relation between the quantity of ozone in the air and the prevalence of epidemic diseases. The action of ozone was therefore a subject to be elucidated; and having occasion to employ ozone in another experimental inquiry, the authors resolved to investigate the matter. The ozone was made by passing a current of dry air or oxygen from a gasometer through a narrow glass tube, bent for convenience like the letter U, about 3 ft. in length, and containing a platinum wire 2 ft. in length, which had been inserted into the interior of the tube, and one end of which communicated with the outside through the wall of the tube. Round the whole external surface of this U-shaped tube, a spiral of copper wire was coiled, and the induction current from a coil giving half-inch sparks was passed between the external copper to the internal platinum wire, so as to have the platinum wire as the negative pole in the interior of the tube. After the stream of gas was ozonised by the transmission of the induction current, it was washed by passing through a bulb-tube containing caustic potash, when air was employed, or water when pure oxygen was used, in order to eliminate any traces of nitrous and nitric acids that might have been formed. By means of the gasometer, the volume of gas passing through the tube could be ascertained.

The action of ozone was determined (1) on the living animal enclosed in an atmosphere of ozonised air or of ozonised oxygen; and (2) on many of the individual living tissues of the body. Numerous experiments were made on frogs, birds, mice, white rats, rabbits, and on the authors themselves. Two experiments may be given here as illustrating the action of ozone on (1) a cold, and on (2) a warm-blooded animal.

1. *On a Frog.*—A large, healthy male frog was introduced into the air chamber, through which a current of air was passing sufficient to fill a litre jar in three minutes. At the end of two minutes, the respirations were ninety-six per minute. The induction machine was then set to work so as to ozonise the air. In half a minute, the eyeballs were retracted, so as to appear deeply sunk in the orbits, and the eyelids were closed; the respirations were now eight per minute. At the end of six minutes, the animal was motionless, and there were no respiratory movements. Pure air was then introduced. In half a minute, there was a slight respiratory movement, and in eight minutes there were eighty-five respirations per minute. At the end of other twelve minutes, ozone was again turned on, with the same result. A frog will survive in a dormant condition in an atmosphere of ozonised air for several hours. In one case, the animal died. The heart was found still pulsating. It was full of dark blood. The lungs were slightly congested. The blood was venous throughout the whole body. In ozonised oxygen the effects were, on the whole, the same as in ozonised air, with this difference, that in ozonised oxygen the respiratory movements were not affected so quickly, and were never completely arrested.

2. *On a White Mouse.*—A full grown and apparently healthy white mouse was introduced into a vessel through which a stream of air was passing at the rate of eight cubic inches per minute. Five minutes thereafter, the animal was evidently at ease, and the respirations were over 100 per minute. The air was then ozonised. One minute after, the respirations were slower, but the number could not be ascertained owing to the animal moving uneasily about. In four minutes from the time of the introduction of the ozone, the respirations were thirty-two in a minute. The mouse now rested quietly, occasionally yawned, and, when touched by a wire, moved,—but always so as to remove its nose from the stream of ozonised air. At the end of fifteen minutes, the animal had slight convulsive attacks, which increased in severity until it died—nineteen minutes after the introduction of the ozone. The post-mortem appearances were great venous congestion in all parts of the body. The heart pulsated for several minutes after systemic death. In ozonised oxygen, death was delayed for a much longer period. Instead of dying at the end of fifteen or twenty minutes, as happened to mice in ozonised air, they lived for forty or sixty minutes. It is noteworthy that even after death in ozonised oxygen, the blood was found to be in a venous condition.

On breathing an atmosphere of ozonised air themselves, the authors experienced the following effects:—a suffocating feeling in the chest; a tendency to breathe slowly; irritation of the fauces and glottis; a tingling of the skin of the face and conjunctivæ. The pulse became feebler. The inhalation was continued for eight minutes, when they were obliged to desist; and the experiment was followed by violent irritating cough and sneezing, and for five or six hours thereafter by a sensation of rawness in the throat and air-passages.

The general result of the inquiry may be briefly stated as follows:—

1. The inhalation of an atmosphere highly charged with ozone diminishes the number of respirations per minute.

2. The cardiac pulsations are reduced in strength and this organ is found beating feebly after systemic death.

3. The blood is found after death to be in a venous condition, both in those cases of death in an atmosphere of ozonised air and of ozonised oxygen.

4. The inhalation of an ozonised atmosphere is followed by a lowering of the temperature of the body to the extent of at least 3° to 5° C.

5. The inhalation of ozone does not exercise any appreciable action on the capillary circulation, as seen in the web of the frog's foot under the microscope (200 diameters).

6. In the bodies of frogs killed in an ozonised atmosphere, the reflex activity of the spinal cord is not appreciably affected.

7. By means of a myographion, the work done (in gramme-millimetres) by the gastronemius muscles of frogs subject to the action of ozone was noted. The muscles were stimulated by a single opening or closing induction shock produced by Du-Bois-Reymond's apparatus and a Daniell's cell. The result was that the contractility and work-power of the muscle were found to be unaffected.

8. Ozone has an action on the coloured and colourless corpuscles of human blood and of frog's blood resembling that produced by a weak acid; and in the case of the coloured corpuscles of the frog like that of a stream of carbonic acid. The corpuscles of animals killed in an ozonised atmosphere are normal in appearance.

9. Ciliary action is not affected by a stream of ozonised air or oxygen, provided there is a considerable amount of

fluid covering the cilia; but if the layer of fluid be very thin, the cilia are readily destroyed.

In conclusion, the authors stated that it would be premature, at this stage of the inquiry (which opened up many points of interest in the physiology of respiration), to generalise between physiological action and the physical and chemical properties of ozone; but they pointed out the fact that the density of ozone ($O_3 = 24$) is slightly greater than than that of carbonic acid ($CO_2 = 22$); and that although the chemical activity of the substance is much increased, yet, when inhaled into the lungs, it must retard greatly the rate of diffusion of carbonic acid from the blood, which accounts (from the accumulation of CO_2) for the venous character of that fluid after death. From this point of view, destruction of life by ozone (with the exception of its irritant action) resembles that caused by an atmosphere surcharged with carbonic acid. This has been found to be the case more especially as regards the diminished number of respirations per minute, and the appearance of the blood after death. If, however, the analogy were perfect, the inhalation of an atmosphere of ozonised oxygen would not have produced death, because it is now well known, as shown by Regnault and Reiset,* that animals can live in an atmosphere containing a large per-centage of carbonic acid, provided there is an excess of oxygen present. The amount of oxygen in these experiments converted into ozone certainly never exceeded ten per cent. But the authors have observed that an animal lives only a somewhat longer time in ozonised oxygen than in ozonised air; and they are thus induced to regard ozone as having some specific action on the blood that their future experiments may elucidate. They are now prosecuting a series of researches (*a*) on the action of smaller percentages of ozone; (*b*) on the action of ozone on noxious gases and effluvia; and (*c*) on any therapeutical or hygienic influences it may have on the origin and treatment of zymotic diseases.

THE ATMOSPHERIC TELEGRAPH †

II.

A VERY common question with visitors who witness the departure of a train is,—If the boxes stick on the road how do you manage to disengage them? To answer this question we shall notice in detail the various means employed in transmission, and thus we shall classify the derangements.

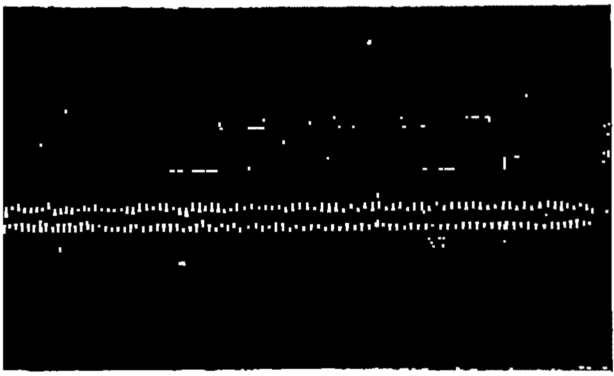

FIG 3—Diagram of the Chronograph 1 Line of the electric *trembleur*. 2. Line of the seconds' pendulum 3 Line of the membrane

Let us commence with the tubes. These may cause an obstruction by a defect of the interior polish, by projecting joints, or by the escape of air through these joints. In the Paris system, however, precautions have been taken against these three sorts of danger. The degree of polish is sufficiently perfect, being obtained without hammering, by pushing the tube along a mandril before it becomes completely cooled. The joints represented in Fig. 2 (p. 66), give an almost mathematical continuity to the interior surface, and they are rendered air-tight by means of India-rubber fittings. In this direction, then, there is little risk of damage and the consequent stoppage of the trains. In fact, since 1866 there has not been a single accident caused by any defect in the tubes, and the experiment is made upon a length of twenty kilometres of pipes so constructed that joints occur every five metres.

The derangements arising from the machinery for compressing the air are not of a special character, and need not be particularised here. There remain the boxes. Numerous types were tried before the system of the two cases in tin and leather, which can be hermetically closed and are easily opened; from its simplicity this method has been adopted. Nevertheless it does sometimes happen that the boxes open during the journey; how this is caused, is not easy to explain in each particular case. Sometimes the collarette of the piston is in a bad condition, and the air divides the train; the cases are separated, and the despatches are scattered in the tube. At other times wrinkles are formed in the envelope of leather, the effect of which is to wedge the train so firmly that it is impossible to make it move. Another form of derangement is when the piston breaks and the pieces are lodged between the boxes and the tube. It is scarcely possible to exhaust the series of accidents of this nature; the mean number of derangements in the working of the system during the year is eight, and it is rare to find the same cause occurring twice. When accidents do occur, it is necessary to make all haste to relieve the train.

Often alternate manœuvres with compressed and rarified air removes the obstruction; at Berlin, for the same purpose, M. Siemens employs water with which he forcibly inundates the tube. The great thing is to extricate the train without having to take the line to pieces. When such means fail it is necessary to have recourse to the operation of excavation; and the necessity will be evident of a preliminary and sufficiently exact determination of the place of derangement. The first means is indicated by the method on which the system is worked. There is at hand a reservoir of compressed air of a certain pressure; if this air is partly distributed in the section of the tube comprised between the reservoir and the obstacle, the new pressure is in a known ratio to the original pressure. In a word, Mariotte's law, which regulates the ratios of the pressures and volumes of the same mass of gas in two different circumstances, furnishes the means of finding *one* of the elements, volume, when we know the three others, two pressures and one volume.

M. Siemens prefers to measure the quantity of water which it is necessary to distribute in order to flood the line as far as the obstacle; the accuracy ought to be very great, but it must be acknowledged that the process, in spite of its apparent simplicity, has a somewhat primitive aspect. It is not difficult to understand how this great mass of water is introduced, but it is very difficult to conceive that it can easily remove the obstacle.

We may speak, finally, of an indirect means which is illustrated in Fig. 4. The reader knows that when a concussion is produced at the end of a tube filled with air, this concussion is propagated in the air of the tube at a speed of 330 metres per second. When the concussion encounters an obstacle, it is reflected and returns to the point of its origin at the same rate of 330 metres per second. If then the time is noted which elapses between the departure and the return, the period thus obtained corresponds to the passage of the concussion along a distance equal to double the distance of the obstacle; from an observation of the time, the distance can be easily calculated.

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* "Air and Rain," by Dr. Angus Smith, p. 182. (London, 1872.)

† Continued from p. 66.

For example:—The interval of time between the departure and return of the wave produced by the concussion is $\frac{1}{3}$ of a second; the double journey is represented by $\frac{330\text{ m.}}{3} = 110\text{ m.}$, and the distance of the obstacle is $\frac{110}{2} = 55$ metres.

The times of the departure and of the return of the wave are graphically registered on a chronograph, by the interruption of an electric circuit obtained by the motion of a membrane of caoutchouc placed at the extremity of the tube.

It is known that an electric current magnetises a horse-shoe magnet. The magnetisation of the magnet communicates to a palette placed above the poles, an attraction which ceases as soon as the current is broken. Without entering into further explanation of this well-known arrangement, which is the basis of nearly all telegraphic apparatus, it will be granted that with conveniently placed conductors it will be possible to make the armature of the magnet move like the elastic membrane; in other words, if the membrane is raised 2, 3, 4 times in a second, the armature will be connected 2, 3, 4 times, and the durations as well as the intervals of the contacts will be identical in the two apparatus.

Let us return to the chronograph. The time is marked by it, and is recorded by means of electro-magnets. The oscillations of a seconds pendulum are repeated electrically and registered on a line, No. 2 in Fig. 4, which is described by a point fixed to the electro-magnet, upon a smoke-blackened cylinder, to which is given a movement of continuous rotation. The electro-magnet whose point describes line. No. 2, is moveable on a carriage that advances along the cylinder in the same time as the latter takes to turn. The carriage bears two other electro-magnets: one corresponds to a sub-divisor of the time which gives fractions less than a second. It is this which traces line No. 1, representing by its tracings sub-divisions equal to $\frac{1}{33}$rd of a second; this division into fractions corresponds to the oscillations of the palette of an electric *trembleur*, a contrivance in which the interruptions and re-establishment of the current take place at the rate of 33 per second in the model here represented.

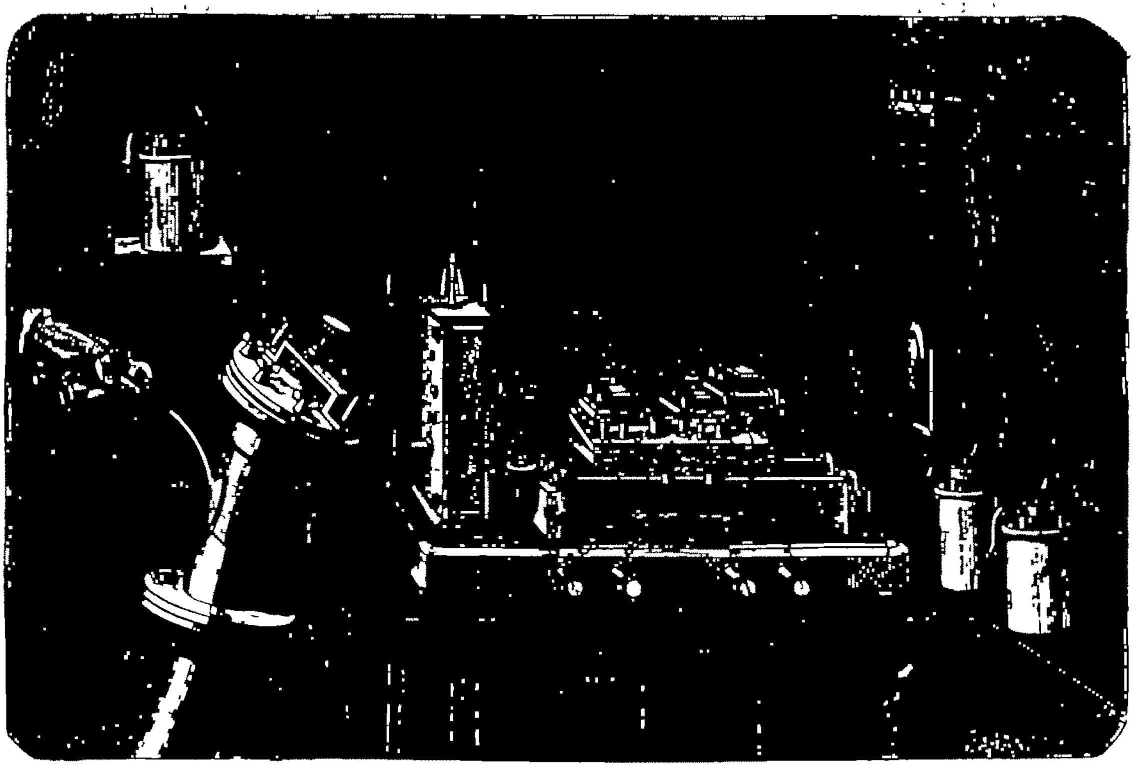

Fig. 4.—Chronograph for determining the point of stoppage of a train.

The third electro-magnet, in connection with the membrane of caoutchouc, corresponds to the movement of the wave in the tube; it furnishes line No. 3 in the figure. It may be remarked that the same wave undergoes many successive reflexions.

It will be easily seen from the diagram how the result sought can be obtained from the experiment. Suppose the obstacle to be placed at 62 metres; the interval between two successive marks of the membrane is about 12 sub-divisions. A comparison of lines 1 and 2 shows that there are 33 sub-divisions in *one* second; the indications of line 3 then are equal to $\frac{12}{33}$ of a second. The double distance represents $\frac{12}{33} \times 330$ m, and the simple length given by the experiment is thus about $\frac{1}{2} \times \frac{12}{33} \times 330 = 60$ metres, the result sought to within 2 metres.

Fig. 4 shows the method adopted for producing the wave. On the left T is the tube in which a small pistol V is placed to produce the detonation which gives rise to the wave. On the table in the centre of the figure is the chronograph; M is the clock-work which turns the registering cylinder, on the surface of which are traced the lines 1, 2, 3; S is the carriage bearing the three electro-magnets, each of which traces its line. The electro-magnet on the right, line 1, is the *trembleur*, in connection with the pile P P′. The middle electro-magnet, line 2, is connected with the seconds pendulum R. Finally, the electro-magnet on the left, line 3, communicates electrically with the caoutchouc membrane that surmounts the tube T, and exactly fits the opening, on which it is stretched like a drum-skin.

THE COMMON FROG*

VI.

The Skeleton of the Frog

IT may cause surprise to speak of the skin of the common Frog as part of its skeleton, consisting as the skin does of soft membranous structures only.

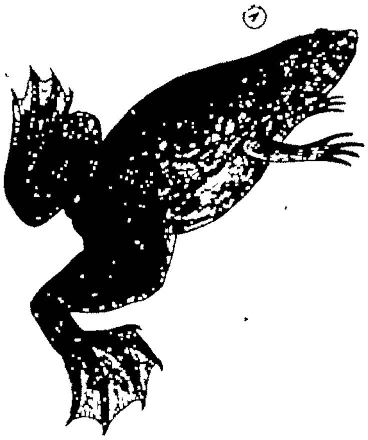

Fig. 26.—*Dactylethra capensis.*

The term "skeleton," however, should properly include all the membranous and gristly, as well as the bony structures.† Moreover, more or less of the skin may attain to so solid a condition as fully to justify its comprehension under the name "skeleton," even in the popular signification of that term.

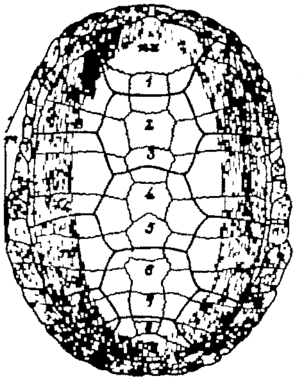

Fig. 27.—Dorsal surface of the Carapace of a Fresh-water Tortoise (Emys) 1—8, expanded neutral spines; r^1—r^8, expanded ribs; *nu*, first median (or nuchal) plate, *py*, last median (or pygal) plate; *m*, marginal scutes The dark lines indicate the limits of the plates of the horny epidermal tortoise-shell; the thin sutures indicate the lines at the junction of the bony scutes.

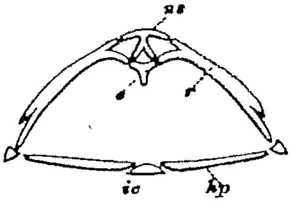

Fig. 28.—Diagram of a vertical section of both Carapace and Plastron of a Tortoise, made transversely to the long axis of the skeleton *c*, vertebral centrum; *ns*, neural spine which expands above into a median dorsal scute; *r*, rib which forms one mass with a lateral scute and terminates at a marginal plate; *ic*, inter-clavicular scute; *hp*, hyo-sternal scute.

The skin of Vertebrate animals consists of two layers: an outer layer (the epidermis or *ecteron*), and an inner layer (the dermis or *enderon*). The *epidermis*, and any projections or processes developed from it when they take on a dense or hardened structure, become *horny*. Of such horny nature are hairs, feathers, nails, and scales, they are more or less dense *epidermal* appendages. The *dermis* when hardened becomes *bony*, and of such nature are the bony skin-plates or "scutes," and teeth. They are *dermal* appendages. Now both layers of the skin of the common Frog are entirely soft and utterly destitute of any of these appendages. Allied forms, however, present us with examples of some interesting epidermal conditions. Thus in old male Toads, in *Dactylethra* and in one of the Japanese efts, the epidermis of some of the finger-tips becomes hardened and horny, in other words we begin to meet with incipient "nails." "Incipient" because, in ascending from the lowest vertebrates, "nails" are first

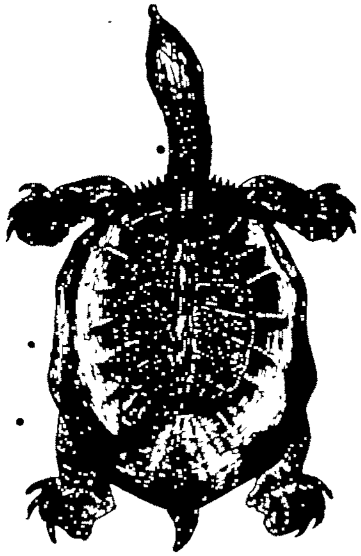

Fig 29—A Mud-tortoise (*Trionyx*), showing the dorsal plates.

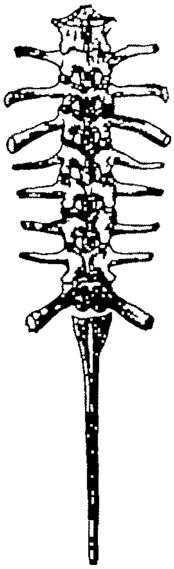

Fig. 30.

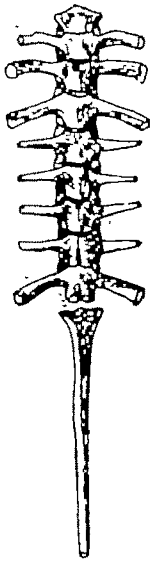

Fig 31.

Fig. 30.—Backbone of the Frog (dorsal aspect). Fig. 31.—Backbone of the Frog (ventral aspect).

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* Continued from p. 69.

† See "Lessons in Elementary Anatomy," Lesson II., p. 22.

met with in the Frog's class, and these only very rarely and in an imperfectly developed condition.

As has been mentioned, in two kinds of Frog (*Ceratophrys* and *Ephippifer*) the skin of the back is furnished with bony plates. These are found in the deeper layer or dermis, and are therefore "scutes."

The remarkable circumstance, however, is that we have here a lower stage (as it were an *incipient* condition) of that more developed dermal skeleton which exists in tortoises and turtles. In most of these reptiles both the back and the belly are protected by bony plates which adjoin one another, and together form a solid box in which the body is enclosed. Moreover the bony plates of tortoises and turtles are invested by large horny epidermal scales ("tortoise-shell"), which scales do not agree in either size or number with the bony plates on which they are superimposed.

Again, the middle series of bony plates of the back are continuous with the subjacent joints of the backbones, and the lateral series of dorsal plates are continuous with the ribs beneath them.

There are certain Chelonians, however—"mud-tortoises"—(of the genus *Trionyx*), which have the dorsal plates much less developed and not connected with the ventral plates save by means of soft structures.

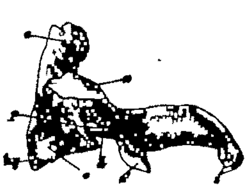

Fig. 32.

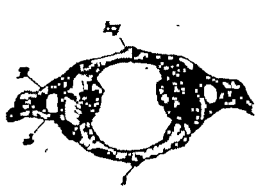

Fig. 33.

Fig. 32.—The Axis Vertebra. *c*, centrum; *s*, neural spine; *d*, tubercular process; *p*, capitular process, *a*, anterior articular surface for atlas; *z'*, postzygapophysis; *o*, odontoid process; *hy*, median vertical ridge beneath centrum Fig 33.—The Atlas Vertebra. *s*, rudiment of neural spine, *d*, tubercular process, *p*, capitular process; *a*, articular surface for skull, *hy*, plate of bone holding the place of a cranium, and articulating with the odontoid process of the axis vertebra.

Here then we have in reptiles an interesting approximation to the condition we have seen to exist in those exceptional Anourans, *Ceratophrys* and *Ephippifer*. Moreover this resemblance is still further increased by the fact that in *Trionyx* the bony plates are not covered with any tortoise-shell, but are merely invested by soft skin as in the genera of dorsally-shielded Batrachians.

Have we then here a true sign of genetic affinity? Are these tortoises to be deemed the more specially modified descendants of shielded frogs or of some as yet unknown slightly-shielded animals which were the common ancestors both of frogs and tortoises?

Certainly tortoises cannot be the direct descendants of frogs, they agree with all reptiles in characters which are both too numerous and too important to allow such an opinion to be entertained for a moment.

The other opinion is hardly less untenable; for if all the multitudinous species of frogs (together with a number of reptilian forms more closely allied to the tortoise than any frogs are) descended from slightly shielded animals, how comes it that all frogs and toads, save one or two species in no other way peculiar, have every one of them lost every trace of such shielded structure which nevertheless cannot easily be conceived to have been in any way *prejudicial* to their existence and survival?

On the other hand, it cannot but strike us with surprise that structures so similar—extending even to the continuity of the dorsal plates with the subjacent joints of the backbone—should have arisen twice in nature spontaneously. Here we seem to have a remarkable example of the independent origin of closely similar structures; and if so, what caution is not necessary before concluding that *any* given similarity of structure are undoubted marks of genetic affinity!

The skin of the frog is also interesting from a physiological point of view. Our own skin is by no means popularly credited with the great importance really due to it. "Only the skin!" is an exclamation not unfrequently heard, and wonder is very often felt when death supervenes after a burn which has injured but a comparatively small surface of the body. Yet our skin is really one of our most important organs, and is able to supplement, and to a very slight extent even to replace, the respective actions of the kidneys, the liver, and the lungs.[*]

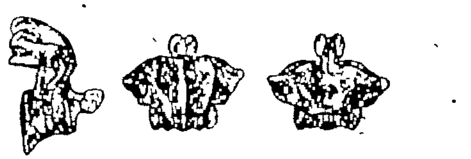

Fig. 34.—Lateral, Dorsal, and Ventral view of first Vertebra of *Amphiuma*.

In the frog we have this cutaneous activity developed in a much higher degree. Not only does its *perspiratory* action take place to such an extreme degree that a frog tied where it cannot escape the rays of a summer's sun speedily dies—nay, more, is soon perfectly dried up—but its *respiratory* action is both constant and important. This has been experimentally demonstrated by the detection of the carbonic acid given out in water by a frog over the head of which a bladder had been so tightly tied as to prevent the possibility of the escape of any exhalation from the lungs. The fact of cutaneous respiration has also been proved by the experiment of confining frogs in cages under water for more than two months and a half, and by the cutting out of the lungs, the creature continuing to live without them for forty days. Indeed it is now certain that the skin is so important an agent in the frog's breathing that the lungs do not suffice for the maintenance of life without its aid.

It is no less true that in Batrachians which breathe by means of permanent gills—as, *e.g.* the Axolotl—such gills are not necessary to life, as the late M. Aug. Duméril and Dr. Gunther have established by cutting them away without inducing any apparent injurious effects. In the whole class of Batrachians skin respiration seems, then, to be of very great importance.

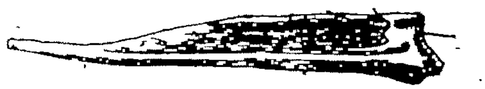

Fig. 35

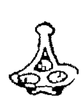

Fig. 36.

Fig. 35.—Coccyx of Frog, lateral view, a black line indicates the course of the sciatic nerve. Fig 36.—Anterior aspect of Coccyx, showing the double articular concavities placed side by side beneath the neural arch.

The *internal* skeleton (or the skeleton commonly so called) of the frog presents some points of considerable interest, especially as exhibiting its intermediate position between fishes on the one hand, and higher vertebrates on the other. First, as regards the *backbone*, it may be remembered that it is made up of distinct bony joints (or *vertebræ*), in which it agrees with all animals above fishes and with bony fishes; its hinder termination, however, is essentially fish-like. It is fish-like, because the terminal piece, as it is called, or "*coccyx*"[†] (unlike the coccyx in man or in birds) is not formed of rudimentary vertebræ which subsequently blend and anchylose together, but is formed by the ossification continuously of the membrane investing (or *sheath* of) the hindermost part of that primitive continuous rod, or notochord, which, as has been said, precedes, in all vertebrate animals, the development of the backbone, making its appearance beneath the primitive groove.

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[*] See "Elementary Physiology," Lesson V., § 19.

[†] From νῶτος, back, and χορδή, chord.

The vertebræ are shaped like rings, and enclose within their circuit the spinal marrow upon which, as it were, these rings are strung. From the side of each ring (except at the two ends of the backbone) there juts out a bony prominence called a "transverse process," and to a certain number of these a bony "rib" is in most vertebrate animals attached (though there are none in the frog), often extending round to join the breast-bone in front, and being capable of more or less motion, so as (by their simultaneous movement) to be able to enlarge or to contract the cavity of the chest, which they thus enclose and protect.

That part of each vertebra which is placed next the body cavity is generally the thickest part, and is called the "body," or "centrum." The series of bodies (or centra) occupy the position which was at first filled by the primitive notochord, the rest of the vertebral rings having been formed in the sides and roof of the canal formed by the upgrowth and union of the two sides of the primitive groove of the embryo.

The frog order is distinguished amongst vetebrates as that which has the absolutely smallest number of joints in the backbone. In the frog there are but nine in the front of the coccyx. In the Pipa toad there are but seven, the eighth vertebra (to the transverse processes of which the haunch bones are attached) having become solidly joined in one bone with the coccyx.

In all higher vertebrates, *i.e.* in all beasts, birds, and reptiles, the head is supported on an especially ring-like vertebra which—because it so supports—is called the *atlas*, and this (in almost all) can turn upon a peculiar vertebra termed (from this circumstance) the *axis*, and provided with a toothlike (*odontoid*)[*] process, round which, as round a pivot, the "atlas" works. Nothing of the kind exists in any fish.

In the frog (and in all its class) we find but a single vertebra representing these two, but in some allied forms, *e.g.* in *Amphiuma*, this vertebra develops a median process, reminding us of the odontoid process of the axis.

The frog, as has been said, has no ribs, in spite of the long "transverse processes" which project out on each side of the backbone. Ribs are not necessary to it, for it could apply them to none of the purposes to which ribs are ever applied.

In all beasts ribs aid importantly in respiration, serving by their motions alternately to inflate or empty the lungs by enlarging or contracting the cavity of the chest in the way before mentioned. The frog, however, breathes exclusively, as regards the lungs, by *swallowing* air by a mechanism which will be described shortly.

In serpents the ribs are the organs of locomotion, as also in the Flying Dragon before referred to; but in frogs locomotion is effected exclusively by the limbs. In the very aberrant species, *Pipa* and *Dactylethra* there are on each side of the anterior parts of the body two enormously long transverse processes, each process bearing at its extremity a short flattened, straight osseous or cartilaginous rib. These little ribs can, however, take no part in such functions as those just referred to.

Ribs, moreover, are found in the other existing orders of the frog's class, *i.e.* both in the *Urodela* and *Ophiomorpha*. In none, however, do they join a breast-bone, or sternum, another character in which *Batrachians* agree with fishes, though they differ from fishes in that they have a sternum at all. In ascending from fishes through the vertebrate sub-kingdom, a sternum first appears in the class *Bratrachia*.

In a certain North African Salamander named *Pleurodeles* the ribs are not only elongated, but their apices, if they do not actually perforate the skin, are so prominent as to seem to do so when the finger is drawn from behind forwards along the side of the animal's body.

The several joints of the backbone are connected together by surfaces which are not the same on both the anterior and posterior sides of the centrum, or body, of the same vertebra. Each of the first seven vertebræ is furnished with a round prominence, or head, on the hinder side of its centrum, and each of the precoccygeal vertebræ, except the first and last, has the anterior surface of its centrum excavated as a cup for the reception of the ball of the hinder surface of the vertebra next in front. The first vertebra has in front two concavities, side by side, to articulate with the skull. The eighth vertebra has a concavity at each end of its "body." The ninth vertebra has a body provided with a single convexity in front and a double convexity behind, to articulate with the concavities placed side by side on the front end of the coccyx.

These arrangements are not constant in the frog's order, still less in its class.[*] In *Bombinator* and *Pipa* the vertebræ are concave behind each centrum, instead of in front: and the same is the case in *Salamandra*. In many tailed Batrachians the vertebræ are biconcave, as *e.g.* in *Spelerpes*, *Amphiuma*, *Proteus* and *Siren*.

The biconcave shape is an approximation towards the condition which is almost universal in bony fishes, though not quite universal, since the bony pike (*Lepidosteus*) has a ball at one end of each vertebra and a cup at the other. Moreover, even in some reptiles (*e.g.* the lizards called *Geckoes*) the vertebræ are biconcave, and the same was the case with the majority of those species of crocodiles the remains of which are found in strata older than the chalk, and even in existing crocodiles the first vertebra of the tail is biconcave.

Vertebræ with a cavity in front of the centrum and a ball behind it are found in the crocodiles now living as well as in the frog, while vertebræ with a ball in front and a concavity behind are found even amongst beasts, as in the joints of the neck of Ruminants, *e.g.* the sheep. Thus though the vertebræ of the frog's class exhibit no very decided signs of affinity, they show more resemblance to those of fishes than to those of any other non-batrachian class.

The transverse processes of the ninth or last vertebra in front of the coccyx, articulate with the haunch bones, but are not very remarkable in shape. In some frogs and toads the transverse processes of this vertebra become enormously expanded, and the expanded or non-expanded condition of this part is a character made use of in zoological classification. The coccyx is made up mainly, as has been said, of a continuous ossification of the sheath of the notochord, and never consists of distinct vertebra. Nevertheless, the small bony arches which are at first distinct coalesce with it. These arches are called "neural" because they arch over the hinder part of the spinal marrow. The great nerve of the leg (the sciatic nerve) proceeds outwards on each side through a foramen situated at the anterior end of the coccyx from the spinal marrow—the spinal marrow being that structure which gives origin to the great mass of the nerves pervading the entire frame.

ST. GEORGE MIVART

(*To be continued.*)

[The author sincerely regrets, that by an inadvertence for which he is exclusively answerable, two cuts introduced into the second of these articles, namely, the figures representing *Rina esculenta* and *Bufo vulgaris*, were copied, without sanction, from two illustrations in Professor Bell's "History of British Reptiles," published by Mr. Van Voorst, to whom, therefore, this apology is due]

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[*] From ὀδούς, a tooth, and εἶδος, form.

EARTH-SCULPTURE *

III.

I DO not consider it necessary to defend my facts. They are familiar enough to the geologists of this country, as displayed more or less plainly in every district of our island. I am at present concerned with the counter-statements which the Duke of Argyll would put in their place.

He states his belief that the Highland mountains have had their contour mainly given to them by "upheavals, subsidences, and lateral pressures, which have folded them and broken them into their present shapes." A belief of any kind must be founded on evidence of some sort, and that evidence must be produced if the owner of the belief desires that it should be accepted by others besides himself. What evidence, then, does his Grace furnish as the basis on which he expects that his "belief" is to supersede what he is pleased to term "the extravagant theories of the younger glacialists"? Having shown "the antecedent improbabilities involved in the extreme theories of erosion," he states that he "proceeds to test them on the field of fact." We follow him anxiously to the field in question, and find that his so-called facts are stated in such words as these: "Loch Fyne . . . occupies, as I believe, the bed (*sic*) of an immense fault." "The transverse valley of Loch Eck lies across a steep anticlinal, and is due, in my opinion, to the extreme tension to which the crystalline rocks have been subjected." "The Pass of Awe is a rupture and chasm." These, and other similar assertions regarding various parts of the Highlands are confidently expressed, but they are accompanied by no evidence by which their accuracy may be tested. In truth, the "facts" which his Grace adduces in support of his "belief" are only other "beliefs" and "opinions" of his own. They may be correct or the reverse, but they cannot legitimately be adduced as evidence in a scientific argument. But they are very far from correct. I utterly deny, for example, the assertion that Loch Fyne lies along the bed of an immense fault, and I ask the Duke of Argyll to try to prove that it does so. Nay more, I challenge him to produce a geological section which would bear a moment's examination on the ground, in which he can show the coincidence of a valley with a line of fault in any part of his own county of Argyll. That cases of this coincidence may be found I do not doubt, but the search for them will be useful in teaching his Grace how exceptional they are.

The Duke of Argyll does indeed offer some explanatory statements regarding some of his assertions of fact. For instance, with regard to Loch Awe, he dwells on the inclinations of the slates and the intrusion of the porphyries among them as evidence that the present contour has been directly the result of subterranean convulsion, and he triumphantly adduces these and similar appearances "ignored" by myself as a demonstration of the truth of his "belief." But any one who knows the Highland rocks at all may well smile when he is told that a geologist who had ever been over the ground, even in the most cursory way, requires to have these phenomena pointed out to him. In reality I had already granted the existence of these, and far more wonderful evidences of underground movements, for I knew the Highland rocks well, and had mapped their structure over leagues of ground from the mountains of Sutherland to the moors of Forfar, and the headlands of Islay. I was therefore perfectly familiar with the phenomena to which the Duke of Argyll so confidently refers. But I had learned more about them than merely their tale of subterranean turmoil. I had found that they did not bear directly on the origin of hill and valley at all. I had traced everywhere evidence that what we now see of intruded granite or curved slate has been laid bare only after the removal of hundreds and thousands of feet of rock under which they once lay. His Grace, it would seem, has still this lesson to learn, and until he has mastered it, and, apart from any theory but simply as a matter of demonstrable fact, has realised what it involves and how vain is the attempt to connect the contorting and hardening of the rocks with the *present* surface features of the country, argument with him on this question seems hardly possible.

Again, I had quoted the mountain Ben Lawers, with its flanking hollow in Loch Tay, as a typical example of the kind of evidence which could be abundantly adduced from all parts of the Highlands to show the relation between geological structure and external form, and to prove from under what an enormous mass of removed rock the present surface of the Highlands has appeared. I gave a section to show at a glance the broad facts of the case—a section from which no conclusion is possible but that which I drew. But here, once more, the Duke of Argyll's belief in the all-powerful efficacy of granite and igneous rocks, or his thraldom to what he calls "the influence of a preconceived theory," brings out in well-marked prominence that obliquity of vision which prevents him from seeing anything but convulsion and fracture. He scents intrusive rocks of some sort along the south bank of Loch Tay. It would be vain to remonstrate that this alleged influence of the igneous rocks is, to say the least, as pure "invention and imagination" as anything which the "younger school" could readily supply, or that the denudation of that region is a momentous fact to be looked in the face and explained, not to be dismissed or denied, no matter what our "theory" or "belief" may be regarding the origin of granite. Without further ceremony, the proofs of enormous denudation at Ben Lawers and Loch Tay, together with their luckless advocate, are all bundled off with the summary judgment, so happily appropriate to its own author, "I attach no value whatever to a theory which passes over and ignores this class of facts altogether."

The dogmatic assertions which the Duke of Argyll makes regarding the influence of granite and other rocks upon the surface, and as to the existence of fractures and depressions along the line of valley and glen, are really most flagrant examples of the *petitio principii*. In effect, his Grace tells us, "The 'inventions and imaginations' of these younger men are based upon 'assumed facts' which 'are, in my opinion, to a large extent purely hypothetical.' I am 'suspicious of the influence which a preconceived theory has had on their estimate of evidence.' I therefore 'attach no value whatever' to their statements, and do not consider it necessary to lose time in weighing what they actually mean by this denudation of theirs, and all which, as they contend, must flow from it. My belief is that valleys are due to fractures and depressions. The Highlands abound in valleys, and therefore it must be evident to everyone capable of forming an opinion on the subject, that they abound also in proofs of fracture and depression."

In the foregoing remarks I have been dealing only with the Duke of Argyll's paper of February 1868, which in his recent vigorously-worded address he cites as still unanswered, and which, therefore, we may suppose still to express his views. And yet no one can peruse that address without perceiving that it betokens a considerable change of opinion. Especially gratifying must it be to that "younger school" of geologists against which the Duke has so vehemently lifted up his protest, to observe that the lapse of time which he would not allow to have had much denuding effect upon the rocks, has yet been able to strip off from himself some of that crust of preconceived "theory" against which no argument or adverse fact could once make any impression. It is true that his Grace formerly thought it necessary to assure us that Time could do nothing by itself, "nothing except by the aid of its great ally Force—Force working in Time." Well, we shall not quarrel about the use of words, but cheerfully admit that the change which has become perceptible in the opinions of the Duke of Argyll is wholly the result of "Force working in Time," and not a very long time, for it cannot be stretched out beyond five years. Surely if the lapse of so brief a space, with all the amount of Force which we can crowd into it, can have modified geological opinions which certainly seemed as solidly and unalterably fixed as his own Ben More itself, it can hardly be too much to hold that by the end of another *lustrum* still further modification may justify the confident belief that his Grace may still come to join the "younger school" heart and hand. We can assure him a jubilant welcome.

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* Opening Address to the Edinburgh Geological Society, by Prof. Geikie, F.R.S. (continued from p. 91).

But it may be asked what is the nature of this present alteration of view? In brief, it may be put thus: the Duke of Argyll finds that, after all, denudation is one of those disagreeable facts which will insist on being prominent—"chiels that winna ding." He has discovered that it really has had some share in the shaping of the present outlines of the land. He now admits in words "that the forms of hill and valley which preceded the coming-on of glacial conditions [during the Ice age] had been themselves determined in a large degree by previous denudations." And even though this general admission is neutralised by statements which follow it, it is most welcome as an indication doubtless of the effect of those "more extended opportunities of observation" which his Grace tells us he has since enjoyed, and on the continuance of which our hopes of his secession to the ranks of the "younger school" are mainly based.

The Duke of Argyll appeals once more to the details of geological structure. Most gladly do we accede to the appeal. He points to the contorted condition of the older rocks as evidence of the extent to which they have been affected by subterranean movements. But no geologists are more familiar with these facts than his maligned "younger school." He conceives that it was after such movements that the forces of denudation began to work. Most assuredly; this has been explained over and over again. He affirms that "so long as such hills and mountains last at all, and wherever they are exposed to view, they bear upon them the unmistakeable impress of their origin and of the mighty subterranean forces to which their structure is due." This sentence is rather ambiguous. If it means that contorted rocks retain evidence of contortion, such an obvious truism was hardly worth a sentence to itself. If it means that a mountain made of contorted rocks has had its form determined at the time of contortion, the statement is mere assertion and a begging of the very question to be proved.

In the same address the noble president declares that "denudation has done its work along the lines determined by upheaval, by fracture, and by unequal subsidence." This has never been denied by anyone. A main object of my book was to show how, by means of denudation along such lines, much of the present contour of Scotland has been produced. Again we are told—"All sedimentary beds must have had an edge somewhere; and if they are lifted into a vertical position and the edges come to be exposed, the removal of a small amount of material may result in a horizontal surface, or, in surfaces cutting across the lines of structure at every variety of angle." If the Duke intends this explanation to apply not to a mere hand specimen, but to any district of convoluted and vertical rocks, such as the hills of Wales or the Southern Uplands or Highlands of Scotland, he cannot have noticed the string of physical absurdities which it involves. The rocks are often vertical, or nearly so, for miles at a stretch. Could we put them into something like their original horizontal or gently inclined position their present edges would end off in a cliff many miles high. Can his Grace expect anyone to believe that the beds, which certainly "must have had an edge somewhere," ever ended off in that fashion? But this would be only a part of the feat. In actual fact the rocks have been violently contorted, so that a series several hundreds or even thousands of feet in thickness is folded again and again upon itself. The present surface has been cut across these foldings, and in great part has its inequalities independent of them. If we could flatten these curved rocks out again from their present condition they would show a series of deep sharp troughs separated by steep pyramidal ridges of flat strata. And from the Duke of Argyll's teaching we should learn that this wonderful arrangement was the normal plan in old times of laying down sediment which, instead of always going to the bottom and filling up the hollows as it does nowadays, contrived then to ascend, layer after layer, like the tiers of the Great Pyramid, as if, it were under the impulse not of mere gravity or of the play of ocean currents, but of the methodical action of organisms like the coral polyps. We should further learn that these neatly-shaped sand and mud ridges and troughs were so accurately laid down that when subterranean forces came into action and crumpled the whole up, every ridge popped conveniently into a trough below, as if a trap-door had been opened for its reception, and with such nice adjustment as to bring its top to the same general level as the bottom of the former troughs!

The truth is, and, in common fairness I am bound frankly to state it, that such assertions as these with which I am dealing, could never be made if geological structure were really understood and kept in view. This is a matter of science, and is only to be mastered by the same patient toil which is required in other scientific inquiries. Moreover, it is by no means so easily mastered as it seems to be. The first absolute requisite for overcoming our ignorance, is to reduce our facts to the test of ocular proof and measurement. Let us construct a section across the tract of which we would master the structure, and to avoid risks of error from exaggeration of proportion, let us begin by making the section as nearly as possible on a true scale, that is, giving the same value to length as to height. With the outline of the ground accurately traced we may then, section in hand, insert upon it at the proper places, and with the true angle of dip, such rocks as we be able to see exposed. Having fixed these data in this patient way, we may expect with some confidence to understand and fill in the geological structure of the ground for ourselves, and to make it intelligible and credible to others. Until we have gone through such a training ourselves, or have learnt adequately to appreciate what it is from the labours of others, we have no right to utter an opinion on the relations between geological structure and external form, for we are destitute of one of the necessary qualifications for dealing with the problem.

The greater part of the recent address of the president of the Geological Society deals with the traces of ice-action in this country, and the manner in which they are to be accounted for. In his remarks upon this subject, the Duke again places himself in opposition to the views of the "younger school," and expresses opinions from which every member of that school would, I am sure, emphatically dissent. It is no part of my present purpose to enter upon these. I cannot, however, pass by one statement in the address. His Grace asserts that these restless "younger geologists" have recently made a most complete change of front. He therefore directs his attack against this new position. He says that they no longer maintain the existing systems of hill and valley to have been cut out of the solid by an enormous glacier, but admit the general contour of the country to have been very much the same before the Ice age as after it, all the work of the ice having been to deepen valleys, degrade hills a little, and fill up the plains and hollows with clay and sand. "Such as I understand it," says the Duke, "is the new glacial theory." But surely he can have paid but scant attention to the subject if he imagines that this idea is in any sense new. I really cannot recall that the geologists of the "younger school" have for many years past held any other view than that which they are now said to have adopted only recently. If, for example, his Grace will turn to the little volume which he abused so heartily in the spring of 1868, he will find the "new view" stated as plainly there as words can express it (see page 150). And yet in this address he thinks it needful to adduce evidence to disprove that valleys have been gouged out by an universal ice-sheet—a notion which, according to his own showing, the "younger school" does not hold.

These remarks have been extended this evening beyond the length to which I had originally proposed to confine them. My excuse must lie partly in what to myself is the ever fresh charm of the subject, and partly in the desire to vindicate the fair fame of the modern Huttonian school of geology from attacks which had been in some measure called forth by writings of my own. I have again to express my regret that it was impossible to avoid an appearance of personal conflict, and I am conscious that a man who does his best to give as good as he gets in such conflict is apt to do more than he meant. I can only hope that this consciousness has kept me far within the bounds of legitimate reply.

Of one thing I feel securely confident. When the din of strife has ceased and men come to weigh opinions in the dispassionate light of history, the profound influence of the Huttonian doctrines of the present time on the future course of geology will be abundantly recognised. By their guidance it will be possible to reconstruct the physical geography of the continents, in successive ages back, perhaps into some of the earliest periods of geological history. This work indeed is already in part accomplished. But much more remains to be done before the history of the land on which we live has been wholly unravelled. This is the task to which we have set ourselves, in which we have found ample scope for enthusiasm and hard work, and out of which we trust that there will eventually come a story of permanent interest to all whose range of vision extends beyond the present condition of things, and who would fain understand what now is by the light of what once has been.

EXTERMINATION OF MARINE MAMMALIA

THROUGH the kindness of a friend, there has been placed in my hands a little book—one of the few copies in England—which though not much bigger than a pamphlet, seems to me more deserving of notice than I fear it is likely to obtain. Of its author, I may say, I know nothing. Its title is "Mammalia, Recent and Extinct; an Elementary Treatise for the use of the Public Schools of New South Wales. By A. W. Scott, M.A." It is published at Sydney by Thomas Richards, Government Printer, and bears date 1873. One's first wish on looking at it was that such a book might be wanted "for the use of the Public Schools of" the old country; but it is not my object now to enlarge on this theme or even to call attention to, or pass judgment upon it from a scientific point of view—though some of the author's opinions are, if not novel, such as have not been generally received. Mr. Scott's treatise is confined to the "*Pinnata*, Seals, Dugongs, Whales," &c. and he tells us in his preface why he has so limited it:—

"Whatever information we possess upon the natural history of the finned mammals, particularly in a popular, yet scientific form, has been so scantily and unequally distributed, that in this direction a comparatively new field may be said to be open to the teacher as well as to the youthful inquirer.

"Influenced, also, by the great commercial value of several species of the pinnata, I have felt anxiously desirous to direct, without further delay, the attention, and thus possibly secure the sympathy, of readers, other than students, to the necessity of prompt legislative interference, in order to protect the oil and fur producing animals of our hemisphere against the wanton and unseasonable acts committed by unrestrained traders; and thus not only to prevent the inevitable extermination of this valuable group, but to utilise their eminently beneficial qualities into a methodical and profitable industry.

"Keeping steadily in view these two objects, whose importance, I trust, will bear me out in deviating from my original intention in the order of the issue of publication, I have endeavoured .... by devoting as much space as my limits would permit to the consideration of the animals whose products are of such commercial value to man, and whose extinction would so seriously affect his interests, to point out the pressing necessity that exists for devising the means of protection for the Fur Seals and the Sperm and Right Whales of the Southern Ocean.

"To evidence what great results may be effected by considerate forethought, I refer the reader to pages 8 to 13 of this treatise [containing extracts from the excellent paper on *Otariidæ* by Messrs. Allen and Bryant (Bulletin Harvard College, ii. pp. 1–108)], where he will see that, under the fostering care of the United States Government, the Northern Fur Seals of commerce, which but a few years ago were nearly extinct, have already, by their rapid increase and mild disposition, developed themselves into a permanent source of national wealth.

"The islands of the Southern Seas, now lying barren and waste, are not only numerous, but admirably suited for the production and management of these valuable animals, and need only the simple regulations enforced by the American Legislature to resuscitate the present state of decay of a once remunerative trade, and to bring into full vigour another important export to the many we already possess."

Mr. Scott's design appears to me eminently praiseworthy; and the question it raises is, without doubt, one which must imperatively demand (and will, I trust, in time) the attention not only of the naturalist, but of everyone who is interested in the commercial prosperity of this country and its colonies. Though to some extent their place has been supplied by mineral and vegetable oils, for certain purposes it is, I believe, admitted that animal oils are absolutely required, and the demand for these oils increases with the increase of civilization. Now no one who has at all closely investigated the subject of the extermination of animals by man can come to any other conclusion than that unless, by some legislative restriction (which from the nature of the case will probably have to be *international*) it is prevented, all the Marine Mammalia are inevitably doomed to early extinction. Who can read of the butcheries which are yearly perpetrated on the breeding Seals of the ice-floes in the North Atlantic, and as yearly recorded with more or less zest in the newspapers, without feeling certain that the same fate awaits them as has overtaken, or is overtaking, so many of their fellow-denizens of the deep? Where is the *Rhytina* of Behring's Island? Absolutely abolished from the face of the earth! Where are the Manatees that played in the waters of the Antilles, when those "isles of the sun" were first visited by Europeans? Limited to some three or four muddy creeks in as many of the larger islands! Where is the Right Whale that used to throng the Greenland seas, the Walrus that haunted the Gulf of St. Lawrence? Driven so far to the northward that ships in the pursuit of either are now led to encounter the greatest perils! Where is that smaller Whale which furnished employment for all the navies of Biscayan ports? You have to seek its remains in the museum at

Copenhagen! Where are the Dugongs of Rodriguez so charmingly described by Leguat? Vanished! Where are the Sea-elephants of Ascension, Tristran d'Acunha and the Crozettes? So hunted down that it is not worth a skipper's while to seek them! Where are the countless and mighty Otaries that Péron found in Bass's Straits? Not there assuredly![*] The list of questions might be extended indefinitely. Surely it is time to stop such wanton, such short-sighted destruction. Let me not be misunderstood, however. No one believes more firmly than I do in the right which man has to turn animals to his *use*. It is the *abuse* of which I complain. It is an abuse of power to slaughter these creatures in such places and at such times of the year as must lead to their utter extinction; and I know there are many naturalists, some of high standing, who think with me, though perhaps their acquaintance with the facts has not been sufficient to make them see so clearly as I do that interference with the abuse must speedily be adopted or it will be too late. Naturalists, as a rule, are rare in the legislature of this country, but is there not one, at least, to call upon the Government to take the necessary steps? Granted that these steps are beset with difficulties—so much the more honour to him who surmounts them. The Russians and the Americans have been before us, and through their wise measures there is now every chance that the Seals of the Northern Pacific will continue to exist for many a long year to the great profit of all concerned.

In this matter, as in similar cases, the present generation will deservedly be reproached by posterity if we steadily shut our eyes to what has taken place and to what is going on now. ALFRED NEWTON

NOTES

SIGNOR SCHIAPARELLI, Director of the Milan Observatory, has been appointed Director of the Florence Observatory in place of the late Signor Donati. The Florentine Observatory, which stands near Galileo's Tower at Arcetri, is in every way superior to that of Milan, and we may look for considerable results from an astronomer who has already done much with small opportunities.

ON Monday evening Sir Samuel Baker met with an enthusiastic reception at the meeting of the Geographical Society, from a large, distinguished, and brilliant audience, which included the Prince of Wales and the Duke of Edinburgh. Sir Samuel spoke mainly of what he had done to suppress the slave-trade, and of the almost overwhelming difficulties he and his brave wife had to face in bringing the lawless African tribes to reason. After Sir Samuel sat down, the Prince of Wales said a few words, and testified to the sincerity of the Khedive.

IN his address at the opening meeting of the Newcastle-on-Tyne Chemical Society, the president, Dr. Lunge, spoke of his visit to the Vienna Exhibition, and of the rapid progress which the Continent is making in the manufacture of the finer chemicals. The reason, he says, is not far to seek. "You find in every chemical works on the Continent, I may say, without exception, one, sometimes several, chemists of thoroughly scientific training, who have acquired their theoretical basis by three or four years' studying at a University or a Polytechnical Institution. One "works," to which I have already alluded, certainly one of the largest in Germany, keeps something like half-a-dozen such chemists (not practical managers), with salaries varying from 300*l*. to 400*l*., and it retains the services of an accomplished chemist, of scientific reputation, at a salary of nearly 2,000*l*. per annum, exclusively for theoretical work in the laboratory, without any trouble or responsibility connected with the manufacturing work outside. But then, they *do* constantly invent new things there, and make them in tons, or hundreds of tons, when the chemical world outside has, perhaps, barely heard of the discovery of a new compound, with a barbarous name, apparently only obtainable at the rate of a few grains in a sealed tube after many weeks' patient work. What I maintain, after a visit to the Vienna Exhibition, and at a few German and Austrian chemical works, is, that foreign countries are taking the wind out of our sails very fast in this line, and that both their rate of progress and the means of attaining it are very much superior to ours."

A PRELIMINARY meeting was held on November 29 in the Physical Laboratory of the Science Schools, South Kensington, to consider the formation of a Physical Society. The chair was taken by Dr. J. H. Gladstone, F.R.S. Thirty-six gentlemen were present, including most of the physicists of London. It was resolved that the following gentlemen be requested to serve as an organising committee:—W. G. Adams, E. Atkinson, W. Crookes, A. Dupré, G. C. Foster, J. H. Gladstone, T. M. Goodeve, F. Guthrie, O. Henrici, B. Loewy, Dr. Mills, A. W. Reinold, and H. Sprengel. A letter was read from the Lords of the Committee of Council on Education, granting the use of the Physical Laboratory and apparatus at the Science Schools, South Kensington, for the purposes of the Society.

THE *Photographic News* says that a curious and important discovery has been made by Dr. Vogel during the last few weeks. It consists, as he describes it in a private letter, in making the non-actinic rays under certain circumstances actinic. "I have found," he says, "that bodies which absorb the yellow ray of the spectrum make bromide of silver sensitive to the yellow rays. In like manner I find bodies which absorb the red ray of the spectrum make bromide of silver sensitive to the red rays. For example, by the addition of *corallin*—which absorbs the yellow ray—to a bromide of silver film it becomes as sensitive to the yellow ray as to the blue ray." This is one of the most important and interesting observations in connection with actino-chemistry which has been made for several years.

AN examination will be held at Queen's College, Oxford, on April 14, 1874, and following days, for the purpose of filling up four open scholarships of the yearly value of 90*l*. tenable for five years. Candidates must not have attained the age of 20 years. One of the open scholarships will be awarded for mathematics and one for natural science in case competent candidates offer themselves. Candidates offering in natural science should be proficient in either physics, chemistry, or physiology, and possess some acquaintance with a second physical science. These candidates are requested to signify by letter to the Provost, as early as may be in March, their intention of standing, and to state at the same time the subjects they propose to offer, in order that the necessary arrangements may be made for their examination. Candidates are to call on the Provost in the College-hall at 9 P.M. on Monday, April 13, bringing with them satisfactory evidence of date and (where necessary) place of birth, and testimonials of good conduct from their schoolmasters or tutors.

THE following alterations have been made in the programme of lectures at the Royal Institution:—In consequence of Prof. Tyndall's desire to give six lectures on the Physical Properties of Gases and Liquids on Tuesdays before Easter, Prof. Rutherford will give five lectures on Respiration before Easter, and six lectures on the Nervous System after Easter. At the Friday evening meeting, March 6, Sir Samuel Baker will lecture on the Suppression of the Slave Trade of the White Nile. Dr. Burdon-Sanderson will lecture after Easter.

MR. HENRY LEE reports the development of a new calcareous sponge in the Brighton Aquarium. In its early condition it closely resembles, in its mode of growth, *Leucosolenia botryoides*, but afterwards, in some instances, becomes massive and semi-globose. It has been submitted for examination to Dr. Bowerbank, who describes it as follows:—"In the young state, a congeries of thin fistulæ, like a *Leucosolenia*; when adult massive; furnished with numerous thin conical or cylindrical cloacal organs, very variable in size and length. Surface of the mass smooth and even; small cloacæ furnished with numerous long, slender, acerate, external defensive spicula, projected ascendingly at small angles to the surface; large cloacæ nearly destitute of external defensive spicula, furnished with a few long, slender, acerate, procumbent spicula; internal defensive spicula of cloacæ spiculated, equi-angular, tri-radiate; spicular ray, slender and attenuated. Oscula minute, distributed on the inner surfaces of the cloacæ. Pores unknown. Dermal membrane pellucid aspiculous. Skeleton spicula, equi-angulated and rectangulated, tri-radiate; radii slender and unequal in length, distorted; colour, cream white. Habitat, Brighton Aquarium, Henry Lee. Examined in the dried state." This sponge will be figured in three several conditions of its development, in the forthcoming third volume of Dr. Bowerbank's valuable monograph of the British *Spongiadæ*, published by the Ray Society, and will be known as *Leuconia Somesii*; Dr. Bowerbank having named it after Mr. Somes, the chairman of the Brighton Aquarium Company.

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[*] See Mr. Charles Gould's remarks in the Monthly Notices, &c., of the Royal Society of Tasmania for 1871, pp. 61—67.

A correspondent of the *Scotsman* points out how desirable a thing it is that a marine aquarium should be erected in Edinburgh. "The city," he rightly says, "abounds in educational establishments, to which such an institution would be an invaluable accessory. Great local facilities exist for the creation of an aquarium, and were a scheme for that purpose but set on foot, many willing hands would aid in its realisation. The cost would not be great, considering the advantages to be obtained; and it is certain the establishment would be self-supporting." We hope to see the matter earnestly taken up by proper hands.

The fifth part of the illustrated work on Lepidoptera, domestic and foreign, by Mr. Herman Strecker, of Reading, Pennsylvania, has made its appearance. In the present part the illustrations relate entirely to the genus *Catocala*, of which one supposed new species is presented under the name of *C. perplexa*, from the vicinity of Brooklyn. Mr. Strecker merits particular commendation from the fact that this work is prepared exclusively by his own hand, the illustrations being drawn on stone, printed, and coloured by himself—and, if we mistake not, the type of the text is set up by him likewise—all done in the intervals of his daily labour as a mechanic. The expense of the work—fifty cents per number—is such a mere trifle that we trust he will be encouraged by a sufficient subscription list to continue it to completion, increasing the number of plates, as he promises to do, without any change in the price, should he receive the desired patronage.

The London Association of Correctors of the Press held a *conversazione* on Saturday last under the presidency of Mr. B. H. Cowper, editor of the *Queen*. We are glad to notice that the principal items of the programme were of a scientific character. Mr. E. R. Johnson, Chairman of the Association, read a paper on the past work of the Association, enumerating some of the papers and discussions on philological topics which had engaged its attention, and while commending the study of philology, the advantage of an acquaintance with one or other of the exact sciences was set forth. Mr. G. Chaloner, late Secretary of the Association, and lecturer on Chemistry at the Birkbeck Institution, enlightened the meeting as to some of the properties of hydrogen, accompanying his remarks with appropriate experiments. Mr. J. T. Young discoursed on the glacial period, and exhibited some fossils illustrative thereof. The wonders of the microscope and stereoscope also contributed to the enjoyment of the evening.

The two scientific papers in the last number of the *Quarterly Journal of the Meteorological Society* are:—"On some Results of Temperature Observations at Durham," by Mr. J. J. Plummer; and "Notes on the Connection between Colliery Explosions and Weather in the year 1871," by Messrs. R. H. Scott, F.R.S., and W. Galloway. The subject of the latter article is of the greatest importance to miners, and, in connection with it, we would call attention to a letter in yesterday's *Times* warning colliery managers of the present high reading of the barometer. We are glad to see from the Report of the Council that the Society has attained an exceedingly prosperous and altogether satisfactory condition.

No. XI. of Petermann's *Mittheilungen*, contains a brief letter from Dr. Richard v. Drasche, concerning his geological voyage to Spitzbergen in July and August last. The letter contains a few very valuable details as to the physical and geological characteristics of the west coast of the island.

Sir George Rose, F.R.S., died at Brighton on the 3rd inst. in the 92nd year of his age.

Dr. Speier, of Fulda, has been appointed by the Japanese Government as Professor of Natural Sciences at Yeddo. A very handsome salary has been guaranteed to him by the Japanese Embassy at Berlin. Other appointments are expected to follow in the departments of Experimental Physics and Medicine.

Apropos of the letter in this week's number on the British Museum, we take the following from an article in a recent number of *Iron* on "Our National Museums:"—As at present constituted, Museums may be broadly divided into three types: first, that of the South Kensington, Jermyn Street, and Bethnal Green Museums in London, and the Albert Museum in Exeter,—a type of the actually useful museum, where the artisan may see illustrations of manufacturing operations, and the artist may find examples of the masterpieces of old. Here everything is neat, orderly, and simple; no object is without a label, which explains clearly what it is; and spectators need not wander about among collections of incomprehensible curiosities, which excite in their minds wonder but no interest. The second type is that of the British Museum, which is purely scientific. Museums like this are scattered over the country, containing vast numbers of useful specimens buried in drawers and cases, adorned with Latin labels; museums wherein the populace rove about with awe, partly at the monstrous objects displayed to their gaze and partly at the tremendous names which they bear. These museums are only fitted for scientific persons; they are next to useless to others, unless, as has been lately done in the British and Ipswich Museums, superintendents and curators are willing to descend from their high level and escort bodies of the simpler folk through the collections, giving as they go some plain account of the more prominent objects. A third type of museum is scarcely to be found in any national collection. It is usually seen in small country towns, where dusty cases are arranged in ill-lighted rooms, and are made the receptacle of rubbish brought by resident gentlemen from all parts of the world—one giving a collection of minerals for which he has not room; another a few drawers of butterflies of which he has grown tired. South-Sea Islanders' weapons, elephants' tusks, and other spoils of the chase are scattered about in corners and on walls, and the collection of oddments is dubbed a "museum." Our readers can draw on their own experience for other details on this subject, and we are much mistaken if they do not agree with us that the energy that is expended with but little useful result on our local and national museums is almost or entirely thrown away.

THE little town of Massa Marittima (Tuscany), says the *Journal of the Society of Arts*, sets an example which would be well to be followed by many larger and better known towns, both in Italy and this country. In 1867 the municipality of Massa purchased the interesting collection of minerals, models of mining machinery, and specimens of tools used in mines in various countries from Signor Teodoro Haupt, a well-known mining engineer of Florence, together with a complete series of maps and plans of most of the mines in Tuscany. This forms the nucleus of the museum, which has since been enriched by a collection of the birds and animals found in the province, the donation of a medical man residing in the town, and their value is considerably enhanced by being well arranged and tabled with both common and scientific names. The library now contains about 6,000 volumes, some of which are of great value, as being extremely rare, and relating to the history of the republic of which Massa was once the capital. The archæological department contains a very beautiful Etruscan funeral urn.

THE additions to the Zoological Society's Gardens during the past week include four Bull Frogs (*Rana mugiens*) from Nova Scotia, presented by Dr. B. Sanderson, F.R.S.; two white-handed Gibbons (*Hylobates lar*) from the Malay Peninsula, presented by Sir H. Ord, C.B.; two Griffin Vultures (*Gyps fulvus*) and a Golden Eagle (*Aquila chrysaetos*), European, presented by Mr. A. J. White; two Rough-legged Buzzards (*Archibuteo lagopus*), European, presented by Mr. A. B. Hepburn; a Green Monkey (*Cercopithecus callitrichus*) from India; and a Bonnet Monkey (*Macacus radiatus*) from India, presented by Miss Bradshaw; a Barasingha Deer (*Cervus duvaucelii*) from the Himalayas, received in exchange; and a Hairy Armadillo (*Dasypus villosus*) from La Plata, deposited.

SCIENTIFIC SERIALS

Der Naturforscher, Oct. 1873.—Among the abstracted matter in this number we find an account of recent experiments by M. Exner, to determine the "reaction time" of the sensorium. Some part of the body having been stimulated, the person immediately made a signal by pressing a key with the right hand. Marks were produced on a blackened cylinder, both at stimulation and at signalling, and the interval was noted. The reaction time (which ranged between 0·1295 and 0·3576 sec. in 7 persons) seems independent of age, and is shortest in those who have the habit of concentration. The tables also show it to have been shortest in stimulation of the eye with an induction shock; then follow, in order, electric shock to finger of left hand, sudden sound, electric shock to forehead, shock to right-hand finger, sight of an electric spark; and lastly, shock to toes of left foot. M. Exner analyses the reaction time into 7 "moments."—In chemistry we have some important observations on the non-luminous flame of the Bunsen burner, by M. Blochmann; and on vinegar-ferment and its cause, by MM. Mayer and Knierim, who think the action of mycoderma aceti probably physiological, and that it is a kind of bacterium which shows a mobile and an immobile state; the latter producing rapid acetification. Further, the vinegar-production occurs without the presence of nitrogenous substances, though less slowly than where they are present.—An interesting question in plant-geography is that as to the transport of seeds by ocean-currents, and in other ways independent of human agency. M. Thuret has been experimenting on this in Antibes. Having tried 251 different species, he knows of only two kinds of bare seed which are capable of floating, Maurandia and Phormium. A long immersion in sea-water does not always destroy the vitality of seeds. Out of 24 species immersed more than a year, at least 3 germinated afterwards as vigorously as seeds kept quite dry.—We find astronomical notes on the spectra of the two new comets, III. and IV., of 1873, and on the connection of solar protuberances with auroras (Tacchini); and in meteorology there is a notice of Dr. Koppen's valuable researches on an eleven years' period of temperature.—In physics, the subjects are: short galvanic currents and electrical discharges (Edlund), armatures of magnetic bundles (Jamm), and molecular rotatory power of vinous acid and its salts (Landolt).—A review of Häckel's *Die Kalkschwämme*, by M. v. Martens, is worthy of notice.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, Dec. 2.—Dr. A. Gunther, F.R.S. vice-president, in the chair. A communication was read from Dr. James Hector, containing an account of the complete skeleton of *Cnemiornis calcitrans*, Owen, and showing its affinity to the Natatores.—Prof. Owen, F.R.S., read a paper containing a restoration of the skeleton of *Cnemiornis calcitrans*, Owen, with remarks on its affinities to the Lamellirostral group, and forming the twentieth part of his series of memoirs of extinct birds of the genus *Dinornis* and its allies.—A communication was read from Mr. W. H. Hudson, containing an account of the habits of the Pipit (*Anthus correndera*) of the Argentine Republic.—A communication was read from Mr. A. G. Butler, containing a revision of the species of the genus *Protogonius*.—A communication was read from Dr. J. E. Gray, F.R.S., on the skulls of some seals from Japan, with description of a new species, proposed to be called *Eumetopias elongata*.—Mr. P. L. Sclater read a paper on some birds collected in Peru by Mr. H. Whitely, being the seventh of the series of articles upon this subject.—A communication was read from Mr. Henry Whitely, containing additional notes on humming-birds collected in High Peru.—A communication was read from Mr. R. Swinhoe, containing remarks on the Black Albatross with flesh-coloured bill, of the China Seas.—Mr. Garrod read a paper on the visceral anatomy of the Ground Rat (*Aulacodus swinderianus*).

Linnean Society, Dec. 4.—Mr. G. Bentham, president, in the chair.—Revision of the genera and species of Tulipeæ, by Mr. J. G. Baker. In this tribe of Liliaceæ the author includes the caulescent capsular genera with distinct perianth-segments and leafy stems bulbous at the base, viz., Fritillaria, Tulipa, Lilium, Calochortus, Erythronium, and Lloydia. The characters presented by the different orders were described *seriatim* in the paper. In the structure of the underground stems there are four leading types, viz., (1) a squamose perennial bulb, consisting, when mature, of a large number of thin flat scales tightly pressed against one another, and arranged spirally round a central axis which is not produced either vertically or horizontally, as exemplified in all the Old-world species of Lilium; (2) in most of the species of *Fritillaria* we have a pair only of hemispherical scales, half as thick as broad, pressed against the base of the flower-stem, these scales being the bases of single leaves which die down before the flower-stem is produced; (3) an annual laminated truncated bulb occurs generally in *Tulipa*, *Calochortus*, and *Eu-Lloydia*; (4) in the section *Gageopsis* of *Lloydia* we have a truncated corm. The leaves are very uniform throughout the tribe, with the exception of a section of *Lilium*, *Cardiocrinum*, with long clasping petioles. The perianth leaves are all coloured except in *Calochortus*, when the three outer segments are sepaloid and lengthened into points. The stamens are always six in number and nearly equal in length, hypogynous, and the dehiscence of the anther never properly introrse, but lateral, exactly as in *Colchicum*. In the capsule, *Calochortus* differs from the other genera in its septicidal dehiscence. As regards the connection between Liliaceæ and Colchicaceæ Mr. Baker is disposed to lay less stress than before on the evidence of any sharp line of demarcation between the orders, all the characters usually ascribed to the latter order being found in some of the genera of Liliaceæ. In its Geographical Distribution the tribe is spread throughout the north temperate zone; only one species, *Lloydia serotina*, is really boreal and Alpine; the southern limits are Mexico, the Philippines, South China, the Neilgherries, and the southern borders of the Mediterranean; the principal concentration of species is in California and Japan; nearly all are hardy in this climate. Lilium with 46, and Fritillaria with 55 species, have the distribution of the tribe; the latter stopping eastwards at the Rocky Mountains, while the former reaches the Atlantic sea-board; Tulipa, with 48 species, is restricted to the Old World, reaching from Spain, Britain, and Scandinavia to Japan and the Himalayas; Calochortus, with 21 species, is confined to Mexico and the west side of the Rocky Mountains; of the 5 species of Erythronium, 1 is confined to the Old World and 4 to the New; the 3 species of Gageopsis are Oriental and Siberian; while *Lloydia serotina* is the most widely spread of all the Liliaceæ, and a unique instance of a petaloid Monocotyledon of the North Temperate Zone with almost universal high mountains and Arctic distribution.

Chemical Society, Dec. 4.—Dr. Frankland, F.R.S., vice-president, in the chair.—A paper entitled Mineralogical Notices, by Prof. Story-Maskelyne and Dr. W. Flight, was read by the former, treating of the composition of caledonite and lanarkite.—Mr. John Williams then exhibited some fine specimens of crystallised phosphorous acid and metallic phosphites, and gave a short account of their reactions.—Prof. Church made a communication to the society on the composition of the mineral autunite.—Prof. Lawrence Smith of the United States, whilst describing a modification of the Bunsen gas burner employed by him for heating the crucible in determinations of the alkalis in silicious minerals, gave a short sketch of the process he had devised for that purpose.—In the course of the evening a gas burner by Mr. Fletcher of Warrington was also exhibited.

Royal Microscopical Society, Dec. 3.—Chas. Brooke, F.R.S., president, in the chair.—The list of donations to the society included a valuable binocular microscope with apparatus complete, from Mr. Charles Woodward, for which the special thanks of the meeting were returned.—A paper in continuance of the one read at the November meeting, was read by the secretary.—On some further researches into the life history of the Monads, by Rev. W. H. Dallinger and Dr. Drysdale, in which the complete process of fission was described in all its stages, and also the conjunction of two or more bodies, the whole course of internal division, of final rupture of the containing envelope and escape of minute free organisms.—Mr. Charles Stewart exhibited a section of *Ficus elastica* showing cystoliths, described the method of preparation and mounting, and stated it to be his belief that they were rather deposits of a gumlike substance, than actual concretions.

Society of Biblical Archæology, Dec. 2.—Dr. Birch, F.S.A., president. The following papers were read:—Future Punishment of the Wicked, a Doctrine of the Assyrian Religion, by H. Fox Talbot, F.R.S.—Notes from Borneo, illustrative of Passages in Genesis, by A. M. Cameron. In this paper the author cited a Dyak tradition, that at an archaic general inundation, the ancestors of the Chinese, Malay, and Dyak had to swim for their lives; and (possibly foisted on this tradition) the Dyak preserved his weapons, and the Chinaman his books. A second tradition stated that an ancestral Dyak made a ladder to go up to heaven; unhappily one night a worm ate into the foot of the ladder and brought all down. Mr. Cameron further stated that one of the two Dyak names for the Supreme Being is Yaouah: the author refers to the similar sounding Jehovah and Yahveh of the Bible.

PHILADELPHIA

Academy of Natural Sciences, June 17.—The president, Dr Ruschenberger, in the chair.—*Laws of Sex in Juglans nigra.*—Mr. Thomas Meehan said he had at various times during the past few years called the attention of the Academy to specimens of numerous plants which illustrated the principle that sex in plants was the result of grades of vitality; or, as it had been suggested, viability; and that this power of life was a mere matter of nutrition; the highest grades of vitality only producing the female sex. He now exhibited specimens of the common black walnut, *Juglans nigra*, which furnished excellent illustrations of what had been said on other occasions. Examining the tree at the flowering season, it would be plainly seen, by even a superficial observer, that there were grades of growing buds. The largest buds made the most vigorous shoots. These seemed to be wholly devoted to the increase of the woody system of the tree. Lower down the strong last year shoots, were buds not quite so large. These made shoots less vigorous than the other class, and bore the female flowers on their apices. Below these were numerous small weak buds, which either did not push into growth at all, or when they did bore simply the male catkins. He was fully satisfied that there is not so great expenditure of vital force on the production of male flowers as there is in female flowers.

PARIS

Academy of Sciences, Nov. 24.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the development of polyps and their corals, by M. H. de Lacaze-Duthiers. The author described some results obtained by him in a cruise on board the *Narval*, off the North African coast of the Mediterranean during the summer.—Remarks on the South American fauna, with anatomical details of some of its most characteristic types, by M. P. Gervais.—Observations on the expansion of water below 4°, in relation to M. Pierron de Mondesir's note, by M. F. Hement. The author suggests that the phenomenon in question is due to a re-arrangement of the molecules of the water just as a box of pins when shaken up will occupy more room than they did when arranged in regular layers.—A long extract from a letter by M. A. Poey was read relating to his observations of the relation between solar spots and terrestrial hurricanes. The author stated that during the last 125 years there have been 12 maximum periods of hurricanes and 10 of these correspond to sun-spot maxima and 11 periods of hurricane minima, of which 5 correspond to sun-spot minima.—Observations on the analogies which exist between the solar spots and terrestrial cyclones, by M. Marié Davy.—Note on solar and terrestrial cyclones, by M. H. de Parville.—On the discharge of electrified conductors, by M. J. Moutier.—On the variable state of electric currents, by M. P. Blaserna, an answer to M. Cazin.—Application of the phosphates of ammonium and barium to the purification of saccharine products, by M. P. Lagrange.—On the physiological and therapeutic action of hydrochlorate of amylamine, by M. Dujardin-Beaumetz. During the meeting Dr. A. W. Williamson and M. Zinin were elected Correspondents.

December 1.—M. de Quatrefages, president, in the chair.—On solar and terrestrial whirlwinds, by M. Faye. The author argued against Reye's ascending axes in the cases of these cyclones, and urged that both by theory and observation there is a down-rush about the axis.—On the conclusion of the note, General Morin made some remarks on the small eddies observed in rivers as examples of the descending current in the centre of similar vortexes.—On the directions of the vibrations in the rays refracted in uniaxial crystals, by M. Abria.—Analytical and experimental investigations of the interference of elliptical rays, by M. Croullebois.—On the return of carrier pigeons during the siege of Paris, by M. W. de Fonvielle.—On the habits of the *Phylloxera*, by M. Max. Cornu.—On a theorem of celestial mechanics, by M. F. Siacci.—Note on magnetism, by M. A. Trève.—On the difference of physiological action caused by induced currents from coils formed of different metals, by M. Onimus. The author stated that, with a coil made of a badly conducting metal the contraction of the muscles was greater and the effect on the cutaneous nerves smaller than when the coil is made of a good conductor.—On the conjunctive elements of the spinal marrow, by M. L. Ranvier.—On the *Anthracotherium*, discovered at Saint Menoux by M. Bertrand, by M. Gaudry.—On the secretions of the flowers of *Eucalyptus globulus*, by M. Gimbert.

BOOKS RECEIVED

English.—The Pearl of the Antilles: Walter Goodman (H. S. King and Co.).—The Internal Parasites of our Domesticated Animals: Dr. Spencer Cobbold (*Field* Office).—A Phrenologist among the Todas: Col. Marshall (Longmans).—The Bible and the Doctrine of Evolution: W. Woods Smyth (H. K. Lewis).—The Threshold of the Unknown Region: Clements R. Markham (Sampson Low).—Easy Introduction to Chemistry: Arthur Rigg (Rivingtons).—Chemistianity: J. C. Sellars (Author).—The Romance of Peasant Life: Francis George Heath (Cassell).—Cholera, how to Avoid and Treat it: Henry Blanc, M.D. (H. S. King & Co.).—Centrifugal Force and Gravitation, Supplement B: John Harris (Trubner & Co.).—Kant's History of Ethics. Translated by T. K. Kingsmill (Longmans).—Physical Geography in its relation to the Prevailing Winds and Currents: J. K. Laughton (J. D. Potter).—A Treatise on Medical Electricity: Dr Althaus (Longmans).—Weather Folk-Lore: Rev. C. Swainson (Blackwood).—Ganot's Physics. Translated by Atkinson. 6th edition (Longmans).—Waste Products and Undeveloped Substances: P. L. Simmonds (Hardwicke).—Man and Apes: St. George Mivart (Hardwicke).—Body and Mind: Alex Bain (H. S. King & Co.).—Metamorphoses of Insects: Sir John Lubbock (Macmillan & Co.).

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THURSDAY, DECEMBER 18, 1873

THE TRANSIT OF VENUS EXPEDITIONS

SOME time ago we called attention to the admirable opportunity which would be afforded by the approaching Astronomical Expeditions for the observation of the Transit of Venus to investigate the Natural History of several little-known islands of the Pacific. The addition of one or two members to these expeditions could make but a comparatively trifling addition to the expense, and while the Astronomers were observing, the Naturalists would be busily employed in collecting. We are glad to be informed that at a recent meeting of the Council of the Royal Society it was determined to take action in this matter, and to advise the Government to attach a small staff of Naturalists to the two expeditions destined to observe the transit in the Island of Rodriguez and Kerguelen's Land. There can be little doubt, we presume, that the Government will readily accede to the advice thus offered to them.

Rodriguez, an outlier of the Mascarene group, is in many ways specially worthy of thorough investigation. As a general rule oceanic islands lying at a distance from the great continents are of volcanic origin. The Seychelles and the island of Rodriguez are almost the only known exceptions to this rule. Rodriguez, so far as the slight information we possess on the subject extends, is believed to be composed of granite overlaid by limestone and other recent rocks. It is, therefore, of great importance that an accurate geological examination should be made of this island, more especially as its nearest neighbours Bourbon and Mauritius follow the ordinary rule of being volcanic. A second rich field of biological research in Rodriguez will be found in the fossil remains to be met with in the caves of the limestone rocks. These have already yielded good fruit to the investigations of Mr. Edward Newton. the Colonial Secretary of Mauritius, aided by grants from the British Association. The complete skeleton of *Pezophaps solitaria*, a bird allied to the Dodo of the Mauritius—has been restored from these remains, as is well-known from the excellent memoir upon this extinct bird published by Mr. Newton and his brother, Prof. Newton of Cambridge, in the Philosophical Transactions of the Royal Society. But besides additional bones of the Solitaire, which will be welcome to many Museums, it will be desirable to become acquainted with the other animals which were the Solitaire's fellow-creatures when in existence. Some of these are also obscurely known through the exertions of the Messrs. Newton, but it cannot be doubted that ample materials of this kind are still lying hid in the caves of Rodriguez for the benefit of future explorers.

The recent Zoology and Botany of Rodriguez also merit thorough investigation in order to ascertain whether they show any parallel differences to that of its geological structure as compared with the rest of the Mascarene group of islands.

Kerguelen's Land, the second point selected for biological investigation, is also likely to give ample occupation for a naturalist who will be able to devote several months to its exploration, while the necessary preparations are being made for the observation of the great astronomical event. In 1840 Kerguelen's Land was visited by the Antarctic Expedition under Sir James Ross. The distinguished naturalist who accompanied the expedition ascertained that it contains a scanty land-flora of flowering plants, some of which belong to types elsewhere unknown, and an extraordinary profusion of marine forms of both the animal and vegetable kingdoms. Of the land-plants a good series was obtained, but as regards the marine flora and fauna much must remain to be done—especially as Sir James Ross's visit took place in mid-winter. The *Challenger* will visit Kerguelen's Land early next year in order to ascertain the best station for an Astronomical Observatory, and her excellent staff of naturalists will, without doubt, not neglect the opportunity thus given to them. But looking to the great size of the island, which measures nearly 100 miles by 50, and to what is reported of the excessive richness of the marine forms of life, there will certainly be ample occupation left for the naturalist whom it is proposed to send there along with the Transit expedition.

There is, in fact, no doubt that it would be difficult to find two spots on the earth's surface where investigation is more likely to lead to satisfactory results than in the case of these two little-known islands. Nor is the opportunity now offered of obtaining these results at a very small cost to the nation likely to recur, if now neglected. We trust, therefore, that on the part of the Government there will be no hesitation in acceding to the scheme put before them by the Council of the Royal Society.

ELLIS'S LIFE OF COUNT RUMFORD

Memoir of Sir Benjamin Thompson, Count Rumford, with Notices of his Daughter. By George E. Ellis. (Published in connection with an edition of Rumford's complete Works by the American Academy of Arts and Sciences. Boston.)

THIS biography supplies a want that has been sorely felt by all who have desired to obtain a reliable account of Count Rumford's eventful life. It is, I think, impossible to name any equally eminent man of modern times concerning whom so little was known before the publication of this work. The only preceding sources of information, Prof. Pictet's letters, Prof. Renwick's sketch in "Sparks's Library of American Biography" Cuvier's *Eloge* and the Cyclopædia biographies made up from these and each other, are most vexatiously contradictory on points of primary interest. Aided by Rumford's own correspondence, and other original and direct sources of information, Mr. Ellis's industry has at last rescued us from these perplexities.

The career of scientific notables is usually of a simple and uneventful character, but that of the poor schoolmaster of New Hampshire is sufficiently adventurous and romantic to supply materials for a sensation-novel writer.

He married early; to quote his own words—"I took a wife, or rather she took me, at 19 years of age." He describes his married life as both happy and profitable but it lasted scarcely three years, during which he became a prominent public man and a full-blown soldier, with the rank of major at 20 years of age. The part he took in connection with the American rebellion excited popular indignation against him, led to his imprisonment, the confiscation of his property, and his subsequent flight from home shortly after the birth of his daughter. He never saw his wife again, nor did he see his daughter until 20 years afterwards, when she rejoined him in Europe.

At the age of 23, he appears in a new character upon another scene. He is now a diplomatist, presenting his first state paper to Lord George Germaine in London. He steps at once into a responsible position in the Colonial Office, and presently becomes the "Secretary of Georgia." In the meantime he is doing important scientific work, is elected Fellow of the Royal Society in 1779, when 26 years of age, and suddenly appears on board the *Victory* as a volunteer sailor under Sir Charles Hardy, experimenting with ship's guns, and writing treatises on naval signals and naval architecture. In the following year he is promoted to the office of "Under Secretary of State for the Northern Department" (Colonial).

Thirteen months after this he re-appears in military uniform as Lieut.-Colonel Thompson commanding "The King's American Dragoons," and profoundly occupied with experiments upon light artillery, &c. Before 1781 is ended, we find him on the other side of the Atlantic with his dragoons on Long Island, and fighting in the neighbourhood of Charleston at the beginning of 1782. In April we hear of him in New York, and presently find that he has returned to England promoted to the rank of full colonel, and otherwise honoured for his American services.

In the midst of all this activity and excitement he is busily engaged in scientific research chiefly upon subjects connected with gunpowder, bullets, and artillery. With his characteristic exaltation of present pursuits he is now consumed by military ardour, and, dissatisfied with his late inglorious outpost skirmishing in America, obtains appointment for active service in the defence of Jamaica against the French, but is frustrated by the temporary pacific re-action that suddenly prevails. He offers to serve in India, but the Government has become economical. Determined to fight somebody, he selects the Turks, with whom Austria is temptingly disposed to quarrel, and, having obtained the King's permission, proceeds to Vienna, with war-horse, arms, and uniform. Halting on his way he creates considerable sensation by appearing as a visitor on the garrison parade at Strasburg, displaying his handsome figure, brilliant English uniform, and his skilful management of an English blood-horse. Field-Marshal Prince Maximilian de Deux Ponts rides up to the stranger, salutes, and asks a few questions. Thompson, with the polished courtesy and tact of which he is so accomplished a master, turns this introduction to good account, secures the friendship of the Prince, who is so strongly impressed with the varied attainments of the brilliant soldier, that he presses him to pass through Munich on his way to Vienna and visit the reigning Elector of Bavaria, an uncle of the Prince.

The visit is made most successfully, and, with additional introductions, Thompson proceeds to Vienna with a ready-made continental reputation, though only a few weeks old. Here, as he says, "I owe to a beneficent Divinity that I was cured in time of that martial folly." The agent or Divinity of this reformation, was a lady, who, as he says, "formed an attachment to me, gave me wise advices, and imparted a new turn to my ideas, by presenting me in perspective other species of glory than that of conquering battles." It is proper to add, in explanation, that the lady was seventy years of age.

In the meantime the Elector of Bavaria invites Thompson to enter his service. For an English officer to do this, permission from the king was necessary. This was obtained in London, and with it the honour of knighthood, which was conferred in February 1784, with a continuance of half-pay as colonel.

Sir Benjamin Thompson proceeds immediately to Munich, and there enters upon the most remarkable part of his extraordinary career. The task which he set before himself in Bavaria was nothing less than a complete reformation and re-organisation of the army, and a general improvement of the physical and social condition of the whole nation. Invested with full powers by the Elector he sets about his work in a strictly philosophical manner. The first four years—1784 to 1788—are devoted to a cool, impartial, and systematic investigation of the social statistics and general condition of all classes, civil and military, in Bavaria. Having thus inductively collected and generalised his data, he now proceeds deductively to devise his remedies for the evils thus demonstrated. In all his efforts, from the improvement of saucepan-lids and gridirons to the moral reformation of a whole nation of human beings, he is rigidly methodical and strictly scientific, and his success follows as a direct and visible consequence of this scientific mode of proceeding.

His well-known and important researches on the Convection and general Transmission of Heat were undertaken and carried out mainly for the purpose of determining the best and most economical means of clothing the Bavarian soldiers, and the construction, warming and ventilation of their barracks. Another equally important though less known series of researches were instituted for the purpose of learning how to feed in the most economical manner the beggars, rogues, and vagabonds, whose sustenance and reformation he had projected.

His success in reorganising both the men and materials of the army was marvellous. It was in the course of his work in erecting cannon foundries and remodelling the Bavarian artillery that his celebrated demonstration of the immateriality of Heat was suggested.

It may safely be affirmed that the foundation of the present military system and of the recent military successes of Germany was laid by Benjamin Thompson in Bavaria. He tells us that the fundamental principles upon which he proceeded were "to unite the interest of the soldier with the interests of civil society, and to render the military force, even in times of peace, subservient to the public good;" and further, "that to establish a respectable standing military force which should do the least possible harm to the population, morals, manufactures, and agriculture of the country, it was necessary *to make soldiers citizens, and citizens soldiers.*"

Besides the important technical reforms of discipline, arms, barracks, quarters, military instruction, &c., which he carried out, "schools were established in all the regiments, for the instruction of the soldiers in reading, writing, and arithmetic, and into these schools not only the soldiers and their children, but also the children of the neighbouring citizens and peasants were admitted gratis." Military schools of industry were also established where the soldiers learned useful trades; thus the military clothing was spun, woven, and made up by the soldiers themselves; roads and other public works were made and erected, and the men were permitted to hire themselves out in garrison towns. Besides this the soldiers were used as industrial missionaries for the introduction of improvements in agriculture, manufactures, &c. The potato, until then almost unknown in Bavaria, was thus introduced by the aid of Thompson's military gardens or model farms. One of these gardens still remains, viz. the well-known "English Garden" at Munich.

Still more remarkable was his success in radically curing the overwhelming curse of Bavaria, which was infested with hordes of beggars and vagabonds that had defied every previous effort of suppression or diminution Here again the same strictly philosophical method of proceeding was adopted. Human materials and motives were handled precisely as we manipulate the physical materials and forces of the laboratory, and the results were similarly definite, reliable, and successful. The scientific social reformer not only cleared the country of its rogues, vagabonds, and beggars, but made their industry pay all the expenses of their own feeding, housing, and clothing, besides those of the industrial and general education of themselves and their children. In addition to all this they made clothing for the military police who took them into custody, and earned a handsome net profit in hard cash.

It is not surprising that such success should have earned for him a long list of Bavarian honours and titles which need not be here recounted, and that he should now appear as "Count of the Holy Roman Empire and Order of the White Eagle," or, as better known to us, in the title of his own choice, "The Count of Rumford." Neither need we be surprised that his health should fail, and that in spite of repose and change of scene we next find him lying dangerously ill at Naples.

On his recovery he returns to England, and while busily engaged there in literary and scientific work, is suddenly recalled to Munich, which now has the Austrians at its gates, and is simultaneously threatened by the French. Matters become so serious that the Elector saves himself by flight, only eight days after Rumford's arrival; but before leaving the monarch hands over to the philosopher the command-in-chief of the army, and the practical dictatorship of the capital. During the three months of this supreme command Rumford succeeds in overawing and checkmating both French and Austrians, and saving the city, after which the Elector returns.

This is the climax of the great philosopher's career, and now we find him a second time stricken by dangerous illness. On recovering he returns to London, founds the Royal Institution, publishes his essays, and then leaves England for the last time to reside in Paris, where he marries the "Goddess of Reason," Madame Lavoisier.

Here the curtain falls upon all his greatness, for though but fifty-two years of age, the brilliant career of the Count of Rumford is ended, and the subsequent scenes of his life display a miserable contrast with all that preceded them.

His biographers are evidently puzzled by what follows, and painfully seek apologies for his matrimonial squabbles, his general irritability, his morose seclusion, and the small results of the fussy labours of the last ten years of his life. My own theory is that the illness at Munich—where he describes himself as being "sick in bed, worn out by intense application, and dying, as everybody thought, a martyr to the cause to which I had devoted myself"—was followed by chronic and permanent cerebral disease, and that the gradually developing change of character which he displayed from the date of his return to England in 1798, until his death in 1814, was but a natural symptom of this growing malady.

Present space does not permit me to state in detail the evidence upon which I base this conclusion, but I cannot conclude without protesting against the explanation of Cuvier, who in his *Eloge* states that "It would appear as if, while he had been rendering all these services to his fellow-men, he had no real love or regard for them. It would appear as if the vile passions which he had observed in the miserable objects which he had committed to his care, or those other passions, not less vile, which his success and fame had excited among his rivals, had embittered him towards human nature." Cuvier, if I am right, only knew the diseased wreck of the brilliant, courteous, and even fascinating "soldier, philosopher, and statesman," and I suspect that the unjust oblivion of his merits which so speedily followed his death, was largely due to the bad impression made, not only upon the French Academicians, but also upon his Royal Institution associates, by the moral obliquities and eccentricities due to a diseased brain.

The main interest of the career of this wonderful man appears to me to lie in this, that it affords a magnificent demonstration of the practical value of scientific training, and the methodical application of scientific processes to the business of life. I have long maintained that every father who is able and willing to qualify his son to attain a high degree of success either as a man of business, a soldier, a sailor, a lawyer, a statesman, or in any other responsible department of life, should primarily place him in a laboratory where he will not merely learn the elements of science, but be well trained in carrying out original physical research, such training being the best of all known means of affording that systematic discipline of the intellectual and moral powers upon which all practical success in life depends. The story of Count Rumford's life, and the lesson it teaches, afford most valuable evidence in support of this conclusion, and cannot fail powerfully to enforce it.

This subject is specially important at the present moment, particularly to those Englishmen whose minds are still infested with the shallow foolishness that leads them to believe that scientific men are dreamy theorists, and disqualified for practical business. Let them follow in detail the practical triumphs of this experimental philosopher, and ask themselves candidly whether such success could have been possible had he been trained in the mere word-exalting study of the Greek and Latin classics, instead of the practical school of experimental research.

W. Mattieu Williams

GARRETT'S FISHES OF THE PACIFIC

Andrew Garrett's Fische der Südsee beschrieben und redigirt. Von Albert C. L. G. Günther, Heft i. (Hamburg: L. Friederichsen & Co., 1873.)

THE house of Hr. Cesar Godeffroy & Co. of Hamburg have for several years employed scientific collectors in various parts of the Pacific to prepare and send home specimens of natural history. These have been stored up at Hamburg, in what is now a well-known scientific institution, the "Museum Godeffroy," under the care of an active superintendent, whose services have been engaged to take charge of and arrange the various objects thus accumulated. But not content with thus bringing the rarities of the Pacific within the grasp of European naturalists, Herr Godeffroy has obtained the assistance of some of the best known workers in Science for examination of these materials. The extensive collections of birds made for him by Dr. Edward Gräffe were submitted to the well-known ornithologists Drs. Finsch and Hartlaub of Bremen, and formed the basis of their excellent work on the "Birds of Central Polynesia," published a few years since. For the working out of the Polynesian Fishes, of which we believe, Herr Godeffroy's collection is still more complete, the co-operation of Dr. Günther of our National Museum, the most distinguished of living ichthyologists, has been obtained, and the book now before us contains the first-fruits of Dr. Gunther's labours.

The brilliant colours which adorn many of the Polynesian fishes have been well known to travellers in those regions since the days of Cook, and have been frequently described in lively terms. Unfortunately, however, these colours entirely disappear in fishes preserved in spirit after the ordinary fashion, so that their beauty can only be appreciated by visitors to the distant seas which they inhabit. In order to exhibit these colours in the present work, Herr Godeffroy has acquired a large series of drawings, taken from living specimens, by Mr. Andrew Garrett, who has been many years resident in the Sandwich and Society Islands, and in other parts of Polynesia. Under these circumstances we may well anticipate the production of a first-rate work, more especially as the services of the unrivalled lithographic artist, Mr. G. H. Ford, have been secured to put the drawings on the stones.

Dr. Günther commences his work in systematic order with the Serranidæ, of which numerous brightly coloured forms inhabit the various Archipelagoes of the Pacific. Twenty splendid plates illustrate the letterpress, and it is only wonderful how they can be produced at so small a cost. Nine similar parts will complete the work, which bids fair to become one of the most perfect icthyological monographs ever issued.

OUR BOOK SHELF

Manual of Comparative Anatomy and Physiology. By S. M. Bradley, F.R.C.S. Second Edition. (Manchester: Cornish; London: Simpkin, Marshall and Co.)

ENCOURAGED by the success of an earlier and much smaller edition of this work, the author has entirely re-written the new one. In so doing, we think that he could not have made a greater mistake, as the small size of the original precluded the introduction of detail with which he is not acquainted, and so prevented his exposing his ignorance to the world at large. The impression which remains after the perusal of a few pages is, that the author, after reading rapidly through some one of the standard text-books on Zoology, wrote down his impressions as far as his memory served him. Faults of omission are not uncommon in text-books, especially when they are written by those who are not practically acquainted with their subject, but faults of commission are, fortunately, much less common. In the work before us there are several of the former, and they cannot all be laid down to want of space; for in the case of the Myriapoda, respecting the peculiarities of the main divisions of which the position of the legs is not referred to, two-thirds of the page on which they should have been found is left blank before the commencement of the following chapter. The faults of commission are so numerous that they admit of easy classification. There are those of sheer carelessness from inattentive reading, otherwise, how is it that we are told that the Dugong has six cervical vertebræ, and that the *Tragulinæ*, or Musk Deer (!) have all the tarsal bones anchylosed. Others arise from a want of power to realise the meaning of the ordinary descriptions of well-known anatomical facts, as when it is indicated that the ventricles of the Crocodile's heart are not completely separated, and the marsupium, or pouch of the female Kangaroo in the male is everted, and supports the penis. Absolute and inexcusable errors it is difficult to explain, but among such we are told that the Nummulites are Cephalopoda; the Marsipobranchii have more than one nasal sac; that in the Lepidosiren the nasal canals are not open at both ends, and the vertebræ are ossified; and that in the Bear the clavicles are more developed than in other Carnivora, when they are in reality absent altogether. Peculiarities found in one division are omitted with regard to them, and referred to others entirely different, as when it is stated that among the Marsupialia "each oviduct in the female leads into a perfectly distinct uterus, which opens into a separate vagina, which is also the passage of the urine," and that in the male the vasa deferentia "open into a cloaca common to the urinary and generative secretions." These remarks apply to the Monotremata well enough, how is it they are omitted in speaking of them, and stated of their allies, which in these respects are quite differently constructed. We rarely remember to have seen a work so carelessly undertaken, and by so incompetent an author.

Seventeenth Half-Yearly-Report of the Marlborough College Natural History Society for the Half-Year ending Midsummer, 1873. (Marlborough: Perkins.)

ALTHOUGH the tone of the Preface to this Report is not quite so desponding as that of the previous one, still it contains a good deal of complaint. It seems to be the rule, for which we cannot see any reason, that members on entering the fifth form resign their membership. Is it because their schoolwork occupies all their time? or is it considered beneath the dignity of a fifth-form boy to belong to such a society? Probably no satisfactory reason could be assigned for the practice, therefore we hope it may not be continued. Another discouragement to the society has been the difficulty of getting papers except from a very few, who, after a time, "struck work," because they "felt that others ought to help in keeping up the interest of the meetings." We think the few workers would have been more likely to attain this end had they continued to prepare and read papers amid all discouragements; by this means, we think, they would be more likely "encourager les autres." We see no reason why the reading of papers should not be combined with the exhibition of objects and with discussions. Is not the Marlborough College Society too sensitive? From the reports of the field-work done and the collections made, it seems to possess a few admirable workers, who possess energy, knowledge, and earnestness enough to keep any such society from collapsing. The Botanical list is a model one. The papers in the Report are,—"Heraldry," by Mr. F. E. Hulme, F.L.S.; "On the Perception of the Unseen," by Mr. G. F. Rodwell; "A Walk across the Karst," by the Rev. J. Sowerby; and "The Luschari (Heilige) Berg in Carinthia," by the same gentleman.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Prof. Agassiz

THE sad intelligence received in London this morning of the death of Prof. Agassiz adds another illustrious name to the long roll of victims to the insidious demon, "over-work." May I ask you to give room in your next issue to the following passage from a letter (probably one of the last he penned) received from Prof. Agassiz only last week, which may be interesting to his many sorrowing friends on this side the Atlantic, as attesting indirectly to the cause of his death, viz., excess of mental and physical exertion.

P. DE M. GREY EGERTON

Athenæum Club, Dec. 16

"Museum of Comparative Zoology,
"Cambridge, Mass., Nov. 26, 1873

"A feeling of despondency comes over me when I see how long a time has elapsed since I received your last letter, which at the time I meant to answer immediately. With returning health, I have found the most frightful amount of neglected work to bring up to date, with the addition of a new institution to organise. I have given myself up to the task with all the energy of which I am capable, and have made a splendid success of the Anderson School, which cannot fail henceforth to have a powerful influence upon the progress of Science in the United States. But this has driven out everything else, and I should have neglected even the Museum had not a constant appeal to my attention arisen from the close connection in which the Anderson School stands to the Museum, of which it is, as it were, the educational branch. So School and Museum have made gigantic strides side by side; but I am down again. At least I feel unable to exert myself as usual, and such a feeling in the beginning of the working season is disheartening. When I last wrote I had strong hopes of an easy summer with my family, and confidently expected to be able to pass the greater part of the winter in Europe, and to have prepared the volume on Selachians of the 'Poissons Fossiles' for a new edition, or rather an English work on the subject. Now that hope is gone; the immense accessions to our Museum make even the progress of the Coal Fishes from Iowa slow and almost hopeless. With 22 assistants and 14 sub-assistants in the Museum, I have my hands full with administrative duties and responsibilities, and science and friends suffer.

"Ever truly your friend,
"(Signed) L. AGASSIZ"

Experiments on Frogs

WILL you grant me the space in your journal for a few words called forth by Mr. Lewes's letter in your number of December 4, on "Sensation in the Spinal Cord"?

In that letter the writer describes some experiments on frogs of such excessive cruelty that I cannot refrain from entering a protest against the principle which justifies such actions.

The right to perform such actions as vivisection, &c., in the cause of Science, has often before been questioned; but the present case—a case in which the infliction of pain is not an unavoidable *attendant* on the experiments, but the very essence or object of them, and the slowness and prolongation of agony a necessary part—stirs and revolts the whole mind, and brings the question again prominently to the front.

The question then is—are either the possible or probable benefits to a portion of mankind, or the advancement of Science for its own sake, sufficient reasons for the infliction of intense suffering on our fellow-animals? Of course much may be urged in favour of vivisection. It may be said that without its assistance Science, and especially the science of medicine, could never have advanced to the point it has now reached; and mankind urges that the good of mankind is of such paramount importance that that of all other animals must be subordinated to it unconditionally, and consequently that the smallest good to mankind balances the greatest evil to other animals.

To many this would be considered an amply sufficient reason for answering the question in the affirmative, but at least it should be remembered at what tremendous cost to one portion of creation these benefits to another portion are purchased.

As time and Science advance it is becoming more recognised that other animals have their rights as well as men; and perhaps it may some day be found that the right which mankind assumes to himself of supremacy over his fellow-animals (including the right to inflict deliberate torture, for whatever purpose) is, after all, but the right of the strongest or most powerful.

It seems to me so shocking that such things should be written of and read with indifference, and without evoking one word of protest on the other side, that on this ground alone, *i.e.*, that the assumption of the right to inflict torture may not pass quite unchallenged, I venture to beg for the insertion of this letter.

Dec. 8 X.

Proposed Alterations in the Medical Curriculum

IN a recent number of NATURE, remarks are made in regard to the present Medical Curriculum, more especially in connection with the proposal of Prof. Huxley to alter the Curriculum for medical graduation in the University of Aberdeen. His object is to remove the subjects of Botany and Natural History from that Curriculum, and to put them in the category of a preliminary examination, without any compulsory attendance upon lectures. Such a proposal if carried into effect would tend in no small degree to limit the medical student's acquirements in the biological sciences, as he will not be required to take full scientific courses on these subjects. The tendency of such a system will be to encourage what is commonly called "cram," inasmuch as there will be no guarantee for methodical practical instruction under a qualified teacher.

While it may be true that those who take the diplomas of the medical corporations are not called upon to attend courses of lectures on these subjects, and rarely undergo an examination on them, the case is quite different with those students who aspire to university degrees. The latter look not merely for a license to practise, but desire also a university honour. An important distinction at the present day, between the licentiates of colleges and the graduates of universities, is that the latter are expected to have a higher literary and scientific knowledge. In place of reducing the qualifications for degrees, so as to compete with colleges, we ought to keep up the standard, and send forth medical men who are not only well fitted for the practical duties of the profession, but who can also occupy a prominent position in the scientific world. In accomplishing this object we should arrange the curriculum in such a way as to put the study of the sciences in its proper place. The student ought to commence the study of botany and natural history in summer, before entering upon anatomy, surgery, and other purely medical subjects. This is now to a large extent carried out in the University of Edinburgh, and by so doing a three months' course of scientific study is added to the curriculum. The student might be encouraged to take his science examination at an early period of his curriculum, say at the end of his first year of study. The training which these studies give to the mind of the young medical student, is most important. They call forth his powers of observation and diagnosis; they present to him the principles of classification, and they enlarge his views of anatomy and physiology. In primary schools of the present day we frequently find that the elements of botany and zoology constitute a part of the teaching, and most properly so. But this is not enough for the graduate in medicine. He must supplement this by going through the higher University Curriculum.

The commissioners for visiting the Universities of Scotland, remark in their report "that it is desirable that graduates in medicine should have that degree of literary and scientific attainment which will prevent them when mingling as they must do with mankind, in the exercise of their profession, from being looked upon with contempt; or from committing errors in conversation and in writing, for which others would be despised; because even upon the supposition that they have high professional acquisitions, the law of association will operate, and the conclusion will be drawn that much confidence cannot be placed in them." The value of university training was strongly insisted on by the late Prof. Edward Forbes, when speaking of the relation which scientific studies bear to medicine. The following are his remarks:—"It is the training of the mind in correct methods of observation that gives the Natural History Sciences so much value as instruments for preparation in professional education. Not unfrequently do we hear the short-sighted and narrow-minded ask—what is the use of zoology or botany or geology to the physician and surgeon? what have they to do with beasts or plants or stones? Is not their work among men healing the sick? Of what use save as remedies, are the creeping things, or the grass that grows upon the earth, or the minerals in the rock? Vain and stupid questions all—yet they are sometimes put by persons who profess to promote the spread of education. They want something, but the best of them mistake the end for the means. The best want knowledge, but have not learnt that the mind must be trained ere it is prepared to gather and digest knowledge. They want science, but science turns mouldy and unwholesome in our unprepared mind. They forget or do not know that education consists chiefly in training, not in informing.

"We must counteract the natural tendency of purely professional studies—the tendency to limit the range of mental vision. We can do this most beneficially through the collateral sciences, which are sufficiently different to give them a wider sphere of action. It is from this point of view that we should regard the natural history sciences as branches of medical education. For my part," continues Forbes, "after much intercourse with medical men who had studied at many seats of professional education, some collegiate, some exclusively professional, I have no hesitation in saying that, as a rule, the former had the intellectual advantage. There are noble and notable exceptions old and young, but the rule is true in the main. The man who has studied at a seat of learning, university or college, has a wider range of sympathies, a more philosophical tone of mind and a higher estimate of the objects of intellectual ambition, than his fellow-practitioner who, from his youth upwards, has concentrated his thoughts upon the contractedly professional subjects of an hospital school. I will not believe that the practitioner of medicine, any more than the clergyman, or the lawyer, or the soldier or merchant, is wiser, or better able to treat the offices of his calling, because his mind takes no note of subjects beyond the range of his professional pursuit. It is a great pleasure, both to patient and neighbourhood, to find in our doctor an enlightened friend, one who, whilst he does his duty ably and kindly, has a sympathy and an acquaintance with science, literature, and art."

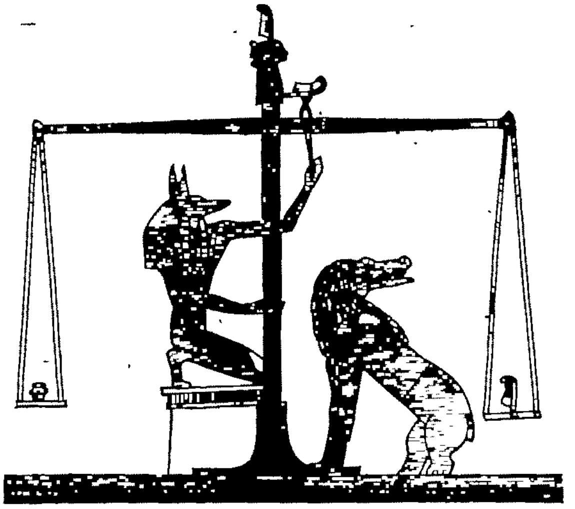
In Scotland a university is not merely a board authorised to examine students and grant degrees, it is an educational institution, intended to exercise a surveillance over the studies of youth, to train their minds for the proper acquisition of knowledge, and to direct their energies in such a way as to insure that mental culture which will fit them for all the duties of life. We speak of our University in Scotland as our *Alma Mater* because she acts the part of a mother to her *alumni*, educating them and superintending their progress in liberal studies.

It appears to me that a great injury would be inflicted on the character of our medical degrees if the required curriculum did not embrace the natural sciences To study these properly something more than books is required. There must be practical training under an able teacher, examination of living objects both with the naked eye and with the microscope, and a certified course of study. I am sure that everyone, in Scotland at all events, who desires to make graduation in medicine a University honour will aid in keeping up a scientific curriculum under qualified teachers.

Edinburgh University JOHN H. BALFOUR

Ancient Egyptian Balances

I HAVE to thank Mr. Rodwell for calling my attention, in NATURE, vol. ix. p. 8, to the curious representation of an equal-armed Egyptian balance in a papyrus, now in the British Museum. This papyrus, which is perhaps the most beautiful in the whole collection, all the colours and lines being as bright and distinct as when originally painted, has been shown to me by Dr. Birch, who also informed me where I could procure a photograph of it, being one of a series of photographs from the collection at the British Museum, taken by S. Thompson, and published by Mansell and Co., 2, Percy Street. By Mr. Mansell's permission the following drawing has been made.



From an ancient Egyptian papyrus in the British Museum, of Hennefer, superintendent of the cattle of Seti I., 19th Dynasty, about 1350 B.C., representing the "Ritual of the dead." The heart of the deceased is being weighed in an equal-armed balance, and found lighter than a feather. In the papyrus, the weighing is being made in the Hall of perfect Justice, in presence of Osyris.

It may be seen that what Mr. Rodwell mentions as a sliding weight on one side of the beam, appears rather to be a loop or ribbon for limiting the oscillation of the beam. In the original papyrus the middle and both ends of the beam, as well as the lower part of the column, are coloured to represent polished brass, whilst the other parts of the balance are dark, as if of bronze. It should be observed that the balance beam has box-ends for suspending the pans. Judging from the height of the human figures, the length of the balance beam represented is about six feet, and the height of the column of the balance is nearly the same. Several similar, though rougher, representations of weighing the heart of the deceased may be seen in the papyrus drawing on the staircase leading from the Egyptian sculpture room to the upper Egyptian room in the British Museum.

H. W. CHISHOLM

Stalagmitic Deposits

IN a former number of NATURE (vol. viii. p. 462), Mr. A. R. Wallace, in reviewing Sir Charles Lyell's last edition of the "Antiquity of Man," makes use of the rate of deposits of stalagmite as data for ascertaining the age of animal remains which are found buried in caves. It is evident that the variations of rate will render unreliable data for arriving at correct conclusions; still, calculations based thereon may be of service.

Some thirty years ago I procured a piece of lime deposit from a lead mine at Boltsburn, in the county of Durham; it measured about 18 in. in length, 10 in. in breadth, and fully ¼ in. thick; it was compact and crystalline, and showed distinct facets of crystals on its surface, over which the water was running. I had indisputable evidence that the deposit had taken place in fifteen years. The water, from which it was produced, issued from an adit driven in the Little limestone, which is about 9 ft. thick. After leaving the adit, the water ran down the perpendicular side of a rise, for some fathoms, on to some rock *débris*, which was lying on the bottom of a hopper, whence it proceeded from the upper part of the hopper mouth, then perpendicularly down over two narrowish wood deals, which were set on edge, and put across the mouth of the hopper to retain the worked materials. It was from off these deals that I obtained the specimen above described. On its back side the forms of the deals were well defined; on the front one the crystals were best developed where the stream was most active.

In accordance with the above rate of increase of deposit, namely, ¾ in. in fifteen years, 5 in. would require 100 years, 4 ft. 2 in. 1,000, and 41 ft. 8 in. 10,000 years. The data given to arrive at these results may be relied on as being accurate. In the case now related, the rate of increase of deposit was likely to continue tolerably uniform; as the surface water could have no appreciable influence in augmenting or lessening the flow from the adit.

Boltsburn, Nov. 26 JOHN CURRY

Shooting-stars in the Red Sea

ON my way to India, in November 1872, I witnessed in the Red Sea the splendid phenomenon of a star-drift, a note about which may be of interest, in comparison with the observations at the same time in Europe.

November 24, at 8 P.M., about 600 miles to the south of Suez, I first saw a series of shooting-stars falling from about 70° W.N.W., but not in such a quantity that my attention was much attracted; I only made a note about it in my diary.

In the night of the 25th–26th I noticed nothing particular, but in that of the 26th–27th again many shooting-stars were to be seen.

But in the night of the 27th–28th, about 100 miles to the west of Aden, the phenomenon reached its height. Through the whole night many thousands of shooting-stars were falling from every quarter of the heavens, and in all directions. It was impossible for me to count the average number falling in one minute, although I tried several times to do so, because the eye could not be everywhere, and the shooting-stars did not come from one point only. I sat the whole night on deck, to witness this sublime phenomenon of nature, which certainly was far more splendid here in the tropics than in Europe, on account of the generally greater brightness of the stars in these latitudes.

A. B. MEYER

Cuckoos

IN vol. v. p. 383 of NATURE, you were so good as to publish a note of mine, in which I tried to describe exactly all that took place when I saw a young cuckoo throw a young pipit out of the nest.

I am much flattered to find that Mr. Gould has thought my note fit to be transferred to the introduction of his magnificent "Birds of Great Britain," and a rough sketch of mine worthy to be made the foundation of one of his large coloured plates. As, however, I have always tried in my drawings of facts in natural history to express neither more nor less than what I saw, I think it right to say that I am not the authority for many of the details in the large plate.

None of us saw the parent pipit looking on while the young cuckoo behaved so naughtily; we saw only two young pipits besides the young cuckoo, and no egg-shells. The young cuckoo was absolutely naked and blind, the young pipits partly fledged and bright eyed.

One curious point I tried to call attention to in my former note in these words:—"The nest . . . . was below a heather-bush on the declivity of a low abrupt bank . . . . The most singular thing of all was the direct purpose with which the *blind* little monster made for the *open* side of the nest, the only part where it could throw its burthen down the bank." This peculiarity my rough sketch could not, and Mr. Gould's plate does not, express.

J. H. B.

*ASTRONOMICAL ALMANACS**

VII.—*Continuation of the History of the "Nautical Almanac."*

UNTIL towards the end of the life of Maskelyne, its founder, the *Nautical Almanac* had the approbation of the English, and knew how to deserve the praise of foreigners; it was, according to Lalande, the most perfect ephemeris that had ever existed.* But, in 1808, death deprived Maskelyne, who was then about 76 years of age, of his pupil and industrious *collaborateur*, R. Hitchins, upon whom he had depended for ten years for the most important part of his work, the verification of the calculations, and who was during that time the real editor of the *Nautical Almanac*. The advanced age of Maskelyne no longer permitting him to undertake any active occupation, the work passed into irresponsible hands, the calculations fell into great confusion, and "while astronomy advanced, the *Nautical Almanac* remained stationary, and even retrograded."† Maskelyne died shortly afterwards, in 1811, and Brown of Tiedeswill (Derbyshire), was appointed to succeed him. The new director did not improve the *Nautical Almanac*, and English mariners and astronomers complained loudly; a reform was necessary. The Board of Longitude being incompetent to improve the work of which it had charge, Government abolished that body in 1818, by advice of the Admiralty, to which the publication of the work was entrusted, and which replaced the former body (which numbered sixteen members) by another much less numerous.

This new Board of Longitude was ingeniously formed; it was composed of a Resident Committee "of three persons well versed in mathematics, astronomy, and navigation, nominated by Government," to which was added, a Commission of the Royal Society, consisting of the president and three members, charged to support it, and, if need be, to control it. The members of the resident committee had to live in London, or its neighbourhood, and to lend their aid to the Commissioners of the Royal Society for the scientific questions within the domain of the Commission. They received a salary of 100*l.*, and the secretary of the committee, who was charged with the publication of the *Nautical Almanac*, a salary of 500*l.* Captain Kater, Dr. Wollaston, and Dr. Young were appointed resident members, and the latter, the secretary of the committee, had the editorship of the *Nautical Almanac*.

Young did much to improve the work, to restore to it the reputation for accuracy which Maskelyne had given it, and to render it capable of satisfying the constantly increasing wants of navigation. Thus, he introduced into the *Almanac*, in 1822, the apparent position, for every ten days, of twenty-four fundamental stars, which number was increased to sixty in 1837; mariners had thus constantly at their command the exact position of their reference points. Moreover, it is to him that we owe the publication of the elements by means of which we can predict occultations of stars by the moon, phenomena so useful to astronomers on an expedition, and to sailors whose ships are in a foreign harbour.

But these improvements were by no means the only ones which English astronomers and mariners demanded; as it was, the *Nautical Almanac* satisfied neither the one nor the other of these; sailors stood in need of the ephemerides and planetary distances of Schumacher, and astronomers of the supplement to these ephemerides.‡ Moreover, it often happened that these ephemerides appeared too late to be of any service to mariners who were setting out on a long voyage. Thus Young was exposed to criticism, very just, no doubt, but sometimes extremely violent. The result was an excessively sharp controversy, which, although sustained by most of the English astronomers, was concentrated in two eminent men, especially remarkable for their intense love of astronomy. The one was Sir James South, a rich landowner, who carried his love of astronomy so far as to devote the greater part of his income to the construction and maintenance of his observatory of South Villa. The other was Francis Baily, who, by dint of his persevering efforts, got the Board of Longitude to publish, in 1825, the original observations of T. Mayer, and who was, at a later period, the promoter of the measures taken for the publication of the numerous observations of Lalande. Behind these was the Royal Astronomical Society.

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* Continued from p. 70.

* "Correspondance astronomique francaise," of Baron de Zach, vol. iv. pp. 87, *et seq.*

† Sir James South's Address to the Royal Astronomical Society, February 12, 1830.

‡ The first of these ephemerides was due to the Baron de Zach, and Rear-Admiral Hovernorn caused them to be adopted by the Danish Governor in 1800. The Director of Copenhagen Observatory, Thomas Bugge, was then entrusted with their editorship, they were continued by Schumacher, and a little later were published, partly at the expense of the British Government. They gave the position of the planets Venus, Mars, Jupiter, and Saturn for every day in the year, and their distances from the moon every three hours.

The end to be attained was as clear as it was legitimate; it was sought to make the astronomical part of the *Nautical Almanac* more complete and make it answer all wants. Young and the other members of the Board of Longitude opposed to these attacks a resistance unhappily too energetic. But public opinion was formed, and the first satisfaction it obtained was the suppression of the Board of Longitude in 1828. Young was then in very bad health; indeed, it was seen that he could not live long, and it was not thought right to sadden his last days by taking from him the direction of the *Nautical Almanac.*

In the meanwhile, an event of the greatest importance took place on the Continent, which rendered reforms more urgent than ever. We speak of the radical change which the illustrious Encke had introduced into the "Jahrbuch" of Berlin, a change which embodied the greater part of the desiderata named long before by Baily and Sir James South, and for which was awarded to its author the gold medal of the Astronomical Society. To comprehend this completely, it is necessary to go a little further back, and learn the history of the "Jahrbuch" from the point where we left it.

VIII.—*Continuation of the History of the "Jahrbuch"*

After the death of Lambert, Bode was entrusted with the care of the *Jahrbuch* under the direction of the Berlin Academy. But soon the difficulties which resulted from the publication of this special work, under the orders of a numerous assembly, "in which everybody had the right of criticism, but in which no one had the effective responsibility," difficulties which, during the life of Lambert, had not had time to manifest themselves, became such that in 1783 the Academy of Sciences of Berlin decided of its own accord to give up the direction of the *Jahrbuch*, and to leave to that member who had the actual editorship the complete responsibility as well as the honour of that publication. It was, besides, by the advice of the celebrated Lagrange that Bode was consulted. The latter then became editor of the *Jahrbuch*, which was now published only "with the approval of the Academy."

This astronomer, however, followed religiously the plan traced by Lambert, not attempting any essential modification in the form of the *Jahrbuch*. But in attempting to render perfect the ephemerides, he sought chiefly to collect in the second part the most remarkable astronomical results of Germany and foreign countries. For this purpose he entered into correspondence with nearly all the astronomers of Europe, and the *Jahrbuch* of Berlin soon attained, in this respect, such a renown that, "from this time," says Lalande in his "Bibliographie Astronomique," "all astronomers are obliged to know German, for this work cannot be dispensed with." In the ephemerides the only modification of any importance on the plan of Lambert which Bode allowed himself during the whole of his editorship, was the addition of a table giving the corrections which it was necessary to make on the times of the rising and setting of the heavenly bodies at Berlin to obtain the times of the same phenomena in other latitudes.

During this time, however, astronomy had progressed. The beautiful memoirs of Bessel on the determination of the apparent positions of the stars, the improvement made on instruments, the convenience of the methods by which Bessel had learnt to correct and revise the results of these, had increased the wants of astronomers. On the other hand, the theory of the planetary movement had made immense advances, and the planetary system itself had been enriched by four telescopic planets—Ceres (Piazzi, Jan. 1, 1801), Pallas (Olbers, March 28, 1802), Juno (Harding, Sept. 1, 1804), and Vesta (Olbers, March 29, 1807). All presented the same peculiarity, that of revolving between Mars and Jupiter. It became necessary then to publish the ephemerides of these new planets, in order that astronomers might be able to observe them.

But Bode, who held for nearly half a century the astronomical sceptre of Europe, had then reached an advanced age, when the mind does not take easily to reforms.*

Bode died at Berlin, Nov. 23, 1826. J. F. Encke, then astronomer of the Observatory of Seeberg, near Gotha, Saxony, was called to the direction of the Observatory of Berlin and of the *Jahrbuch*.†

From the first volume which he published (*Jahrbuch* for 1830, May 1828), he realised all the reforms that German astronomers demanded. What then were those reforms universally called for?

IX.—*Programme of Reforms*

If we wish to understand them, it is enough to recall to mind that for a maritime people, ephemerides such as the *Nautical Almanac* and the *Connaissance des Temps* have a double purpose: to be serviceable to mariners and travellers, and also to astronomers, that is to say, to observatories.

At the very outset, it was evidently very useful to all that all the data of the work should be connected with the same kind of time, instead of giving for some the mean time, and for others the true time. And as astronomical tables are necessarily arranged on mean time, as on the other hand it is the most convenient for all the uses of navigation, it was good to take this mean time as the only time of the tables. It was, however, necessary to make an exception for the co ordinates of the sun at the moment of his passage on the meridian, which, very evidently, ought to be calculated for the apparent noon or the true noon. Besides, from the purely astronomical point of view, it was evidently convenient to calculate the places of the sun, of the moon, and of the planets, with all possible precision, so that the comparison of the observations with the tables might serve to amend the latter. It was necessary then to calculate to the 100th of a second the co-ordinates expressed in time, and to the 10th those expressed in arc. On the other hand, it was necessary to give, for every day in the year, at mean noon, the geocentric (AR, and D), and heliocentric co-ordinates of all the principal planets, and to publish in advance ephemerides of the telescopic planets near their opposition, an epoch favourable for their observation.

Again, the observation of the eclipses of the satellites of Jupiter being one of the best means of determining the longitude of a station, it was evidently of importance that the tables of these satellites should be brought to a high degree of perfection; and as, according to the opinion of the most distinguished mathematicians, the observation of all the phenomena which are presented by one of these satellites in superior or inferior conjunction is the best means of determining certain elements of the theory of the satellites of Jupiter, it was useful to give in the collection of ephemerides not only the epochs of the eclipses, but also those of the contact of the shadow of the satellite with the planet. Tables for the observation of the satellites at the time of their maximum elongation would also be very desirable.

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* Johann Elhert Bode was born at Hamburg on Jan 19, 1747. He studied under the guidance of his father, who kept a boarding-school, and at first intended him for a teacher. Mathematics, and particularly astronomy, were at an early age his favourite studies. He made his first astronomical observations in a granary, by means of a telescope which he had himself made; at 18 years he knew how to calculate, with considerable precision, eclipses and the course of the planets. Some time after, Dr. Bush, with whom chance made him acquainted, lent him his books and instruments, the vocation for which he was originally destined was from that time abandoned. In 1768 he published his treatise on Astronomy, "Die Anleitung zur Kenntniss des gestirnten Himmels," which had an immense success, shortly after he was made *Astronomer* of the Berlin Academy His most important astronomical work is his "Uranography," containing in 20 charts a list of 17,240 stars, double stars, nebulæ, &c.; *i.e.* 12,000 more than in the ancient charts

† Encke was born at Hamburg, Sept 23, 1791. Son of a protestant pastor, he studied under the celebrated Gauss at Gottingen, in 1814 he was appointed by B de Lindenau, Minister of State of Saxony, director of the Observatory of Seebo

From the mariners' point of view, for whom the moon is the principal heavenly body, the positions of the moon calculated for noon and midnight of every day would be insufficient on account of the considerable proper movement of our satellite. To obtain the longitude of a place by means of the observation of the passage across the meridian of one of the limbs, there would be required an excessively laborious calculation; the use of that method, however convenient, was then illusory. It was necessary to give the right ascension and the declination for every hour of the day, for the purpose of avoiding the employment of second differences except in cases where very great precision was sought for.

Then, when accurate tables of the movements of the planets were obtained, it was useful to add to the distances of the moon from the sun and from the stars, the distances of that body from the principal planets, the observation of which is more convenient and more certain than that of its distances from the stars.

But it was necessary to consider not only astronomers in observatories and sailors on board their ships, it was useful to enable astronomers on an expedition, and sailors when in a foreign harbour, and also geographers, to obtain the geographical co-ordinates of their station with ease and accuracy. From this point of view the method known as that of the Lunar Culminations holds the first rank, a method to which a beautiful work by Nicolai* gave a capital importance. The learned director of the Observatory of Mannheim showed with what facility the observations of the passage of the moon combined with those of a certain number of stars, called "stars of the moon," bordering on its parallel, and passing the meridian a little before or a little after (half-an-hour at the most), could give, sufficiently approximately, the difference of the longitudes of two places, even with a meridian instrument which was not perfect. On the other hand, Bessel and Hansen had given simple methods for calculating the horary movement of the moon. To apply this method of lunar culminations, it was then necessary to choose "stars of the moon," and to publish their positions every year, day by day, at the same time as those of the moon at the moment of its passing the meridian. This addition had, moreover, this advantage, that by indicating by an asterisk the stars comprehended between 4° and 14° of declination, the observers of the two hemispheres would have the elements most useful for improving continuously (*d'une façon continue*) the value of the lunar parallax. The phenomenon of the occultation of the stars of the moon offers, besides, an excellent means of determining longitudes. It was then important thus to calculate in advance and to publish all the elements likely to serve for predicting all the occultations in a given place, for the purpose of rendering the employment of this method easy to the navigator.

Finally it was indispensable, as well for the astronomical operations of observatories as for those connected with an astronomical or a geodetic expedition, that the collection of ephemerides should contain, for epochs sufficiently close to permit calculation for intermediate dates by simple proportion, the apparent positions of a very large number of stars of the greatest magnitude, and distributed both in the north and south hemispheres. It was useful, moreover, to join to this catalogue the values for very close epochs of the constants of Bessel, which enable one to pass from the mean position of a star at the commencement of the year to its apparent position on any day whatever.

For the principal circumpolars, α and δ Ursæ Minoris, the importance of which is so great in determining the various constants of a meridian instrument, and whose apparent positions vary much more rapidly than those of stars at a distance from the pole,—the apparent positions ought to be given every day.

Such is, with the exception of a few unimportant details, the list of reforms which the general opinion of astronomers demanded in England and Germany.

(*To be continued.*)

ON THE SECONDARY WAVES IN THE SPHYGMOGRAPH TRACE

IN a letter printed in this journal a short time ago (vol. viii. p. 464), Dr. Galabin refers to a paper which has been since published in the *Journal of Anatomy and Physiology* (No. XII. p. 1), for a fuller account of his views as to the theory of the pulse, of which we gave a short notice and criticism in a former number (vol. viii. p. 330). This second and more detailed description calls for further remark, especially as the author has found reason somewhat to modify his opinion on one important point.

As is well known, the sphygmograph trace of a pulse beat (see Fig. 1) consists of a primary rapid rise, followed by a more gradual fall, broken by a considerable undulation, termed the dicrotic wave, which varies in its distance from the next primary rise according to the rapidity of the pulse. Between the primary and the dicrotic rises in the trace, the descending curve is sometimes interrupted by another small undulation termed the "tidal" wave, by Mr. Mahomed, though the name *predicrotic* is better, as it does not involve any theoretical conceptions. It is the development in the trace of these predicrotic and dicrotic waves that Dr. Galabin discusses and his explanation of the former is the following.—The separation of the primary and tidal (predicrotic) waves is due to an oscillation in the Sphygmograph, caused by the inertia of the instrument. . . . In some cases the lever may be separated slightly from the knife-edge on which it rests, but generally the oscillation takes place in the instrument as a whole, and it may be followed by others in a descending series. With reference to this interpretation, it may be first remarked that it seems almost impossible that the whole sphygmograph should acquire a momentum in each pulsation, for it should be so adjusted on the arm that no part except the tip of the spring is in any way in contact with the artery, and when such is the case it is difficult to conceive of any shock being communicated to the whole. Again, any sudden upward impulse given to the instrument itself would be attended with a descent in the trace, for as the lever is only attached at one end, and there only on points, its pen would be slow to participate in the general movement of the framework, and would not rise so rapidly as the recording paper. The momentum acquired by the lever is a different thing. Marey and Sanderson have both shown that the primary rise in the trace may be attended with a sudden sharp-pointed wave, in the production of which the lever leaves the knife-edge on which it rests, returning to it after a very short excursion. *To prevent the excessive development of this imperfection* Marey has employed a small secondary spring to depress the lever; this spring Dr.

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* "Uber die Methode, langen durch Rectascensions-Differenzen gewahlten Vergleichsterne vom Monde zu bestimmen" (*Astronomische Nachrichten* for 1823 and 1824.)

Galabin persists in not employing, because he thinks—though the evidence he brings forward on the subject is extremely small—that it increases the number of minor vibratory undulations. Nothing of the kind, however, is the case. Nearly all properly-taken tracings from the pulse in health present, if there is a secondary spring employed, no percussion wave at all; and when it is present the true predicrotic wave is quite independent, as may be seen in Fig. 2, which is from a powerful, healthy pulse of 44 a minute, in which the rise *a* is the percussion, *b* the primary, *c* the predicrotic, and *d* the dicrotic wave. This true predicrotic wave varies in development with different pulse rates, being much more conspicuous in very slow pulses, and entirely absent in very quick ones, in which last a slight percussion wave is frequently found (see Fig. 1). Dr. Sanderson has previously described these two waves as co-existing, and he is undoubtedly right, as any who have had any considerable experience in Sphygmography in health will agree. It is Dr. Galabin who is in error, and it is but little compliment to other workers in the same field even to suppose that they have been sufficiently simple-minded to study and describe as physiological phenomena, instrumental errors so uncomplicated in origin and so readily comprehended. The chief argument he brings forward in favour of his explanation is that by placing a weight on the lever at different parts, and so altering its moment of inertia, the length of the predicrotic wave is varied. That the percussion wave which is developed when no secondary spring is employed is so affected, no one will doubt, because the resistance of the pen is less significant when the lever is heavy than when it is light, and therefore the wave is of shorter duration when it is weighted. This wave, however, is even then of such considerable length that it has not ceased before the true predicrotic wave has commenced, and it therefore disguises the true nature of the trace. It is, therefore, only when the secondary spring is employed that a proper trace can be obtained; because then only is it possible to see the full extent of the true predicrotic wave, uncomplicated by the superposition of the extraneous percussion wave. The latter does not appear as an extra element of the curve, but entirely disguises its true nature, on account of its being developed quite independently, when the lever is no longer in connection with the rest of the instrument, and therefore unaffected by whatever change may be occurring in the artery.

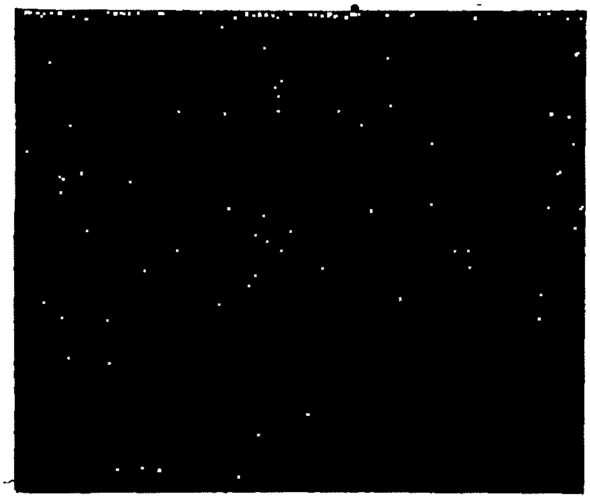
Fig. 1.—Sphygmograph tracings of healthy pulses, drawn to one scale, with rates between 44 and 170 a minute. They read from left to right.

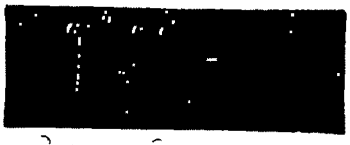
Fig. 2.—A tracing of a healthy pulse beating 44 a minute.

The cause of this predicrotic wave, which Marey gives of the similar one that appears in the hæmadromometer trace (Fig. 3, β) though considered by Dr. Galabin scarcely worthy of refutation, is supported by a large number of facts, especially by the hæmadromometer trace itself (Fig. 3, α, β). Its commencing in the radial artery as well as the carotid, at the moment of closure of the aortic valve, is also strongly in favour of the supposition that it is of shock origin; and that a shock may be transmitted through a column of fluid, which Dr. Galabin and some others seem to doubt, can be easily proved by suddenly closing an ordinary tap through which a large volume of water is passing, whereupon several oscillations of the retained liquid occur, producing a series of blows against the tap and perhaps the side of the tube, which are heard without difficulty.

The hæmadromometer trace (Fig. 3) shows also how completely the dicrotic wave is the result of the closure of the aortic valve, as Dr. Galabin also thought in his earlier paper; but in his second he attributes it to the oscillatory result of the inertia of the arterial walls, and the lateral momentum acquired by the blood. The mass of the arterial walls, and the lateral movement of the blood during distension are so slight, that neither are in any way competent to explain a movement so constant and so considerable as the dicrotic wave, especially when one so much more reasonable is to be obtained as the result of the valve closure. At all events no theory can be considered at all satisfactory which does not explain, in one way or another, the hæmadromometer trace, which is one of the foundations of arterial dynamics, and has been verified in all its details by Dr. Lortet of Lyons. Neither Dr. Galabin's theory, nor that of Mr. Mahomed, can be said in any way to take cognizance of the facts which it discloses, and they are incapable of doing so, therefore they must be considered inaccurate. Both these authors complicate their results by arguing from the analogy of a schema or model of the circulation constructed with elastic tubes; the arteries, however, are not simple elastic tubes, but tubes cut in elastic solids, being surrounded on all sides by yielding tissues, and they are not therefore comparable with tubes experimented on in air, and will not allow of comparative deductions being drawn from them.*

A. H. G.

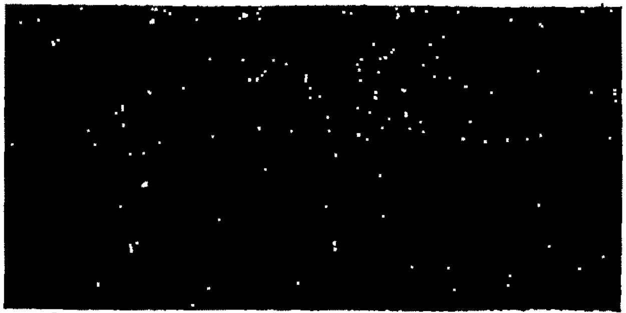
Fig. 3.—Hæmadromograph trace from the carotid. α, Curve of direction and force of blood current, all above the dotted line indicating an onward and all below a heartward stream. β, Simultaneous sphygmograph trace.

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* The blocks for Figs. I and III. are kindly lent by Prof. Humphry.

POLARISATION OF LIGHT

I.

LIGHT is said to be polarised when it presents certain peculiarities, hereafter to be described, which it is not generally found to possess. These peculiarities, although very varied in their manifestations, have one feature in common, viz. that they cannot be detected by the unassisted eye; consequently, special instrumental means are required for their investigation.

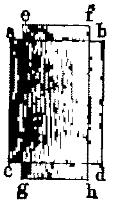

FIG. 1

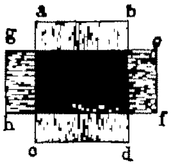

FIG. 2

The origin and meaning of the term polarisation will be better understood when some of the phenomena have been witnessed or described, than beforehand, and I therefore postpone, for the present, an explanation of it.

The subject of polarisation may be approached by either of two roads, the experimental or the theoretical. The theoretical method, which proceeds upon the principles of the Wave Theory of Light, is remarkably complete and explicit; so much so that it not only connects together many very diversified phenomena, but even, in some cases, has suggested actual prediction. But inasmuch as the theory without experimental facts would be little better than a study of harmony without practical music, it will be best to begin with experiment.

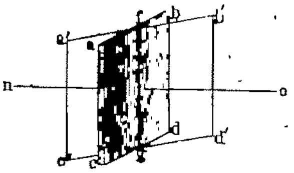

FIG. 3

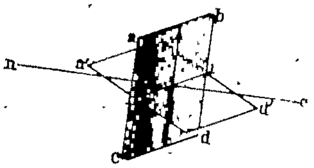

FIG. 4

It was stated above that certain instrumental means were requisite for detecting polarisation. Now there are various processes, some occurring in the ordinary course of natural phenomena, others due to instrumental appliances, whereby a ray of light may be brought into the condition in question, "or polarised." And it is a fact both curious in itself and important in its applications, that any one of these processes (not necessarily the same as that used for polarising) may be used also as a means of examining whether the ray be in that condition or not. This latter process is called "analysation." When two instruments, whether of the same or of different kinds are used, they are called respectively the "polariser" and the "analyser;" and the two together are included under the general name of "polariscope."

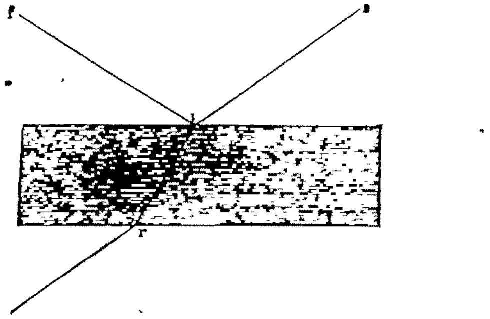

FIG. 5

The four principal processes by means of which a ray of light may be polarised are, reflexion, ordinary refraction, double refraction, and scattering by small particles. These methods will be considered in order; but before doing so, it will be convenient to describe the phenomena of polarisation as exhibited by some instrument tolerably simple in its action and of easy manipulation. For such a purpose a plate of crystal called tourmalin will perhaps serve better than any other to begin with.

Tourmalin is a crystal of which there are several varieties, differing only in colour. Very dark specimens generally answer the purpose well, excepting that it is difficult to cut them thin enough to transmit much light. Red, brown, or green specimens are usually employed; the blue are for the most part optically unsuitable. Some white, or nearly white, specimens are very good, and may be cut into thicker plates without loss of light.

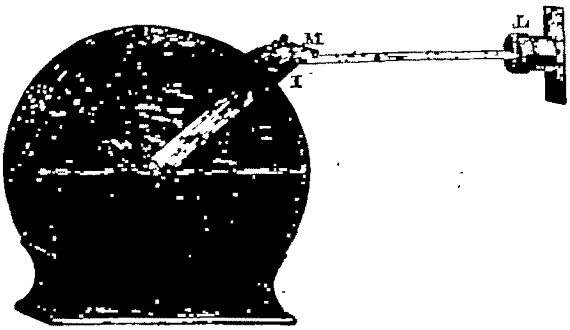

FIG. 6

If we take a plate of tourmalin cut parallel to a particular direction within the crystal called the optic axis (the nature and properties of which will be more particularly explained hereafter), and interpose it in the path of a beam of light at right angles to the direction of the beam, the only effect perceptible to the unassisted eye will be a slight colouring of the light after transmission, in consequence of the natural tint of the particular piece of crystal. But if we examine the transmitted beam by a second similar plate of tourmalin placed parallel to the former, the following effects will be observed. When the two plates are similarly placed, *i.e.* as if they formed one and the same block of crystal, or as it is technically expressed, with their optic axes parallel, we shall perceive only, as before, the colouring of the light due to the tints of the two plates. But if either of the plates be then turned round in its own plane, so as always to

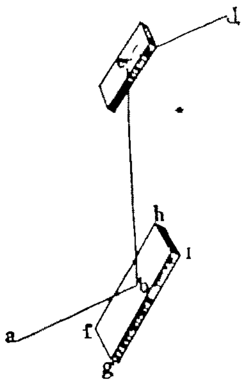

FIG. 7.

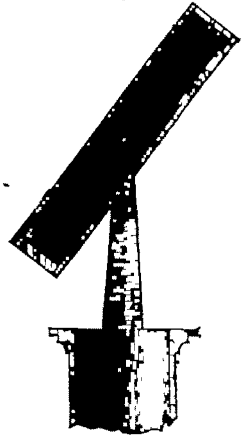

FIG. 8.

remain perpendicular to the beam, the light will be observed to fade gradually, until, when the moving plate has been turned through a right angle, the light becomes completely extinguished. If the turning be continued beyond the right angle, the light will begin to revive, and when a second right angle has been completed, the light will be as bright as at the outset. In Figs. 1 and 2 a, b, c, d, e, f, g, h represent the two plates; in Fig. 1 the two plates are supposed to be in the first position; in Fig. 2 the plate e, f, g, h has been turned through a right angle. Of the parts which overlap, the shading in Fig. 1 represents the deepened colour due to the double thickness of the crystal; in Fig. 2 it indicates the complete extinction of the light. The same alternation of brightness and extinction will continue for every right angle through which the moving plate is turned. Now it is to be observed that this alternation depends only upon the angle through which one of the crystals has been turned, or, as it is usually stated, upon the relative angular position of the two crystals. Either of them may be turned, and in either direction, and the same sequence of effect will always be produced. But if the pair of plates be turned round bodily together no change in the brightness of the light will be made. It follows, therefore, that a ray of ordinary light possesses the same properties all round, or as it may be described, in more technical language, a ray of ordinary light, is symmetrical in respect of its properties about its own direction. On the other hand a ray of light, after traversing a plate of tourmalin has properties similar, it is true, on sides diametrically opposite to one another, but dissimilar on intermediate sides or directions; the properties in question vary in fact from one angular direction to another, and pass through their phases or an entire period in every angle of 18 degrees. This directional character of the properties of the ray, on account of its analogy (rather loose, perhaps) to the directional character of a magnet or an electric current, suggested the idea of polarity, and hence the condition in which the ray was found to be was called polarisation.

Having so far anticipated the regular order of things on the experimental side of the subject, it will perhaps be worth while to make a similar anticipation on the side of theory. It is considered as established that light is due to the vibrations of an elastic medium, which, in the absence of any better name, is called ether. The ether is understood to pervade all space and all matter, although its motions are affected in different ways by the molecules of the various media which it permeates. The vibrations producing the sensation of light take place in planes perpendicular to the direction of the ray. The paths or orbits of the various vibrating ethereal molecules may be of any form consistent with the mechanical constitution of the ether; but, on the suppositions usually made, and none simpler have been suggested, the only forms possible are the straight line, the circle, and the ellipse. But in ordinary light the orbits at different points of the ray are not all similarly situated; and although there is reason to believe that in general the orbits of a considerable number of consecutive molecules may be similarly situated, yet in a finite portion of the ray there are a sufficient number of variations of situation to prevent any preponderance of average direction.

This being assumed, the process of polarisation is understood to be the bringing of all the orbits throughout the entire ray into similar positions. And in the case of the tourmalin plate the orbits are all reduced to straight lines, which consequently lie in one and the same plane. For this reason the polarisation produced by tourmalin, as well as by most other crystals, is called rectilinear, or more commonly, plane polarisation. This property of tourmalin may also be expressed by saying that it permits only rectilinear vibrations parallel to a particular direction determined by its own internal structure to traverse it.

Adopting this view of polarisation as affected by a plate of tourmalin, it would be interesting to ascertain the exact direction of the vibrations. And a simple experiment will go far to satisfy us on that point. The argument, as now stated at least, is perhaps based upon general considerations rather than upon strict mechanical proof; but the experimental evidence is so strong that it should not be denied a place here. Suppose for a moment that the tourmalin be so placed that the direction of vibration lies either in or perpendicular to the plane of incidence (that is, the plane containing the incident ray, and a perpendicular to the surface on which it falls at the point of incidence); then it is natural to expect that vibrations executed in the plane of incidence will be far more affected by a change in the angle of incidence than those perpendicular to that plane. In fact the angle between the direction of the vibrations and the surface upon which they impinge, will in the first case vary with the angle of incidence; but in the second case it will remain unchanged.

In Figs. 3 and 4, n, o represents the ray of light; the arrow the direction of vibration, a, b, c, d, a', b', c', d', the plate in two positions, turned in the first instance about the direction of vibration, in the second about a line perpendicular to it.

Dismissing, then, the former supposition, and supposing that nothing whatever is known about the direction of vibration; then, if all possible directions be taken in succession as pivots about which to tilt or turn the second tourmalin, it will be found that for one direction the intensity of the light diminishes more rapidly with an increase of tilting (or, what is the same thing, with an increase of the angle of incidence) than for any other. And further, that for a direction at right angles to the first, the intensity of light diminishes less than for any other; while for intermediate directions the diminution of intensity is intermediate to those above-mentioned. In accordance, therefore, with what was said before, we may conclude that the vibrations are parallel to the line or pivot about which the plate was turned when the diminution of light was least.

Secondly, polarisation may be effected by reflexion. If light reflected from the surface of almost any, except metallic, bodies be examined with a plate of tourmalin, it will in general be found to show traces of polarisation: that is to say, if the plate be caused to revolve in its own plane, and the reflected rays be viewed through it, then in certain positions of the plate, the reflected light will appear less bright than in others. If the angle at which the original rays fall upon the reflecting surface be varied, it will be found that the amount of alteration in brightness of the light seen through the revolving tourmalin (or analyser) will also vary. This fact may also be expressed thus: in polarisation by reflexion, the degree of polarisation, or the amount of polarised light in the reflected rays, varies with the angle of incidence on the reflecting surface. But at a particular angle, called on that account the polarising angle, the polarisation will be a maximum. This angle (usually measured between the incident ray and the perpendicular to the reflecting surface) is not the same for all substances; in fact it varies with their refractive power according to a peculiar law, which, when stated in the technical language of science, may be thus enunciated: the tangent of the polarising angle is equal to the refractive index. Simple geometrical considerations, combined with the usual expressions for the laws of reflexion and refraction, will show that this relation between the polarising angle and the refractive index may be also expressed in the following way: If light be incident at the polarising angle, the reflected and refracted rays will be at right angles to one another.

In Fig. 5, s, i represents the incident, i, f the reflected, and i, r the refracted ray. Then s, i will be incident at the polarising angle when the angle s, i, r is a right angle.

An apparatus devised by Prof. Tyndall for experimentally demonstrating the laws of reflexion and refraction is admirably adapted for verifying this law. The following description is quoted from his Lectures on Light:—"A shallow circular vessel R I G (Fig. 6) with a glass face, half filled with water rendered barely turbid by the admixture of a little milk or the precipitation of a little mastic, is placed upon its edge with its glass face vertical. By means of a small plane reflector M, and through a slit I in the hoop surrounding the vessel, a beam of light is admitted in any required direction.* If a little smoke be thrown into the space above the water, the paths of the incident, the reflected, and the refracted beams will all be visible. If then the direction of the incident beam be so adjusted that the reflected and the refracted beams are at right angles to one another, and a Nicol's prism be interposed in the path of the incident beam, it will be found that by bringing the vibrations alternately into and perpendicular to the plane of incidence we shall alternately cut off the reflected and the refracted ray. Thus much for the verification of the law. But not only so, if we take different fluids and for each of them in succession adjust the incident beam in the same manner, we shall only have to read off the angle of incidence in order to ascertain the polarising angle of the fluid under examination.

The polarising angle for glass is 54° 35′.

Thus, in Fig. 7, let a, b be the incident, and b, c the reflected ray at the first plate; b, c the incident, and c, d the reflected ray at the second plate; then the ray will be polarised more or less according to the angle of incidence, at b, and will be analysed at c.

But in accordance with the principle stated above, viz. that any process which will serve for polarising, will serve also for analysing, we may replace the analysing tourmalin by a second plate of glass (or whatever substance has been used for the first reflexion) placed parallel to the first, and in such a position as to receive the reflected ray; and if the second plate be then turned round the ray reflected from the first plate b c, as an axis, it will be found that at two positions of rotation (first when the plates are parallel and secondly when one of them has been turned through 180°) the light reflected from the second plate is brightest, and at two positions at right angles to the former the reflected ray is least bright. The degree of dimness at the two positions last mentioned will depend upon the accuracy with which the reflecting plates have been adjusted to the polarising angle; and when this has been completely effected, the light will be altogether extinguished.

Suppose now that the reflecting substance be, as in the case of glass, transparent. Then it will not be surprising if, when the reflected ray is polarised, the refracted ray should also exhibit traces of polarisation. And in fact every ray of ordinary light incident upon a transparent plate is partly reflected and partly refracted; the reflected ray is partially polarised, and so also is the refracted ray. This being so, if, instead of a single plate, we use a series of plates placed one behind the other, each plate will give rise to a series of reflected rays, due to successive internal reflections. The sum of all these will give the intensity and the amount of polarisation of the total reflected light. The phenomenon of these reflexions is therefore rather complicated; and the modifications due to the additional plates do not materially alter the proportion of polarised to unpolarised light. It is, however, otherwise with the refracted rays. The rays transmitted by the first plate enter the second in a state of partial polarisation, and by a second transmission undergo a further degree of polarisation. If this process be continued by having a sufficient number of plates, the ray finally emergent may have any degree of polarisation required.* And it is worthy of remark that, in proportion as the rays become more and more polarised, so does a less and less quantity of light become reflected from the surfaces of the plates; and consequently, except in so far as light is absorbed by actual transmission through the substance of the plates, the emergent ray suffers less and less diminution of intensity by each additional plate. So that when a certain number has been attained the intensity received by the eye or on a screen is practically unaffected by increasing their number.

Fig. 8 is a general representation of such a pile of plates viewed edge-ways. The plates are secured in a brass frame, and the whole supported on a stand.

W. SPOTTISWOODE

(*To be continued.*)

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* Plates of the thinnest description are the best; two or three give good effects, but if the surfaces lie parallel and the glass be highly transparent the number may be advantageously increased to 10, or even 12.

THE ROYAL SOCIETY

THE following extracts from the Minutes of the Council of the Royal Society under the dates given, may be interesting to some of our readers:—

Jan. 26, 1860.—The President having brought under the consideration of the Council the present scale of remuneration of the Secretaries, it was resolved:—"That a Committee be appointed to inquire into the matter and report thereon to the Council; the Committee to consist of the President and the Treasurer, with Mr. Barlow, Mr. Bell, and Dr. Farr."

Feb. 23, 1860.—The President presented the following Report from the Committee appointed on January 26 to consider the question of the remuneration of the Secretaries.

"Your Committee beg to Report to the Council that, in performing the task which was imposed upon them, they have inquired into the duties of the Secretaries at various times, the gratuities which have been awarded to them, and the financial condition of the Society.

"They have been favoured with valuable information and opinions by former Officers of the Royal Society,—Sir John Herschel, Mr. Brande, Dr. Roget, and Sir John Lubbock.

"Previous to the year 1720 no regular salaries were assigned to the Secretaries, but it was customary to present them from time to time with sums varying from 10*l.* to 20*l.* under the name of 'Gratuities.'

"In 1720, on the motion of the President, Sir Isaac Newton, the Council directed that 50*l.* should be paid to each of the two Secretaries annually. In 1732 this amount was increased to 60*l.*, and in 1760 to 70*l.* 10*s.* In November 1799, on the motion of the President, Sir Joseph Banks, the amount of the salaries was reconsidered by the Council, and raised to *One Hundred Guineas* to each Secretary, at which amount they have remained from that time to the present.

"The office of 'Foreign Secretary' originated in a legacy of 500*l.* bequeathed to the Society in 1719 by Mr. Robert Keck, for the express purpose of remunerating a person for carrying on foreign correspondence. In 1720 the first Foreign Secretary was appointed, with a salary of 20*l.* a year, which sum has been paid, without increase, from that time to the present.

"It is the concurrent opinion of all who have the best means of knowing, that since the gratuities were last fixed in 1799 the business of the Society and the duties of the Secretaries have largely increased. The increase of Fellows and the larger income of the Society have enabled it to extend its operations. In the ten years 1790 to 1799, 319 papers were communicated to the Society; and in the ten years 1850 to to 1859, the number of such papers was 672. Some of the communications are short notices for publication in the Proceedings, and it is impossible to determine precisely in what ratio the work has increased; but your Committee are disposed to believe that it is represented approximately by the above figures.

The Secretaries now edit the 'Transactions' and the 'Proceedings' which are found so useful by the Fellows, and this latter duty has added considerably to their labour.

"The current revenue of the Society may be set down at about 3,514*l.* of which 1,150*l.* are derived from rents and dividends, and 517*l.* from the Stevenson bequest. The latter sum, it is known, will increase as lives fall in. The annual subscriptions amount to 1,126*l.*; the entry fees, estimated on an average of eleven years, will be about 170*l.*; the compositions 360*l.*; the Transactions will yield 276*l.*; making the aggregate revenue under these heads 1,932*l.* Your Committee see no reason to believe that these sources of income are likely to fail.

"The current annual expenditure may be stated at about 2,839*l.*; namely, 1,177*l.* on printing; 764*l.* on gratuities, salaries and wages; 187*l.* on books and binding; 511*l.* on house expenses; 200*l.* on Catalogue of Periodicals. These items necessarily fluctuate, and the printing bill last year exceeded considerably the above amount; but the amount just stated for printing is estimated from an average of the last eleven years. The income of the Society has thus for some years exceeded the expenditure by about 675*l.*

"Looking at the duties which now devolve upon the Secretaries, of regularly attending Meetings, reading papers, editing the 'Transactions,' preparing the 'Proceedings' for publication, and other work,—looking also at the remuneration which it is found desirable to give gentlemen who discharge less onerous duties merely as editors of literary works in the present day,—your Committee are of opinion that the Council will be acting quite in conformity with the sound principles which were laid down in Sir Isaac Newton's presidency, and have been acted on since, by increasing the gratuity to each of the two Secretaries. As the result of the inquiries made by your Committee, they would suggest that the addition should be 95*l.*, raising each gratuity from 105*l.* to 200*l.* This would involve an increase of 190*l.* in the expenditure.

"The relations of the Society with foreign countries may be largely extended, and your Committee are of opinion that to accomplish this object 80*l.* may be advantageously added to the 20*l.* now voted making the annual gratuity of the Foreign Secretary 100*l.*

"The total augmentation of the expenditure under this arrangement would be 270*l.* leaving a probable annual surplus of 400*l.* to be devoted to the numerous purposes which fall naturally within the scope of the Society's inquiries.

"Your Committee are of the opinion that the offices efficiently discharged will still be to a great extent honorary; and that so long as the Society itself is so fortunate as to have able, industrious, and eminent men as its Secretaries it will be still largely in their debt.

"Should the finances of the Society, through any unforeseen circumstance, require it, there would not, your Committee apprehend, be any difficulty in again revising the scale of gratuities which may be awarded."

This Report having been read, it was, on the motion of the Treasurer, seconded by Sir R. Murchison,

"Resolved—That the recommendation of the Committee respecting the honorarium to be given to the two principal Secretaries be adopted."

June 20, 1872.—On the motion of Dr. Sharpey, [pursuant to notice given, seconded by Mr. Spottiswoode—

"Resolved—That the following mode of procedure be adopted in the nomination of Fellows to be recommended to the Society for election as Council and Officers.

"1. The subject of the new Council shall be taken into consideration at a meeting of Council, to be held on the last Thursday of October; and with the summons for that meeting there shall be transmitted a list of the members of the existing Council, with the number of their attendances at meetings up to that date; also a list of the Fellows of the Society, with an indication of those who have at any time served on the Council, and the dates of their service.

"2. At this meeting the names of those members of the existing Council who retire at the ensuing anniversary shall be determined. Thereafter each member present shall hand to one of the secretaries a list of not exceeding ten Fellows whom he proposes for the new Council, of whom five shall not have already served on the Council. Members not able to be present may send in similar lists previous to the meeting. The several lists of names so proposed shall then be read out by the secretary.

"3. Before the next following meeting, the president and officers shall prepare a list of twenty-one names for consideration by the Council, which list shall include ten names selected from those proposed at the previous meeting, or other names, if required to make up that number. The list so prepared, together with a statement of the names proposed and the number of votes given for each, shall be sent out confidentially with the summons for the ensuing meeting, at which meeting the names to be finally recommended shall be balloted for. In taking the ballot, a copy of the list prepared by the officers, with such alterations as he may see fit to make therein, shall be delivered by each member of the Council present and voting, and the names found to have the majority of votes shall form the list to be recommended to the Society.

"The President and Council shall then nominate by ballot, out of the proposed Council, the persons whom they recommend to the Society for election to the offices of President, Treasurer, Principal Secretaries, and Foreign Secretary for the ensuing year."

NOTES

THE present year is already remarkable for the number of eminent scientific men who have gone over to the majority: and now, just at its close, one of the most eminent in his own sphere has taken his departure. A telegram dated New York, December 14, announces the death of Prof. Louis J. R. Agassiz, in his 67th year, he having been born in Switzerland in 1807. We shall content ourselves with the bare announcement at present, hoping to be able to give, next week, a memoir of the great naturalist. Meantime we would draw the attention of our readers to the interesting letter from Agassiz in our correspondence column, sent us by Sir Philip de Malpas Grey-Egerton, Bart.

A MEETING, with Sir William Armstrong as chairman, was held at Newcastle last Thursday, to consider the question of a memorial to the late Mr. Albany Hancock. It was unanimously resolved that the most appropriate memorial that could be raised to Mr. Hancock, would be a Professorship of Natural History in the Newcastle College of Physical Science, to be called, after him and his friend and conjoint worker, the late Mr. Alder, the "Hancock and Alder Professorship." Over 1,000*l.* were subscribed at the meeting, and we have reason to hope, from the general esteem in which the two men were held, the high value of their labours, and the great wealth of Newcastle and the surrounding district, that the remaining 4,000*l.* or 5,000*l.* necessary to endow a Natural History chair, will be raised without difficulty. Very few, even of scientific men, seem to be aware of the great amount and value of the work done by Mr. Hancock. The Rev. A. M. Norman, in speaking at the meeting, said that the nature and extent of the work done by Mr. Hancock, would only be realised by degrees. "His work was abstruse science; work which was labour, day by day, under the microscope; work which was carried on from week to week and from year to year, and which was published in the journals of the scientific associations; work which was at present not thoroughly understood even by scientific men, and which could only be fully appreciated and utilised years afterwards, when others should arise who devoted themselves to the same branches of science as Mr. Hancock had done."

The Professorship of Zoology in the Royal College of Science, Dublin, has become vacant through the appointment of Prof. Traquair to the Keepership of the Natural History collections in the Museum of Science and Art, Edinburgh. Candidates for the appointment should apply, forwarding testimonials, to the secretary, Science and Art Department, South Kensington, S.W.

On April 14 and following days, an Exhibition in Natural Science will be offered for competition, in connection with King's College, Cambridge. Candidates must be British subjects under twenty years of age, unless already undergraduates of the College, who are also eligible, if in their first or second year. The Exhibition is worth at least 80*l.* a year, and is tenable for three years, but not with any other Exhibition, Scholarship, or Fellowship. There will be three papers in Natural Science (including Chemistry, Physics, and Physiology), and papers in Elementary Classics and Mathematics.

Ninety-four essays have been sent in in competition for the 100*l.* prize offered by Lord Cathcart, the president of the Royal Agricultural Society, for the best essay on the potato disease and its prevention; but the committee appointed by the council of the society to adjudicate the prize do not advise its being awarded to any of the competitors. They recommended, however, that a sum of money be granted for the purpose of inducing a competent mycologist to undertake the investigation of the life-history of the potato-fungus, *Peronospora infestans*, in the interval between the injury to the potato plant and the re-appearance of the fungus in the following year; and that the society should offer prizes for kinds of potatoes that would resist disease during a series of experiments to be continued for three successive years.

A correspondent sends us a letter from Dr. A. B. Meyer in which the latter asserts that D'Albertis did not cross New Guinea at all, and that he himself is the only explorer who has done so. With regard to his statement that the fauna of New Guinea is not rich, he says he refers to the higher vertebrates; he intends to publish shortly a "Prodromus Faunæ Novæ Guinensis." The latitude of the point on MacCluer Gulf, at which he arrived in crossing, was 2° 38′, and not 20° 38′, as by an obvious misprint was stated in his article in vol. ix. p. 79 of Nature.

A peculiar result has been arrived at by Professor Fick, of Wurtzburg, in his experiments on the blood-pressure in the heart and aorta of the dog (Verhandl. d. physik. med., vol. iv. p. 223). He finds that if a straight tube is the manometer employed, the column of fluid rises higher when the lower end is in the aorta, than when in the left ventricle itself. There are several objections to the method adopted which might tend to the production of this extraordinary result, so contrary to all preconceived notions and to the experience of M. Marey, who, when discussing the subject (Circ. du Sang., p. 192) remarks, "Frequently verified measurements, made by the employment of ampoules on the horse, show that the maximum pressure in the aorta is slightly more feeble than in the ventricle, though, in some cases, it is nearly the same." May it not be that the presence of the tube in the ventricle, and the associated imperfect closure of the semilunar valves, reduces the pressure in the one case, and that on its withdrawal into the aorta the heart again resumes its more vigorous action. It seems physically impossible that the aortic pressure should be greater than the ventricular during any portion of the systolic period in which the semilunar valves are open.

A valuable contribution to anatomical science, by Prof. Turner, has appeared in the current number of the *Journal of Anatomy and Physiology*, in which the relations of the different cerebral convolutions to the parts of the brain-case with which they are in contact are discussed. Each lateral half of the scalp is divided with the aid of the best marked prominences and sutures as landmarks, into ten regions, which are again capable of further subdivision, and the convolutions found in each are stated. It is shown that the lobes of the brain by no means correspond exactly with the bones from which they have been named, but frequently extend under the cover of others, or only partially occupy the surfaces of their own. These observations are particularly valuable now that the subject of the localisation of the cerebral functions has attained such prominent importance.

Captain Potter of the U.S. whaler *Glacier*, we learn from *La Nature*, says that he has discovered some relics of the Franklin Expedition in the Polar regions. Captain Potter left New Bedford, Mass., on July 19, 1871, and remained absent twenty-six months, most of which time he spent in the neighbourhood of the place where Franklin and his companions abandoned their vessels. At Repulse Bay a party of Esquimaux came to trade with Captain Potter. He was considerably surprised to see them offering in exchange for culinary utensils, part of a table-service of silver, which they declared belonged to the appointments of Franklin. There are two large table-spoons, two large four-pronged forks, an ordinary tea-spoon, and sugar-spoon. All these articles are of old-fashioned make. The natives assert that after having quitted their ships, Sir John and his companions separated into two bands, one of which took the direction of the Red River, and the other made for the territory of the Hudson's Bay Company. They say also that Sir John and his companions died solely from natural causes, and Captain Potter believes they speak the truth.

The adult female Indian Rhinoceros, which has been in the Zoological Society's Gardens since July 1850, then not larger than a full-sized dog, died on Sunday last, having been ill for some time previously. The coldness of the weather and the fog were probably the exciting causes of its death, though no definite pathological changes have been found on *post-mortem* examination. There were no symptoms of senile decay. An interesting point may be mentioned, which is, that one of the wisdom teeth from the lower jaw was found in the cæcum, with the fangs and dentine entirely absorbed. This tooth must have been in this peculiar situation for some time, probably years, as it is almost unworn, whilst the corresponding molar on the opposite side is still in place, much worn, as is the same one in the maxilla of the same side; that opposite to the proper situation of the missing tooth being almost as complete as when it was cut. The large accumulation of hay in the cæcum, in which the tooth was embedded, appeared fresh and but little modified by the digestive process, so that it must have been there but a short time. In a Sumatran Rhinoceros, also, which died some time ago, two large beans were found in the cæcum, which could not have been introduced in the food for at least four or five months before the individual's decease.

We have received a Catalogue of Apparatus suitable for Lectures and Class Instruction in, Subject VIII. Acoustics, Light, Heat, and IX. Magnetism and Electricity, in connection with the Science and Art Department. The object of the Catalogue, which contains the names of 141 different pieces of apparatus, is to show those articles on which the Department allows a discount of 50 per cent. It is of great importance that this Catalogue should become widely known, and we hope the facilities here offered for the acquisition of serviceable apparatus will be extensively taken advantage of.

THE fourth number of the circulars of the U.S. Bureau of Education for 1873 contains a list of publications by the members of certain college faculties and institutions of learning in the United States from 1867 to 1873, and constitutes quite a valuable record of scientific activity during that time. We hope the Bureau will continue such a publication yearly, and we only wish there was any prospect of a similar undertaking in our own country.

WE would draw attention to the efforts being made by the Directors of the London Polytechnic Institution to give a scientific character to part of the entertainment which they provide for the public. Mr. E. V. Gardner is at present delivering the seventh and eighth of a series of lectures descriptive of "Inventions and Appliances Useful or Necessary to Everyday Life," the subjects being "Sugar: from the Cane to the Teacup," and "The Silber Light and Lightning." We wish the Directors of the Polytechnic success in this attempt to make their institution administer to instruction as well as amusement.

MR. J. D. PAINTER of Macclesfield sends us some very interesting ornithological notes relative to East Cheshire. A short time ago, a bird which had been hovering round the Grammar School for six weeks, was brought him; it had evidently been killed by a violent blow with either a stick or a stone. Upon examination it proved to be the Crested Lark (*Alauda cristata*), which is a common bird throughout the Continent of Europe, but not a native of Britain. Indeed, it is a very rare visitor, since only two or three instances are on record of its having been met with in this country. Occasionally, the neighbourhood of Macclesfield is resorted to by other strangers of the feathered tribe. Some few weeks since the Black-headed Gull (*Larus ridibundus*) was shot in Swithamley Park, and previous to that, on the same estate, the Common Buzzard (*Buteo vulgaris*) had been shot upon the Roaches. A few years further back the Roller (*Coracius garrula*), and a Hobby were killed two or three miles south of the town. In tempestuous weather the Stormy Petrel or Mother Carey's Chicken has been frequently picked up either dead or in an exhausted condition near Macclesfield: and Terns are occasionally shot. The Siskin is a winter visitor, some become victims to the bird-catchers, and the Brambling, also a winter visitor, is now and then shot or snared. Twenty-five years ago, that delightful songster the Woodlark bred about Gawsworth, but in like manner it became completely extinguished. The Grey or Wild Goose (*Anser ferus*) and the Curlew (*Numenius arquata*) came almost every year to breed on Danes Moss, but when the North Staffordshire Railway was carried across it these birds deserted it. Last year, however, the Curlew returned and nested, but some boys took the eggs when just upon the point of being hatched; and this year the birds have not been seen in the neighbourhood. A few Woodlarks have likewise returned lately, and they will most probably share the same fate as their predecessors, unless the forthcoming amended Birds' Act be extended to them and also to the Skylark, which have been most unaccountably omitted in the Act now in force. A few years ago Mr. Painter gave a lecture at the Town Hall upon the Geology, Archæology, Botany, Ornithology, and Zoology of Danes Moss and its borders, when he mentioned some rare and beautiful bog plants, &c. that grew upon it. In the course of a year or two nearly the whole of them were rooted up and carried away, chiefly by strangers.

THE additions to the Zoological Society's Gardens during the last week include a Zebu (*Bos indicus*) born in the Menagerie; a Greater White crested Cockatoo (*Cacatua cristata*) from Moluccas, presented by Mr. T. Towndrow; a Squirrel Monkey (*Saimaris sciurea*) from Guiana, presented by Mrs. Paget; a Parrot Crossbill (*Loxia pityopsittacus*) and two common Crossbills (*L. curvirostra*), European, purchased.

EFFECTS OF ALCOHOL ON WARM-BLOODED ANIMALS *

AFTER referring to what had already been done in reference to this subject, Prof. Binz gave an account of his and his pupils' researches during the last years. They concerned especially two points (1) the influence of alcohol on the temperature of the blood, and (2) the causes of this influence.

As in every powerful attack on our organism, so also in the case of alcohol, the questions arise—In what quantities it worked? and whether the organism to be experimented on was previously accustomed to its influence or not? Taking into exact consideration these two points, so often disregarded, the answer is as follows:—The pretended heat of the organism does not exist. The subjective impression is, at least partially, the consequence of an irritation of the nerves of the stomach and of the enlargement of the vessels arising in the skin. When given in small doses the thermometer shows no extraordinary increase or decrease of the temperature of the blood. Moderate doses, which lead by no means to drunkenness, show a distinct decrease of about half-an-hour duration or more; and inebriating quantities evince a still more decided lowering of 3 to 5 F., which lasts several hours. The decrease in the temperature after moderate doses takes place most successfully in warm-blooded animals, which have had for some time previously no alcohol administered. When inured to it, the organism does not answer on such doses by any measurable cooling or by the reverse.

Good results are yielded more easily by a feverish than by a healthy animal. For these experiments strong guinea-pigs, rabbits, or dogs of the same origin and of the same quality have been used. Under their skin some cubic-centimeter of ichor or putrifying blood was injected. After thus proceeding, the temperature of the animal rises several degrees, and all the symptoms appear which are to be observed in human beings suffering from putrid fever. If the quality of the poisonous substance be right, the animal expires in a few days. Not so, however, if, simultaneously with the ichor, alcohol diluted with water is administered. The temperature then remains lower from the beginning, and the one animal may be seen to die, whilst the other runs about. The analysis of these experiments shows a threefold action of alcohol in putrid fever—(1) the diminution of the heat; (2) reduction of the putrid processes; and (3) rising of the action of the nerves and of the heart.

Prof. Binz then remarked on the causes of such antipyretic action of alcohol. He pointed out several possibilities which here may concur, and has proved by a series of experiments that two of them really take place. It is the action of the heart, together with the enlargement of the vessels of the skin, which allow a stronger evolving of the blood at the surface of the body, and then the moderating influence of alcohol on the chemical metamorphosis of tissues. All these results seem to be suggestive for the use and abuse of alcohol in social life as well as in illnesses, and they explain a great many empiric observations in both departments. The paper of Prof. Binz will be published at length in one of the next numbers of *Humphrey's Journal of Anatomy and Physiology*.

Dr. Brunton remarked that the performance of the vital functions depended on oxidation of the tissues, and Professor Binz's observation that this was lessened by alcohol was the key to an explanation of its physiological effects. These may be nearly all explained on the supposition that the power of the nervous system is diminished, different parts of it becoming successively paralysed. First, the vasomotor nerves become affected and the blood-nerves consequently dilated. After a glass or two of wine, the hands may be noticed to be of a very red colour and plump, showing that arterial blood is flowing freely through the capillaries, and at the same time the veins are dilated and full. All the vessels of the body, however, are not dilated at the same time. In some persons those of the stomach or intestines become dilated, and the blood being thus abstracted from the head the brain becomes anæmic, and the individual dull and sleepy. In others the arteries of the head become dilated first, and in consequence the brain receives a full supply of blood, and the intellect becomes more vigorous. If this stage is not passed the functions return to their normal condition, and no harm ensues; but if more alcohol is taken the paralyis extends to other parts of the nervous system. Sometimes the cerebral lobes, which are the organs of the mental faculties, are first affected, and sometimes the centre, for co-ordinated movements usually supposed to be the cerebellum, or, as it is often expressed, "one man gets drunk in his head, another in his legs." When the head is affected judgment becomes impaired, though memory and imagination may still be more active than usual. These faculties next fail, and the emotions become hilarious, pugnacious, or lachrymose. The spinal cord is generally unaffected even when the cerebellum is paralysed, and a man who is utterly unable to walk can still ride, the mere pressure of the saddle upon his thighs being sufficient to cause reflex contraction of his adductor muscles and fix him firmly on his seat, although the upper part of his body may be swaying about like a sack of wheat. The cord itself next becomes paralysed, and lastly the medulla oblongata, which regulates the respiratory movements.

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* Abstract of paper read at the British Association, Bradford, by Prof. Binz, of Bonn, with Dr. Brunton's remarks.

After relating an anecdote illustrative of the effects of alcohol in hastening death during exposure to cold, Dr. Brunton remarked that, notwithstanding all these apparently injurious actions, alcohol was of great service when properly used. Many men came home from their offices completely exhausted, and the stomach, sharing the general exhaustion, is unable to digest the food which lies heavily in it, and incommoding instead of strengthening the individual.

A glass of sherry taken with the food will stimulate the stomach to increased action, and by the time the effect of the stimulus has passed away the food has digested and absorbed, and sustains the effect which the alcohol temporarily produced. When taken in considerable quantities for a long time, alcohol is apt to produce deposit of fat and fatty degeneration of organs, rendering a person not only less capable of work, but liable to succumb to disease.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 11.—"Researches in Spectrum-Analysis in connection with the Spectrum of the Sun."—Part III., by J. Norman Lockyer, F.R.S.

The paper commences with an introduction, in which the general line of work since the last paper is indicated. Roughly speaking, this has been to ascertain the capabilities of the new method in a quantitative direction. It is stated that while qualitative spectrum-analysis depends upon the *positions* of the lines, quantitative spectrum-analysis on the other hand depends not on position but on the *length*, *brightness* and *thickness* of the lines.

The necessity of maps carefully executed and showing the individuality of each line is shown; and it is stated that the execution of these maps required the use of the electric arc to render the vapours of the metals incandescent. A battery of 30 Grove's cells of one pint capacity was accordingly employed in the researches about to be described.

The difficulties of eye-observations of the characters of the lines compelled the application of photography, another reason for the use of which existed in the facility it afforded for confronting spectra with each other, and so eliminating coincident lines, since the lines, if due to impurities, would be longest and thickest in the spectrum to which they really belonged.

The portion of the spectrum at present worked upon is that from H to F.

Another branch of the research has been the construction of a Table of all the named Fraunhofer lines, showing the lengths and thicknesses of the metallic lines to the absorption of which they were due; this Table enabled the author to allocate upwards of 50 lines in the solar spectrum, presumably overlooked by Angstrom and Thalén. The table was intended as a preliminary to a new photographic map of the spectrum from H to F, on a larger scale than Angstrom's, which was intended to clear away all the difficulties touching coincidences, and to have below it complete maps of all the solar elements with their long and short lines. This map is incomplete at present, but is making rapid progress.

A preliminary search for elements supposed not to be in the sun has also been commenced.

Of the above-named researches the subsequent parts of the paper refer to:—

I. The experiments made on a possible quantitative spectrum-analysis.

II. The method of photographing spectra adopted.

III The coincidences of spectrum lines.

IV. The preliminary inquiry into the existence in the sun of elements not previously traced.

I. *The Experiments made on a possible quantitative Spectrum-Analysis*

After the two former papers were sent in to the Royal Society, an investigation of the general changes undergone by spectra given by alloys was commenced.

A micrometer eye-piece was mounted on the observing-telescope of the spectroscope. With this the following phemonena were observed:—

I. The lines which remained varied their length as the percentage of the elements to which they were due varied.

II. Some of the lines appreciably varied their thickness or brightness, or both in the same way.

III. In cases where the brightness of a line was estimated through a considerable range of percentage composition by comparison with an air-line, the air-line was observed to grow faint and then disappear as the lightness of the metallic lines increased.

IV. In cases where the brightness or thickness of the line of one element was estimated by comparison with the line adjacent of the other constituent of the alloy, the point of equal brightness was observed to ascend or descend; this method was used to avoid the uncertainty of micrometric measurements of the tips of the lines in consequence of their variation in length due to the unequal action of the spark.

V. In some cases where the percentage of a constituent was so small that none of its lines were visible, there yet seemed to be an effect produced on the vapour of the opposite pole.

As these conclusions were derived from coarse alloys, and it was desirable to observe the effect of very fine gradation, Mr. C. Freemantle, the Deputy Master of the Mint, was begged to allow observations to be made on the gold-copper and silver-copper coinage alloys, and he immediately responded most cordially to the request.

Examples of the behaviour of some coarse alloys of silver and lead are given; they were irregular in their action, but it was observed that silver lines remained in the alloy as long as from ·05 to ·02 per cent. of silver was present. The alloys, however, were very unequal. Experiments on cadmium and tin alloys are described, the cadmium forming 10, 5, 1·0, 0·15 per cent In the last but one cadmium line was permanent; in the first at least five were seen. In an alloy of 0·099 per cent. of cadmium with a mixture of lead, tin, and zinc constituting the rest of the alloy, the behaviour of the cadmium lines was sensibly the same as in a mixture of 0·1 per cent. of cadmium and 99·9 of tin.

In the Mint-specimens the same phenomena were observed *en petit*, as the coarser alloys showed *en grand*. In a gold-copper alloy $\frac{1}{1000}$ increase in the gold made the lines shorter, and a similar increase in the copper made them longer.

In the silver-copper alloy an increase of $\frac{1}{1000}$ in the silver lengthened the lines, a similar increase in the copper shortened them.

These phenomena can be explained by assuming such alloys to be different physical things, and that the spark acts upon the alloy as a whole as well as upon each vapour separately.

Thus in these Mint alloys, copper is common to both, and their melting-points are:—

Gold . . .	1200° (Pouillet).
Copper . .	1200° to 1000°, the precise point not determined.
Silver . .	1000° (Pouillet).

The intermediate position of copper explains the different action on its lines of gold and silver.

II. *The Method of photographing Spectra adopted*

A camera carrying a 5 × 5-inch plate and a 3-inch lens of 23 in. focus, replaced the observing-telescope of the spectroscope. The lens focused from 3900 to 4500 very fairly upon the plate. The beam passing through collimator and prisms was, as in Mr. Rutherford's researches, very small. As the electric arc in its usual vertical position gave all the lines from pole to pole, the lamp was placed on its side, and the arc used in a horizontal position, the slit being vertical. The dense core of the arc then gave all the short lines in the centre of the field, the longer ones extending beyond them on either side. In order to obtain a scale, it was resolved to photograph the solar spectrum immediately adjacent to the metallic spectrum under examination.

To effect this a portion of the slit was covered up while the solar spectrum passed through the free part, and then the part used for the solar spectrum was covered, while the formerly covered part was opened for the metallic spectrum. This was effected by a shutter, with an opening sliding in front of the slit; a diagram of its action and form is given.

The arrangement of the spectroscope, heliostat, &c., for obtaining the sun's light is described. The image of the sun was brought to a focus between the poles of the lamp by an extra lens interposed between the lamp and the heliostat.

The use of the shutter enables us to compare either two or more spectra upon a single plate, or the solar spectrum may be compared with two metallic spectra, being made to occupy the position between the two.

III. *On the Lines coincident in different Spectra*

The bearing of the former papers on the lengths of the lines of the elements is briefly recapitulated.

The examination of the various spectra of metals and alloys indicated the great impurity of most of the metals used, and suggested the possibility of the coincidences observed by Thalén and others being explained in the light of former work.

It is observed that coincidences are particularly numerous in the spectra of iron titanium, and calcium, and that nearly every other solar metallic spectrum has one or more lines coincident with lines of the last element. These coincident lines are, as a rule, very variable in length and intensity in various specimens of the metals in which they occur, and are sometimes altogether absent.

One of the longest calcium lines, that at wave-length 4226·3, is also seen in the strontium spectrum as a line of medium length, and 4607·5, a very long line in strontium, appears in calcium as a short line. Another very long strontium line, 4215·3, is asserted by Thalén to be seen in calcium; but the author has never seen it till lately, and *then only in a specimen of calcium known to contain strontium.*

We have here, then, a case of coincident lines, in which the one that is long and bright in one spectrum is short and faint in the other, and a case of a line said to be coincident in two spectra being, though always visible in one, sometimes absent in the other of them, and only appearing in it when the two substances were mixed. The hypothesis of impurity at once explains the whole case, even without the third line, which renders the fact of mixture certain.

The longest lines of calcium occur in iron, cobalt, nickel, barium, strontium, &c., and the longest lines of iron occur in calcium, strontium, barium, and other metals.

Other cases are adduced, and the following general statements are hazarded, with a premise that further inquiry may modify them.

1. If the coincident lines of the metals be considered, those cases are rare in which the lines are of the first order of length in all the spectra to which they are common: those cases are much more frequent in which they are long in one spectrum and shorter in the others.

2. As a rule, in the instances of those lines of iron, cobalt, nickel, chromium, and manganese which are coincident with lines of calcium, the calcium lines are long, while the lines as they appear in the spectra of the other metals are shorter than the longest lines of those metals. Hence we are justified in assuming that short lines of iron, cobalt, nickel, chromium, and manganese, coincident with long and strong lines of calcium, are really due to traces of the latter metal occurring in the former as an impurity.

3. In cases of coincidences of lines found between various spectra the line may be fairly assumed to belong to that one in which it is longest and brightest.

A description of some photographs of spectra is then given, a photograph of the coincident lines of calcium and strontium being amongst them, and proving that strontium occurs in the sun; and the section concludes with a brief description of the method employed in making the new map, showing lengths and thicknesses, and enumerating coincident lines. This is done thus: papers are pasted on to photographs of the solar spectrum on glass; the lengths of the lines of the metallic spectrum under examination (*e.g.* that of iron) are marked on this paper in prolongation of the solar lines to which they correspond. They are then copied upon a map, and another piece of paper being fixed down, another spectrum is proceeded with in the same way.

IV. *The Preliminary Inquiry into the Existence of Elements in the Sun not previously traced*

The previous researches having shown that the former test for the presence or absence of a metal in the sun, namely, the presence or absence of its brightest or strongest lines in the average solar spectrum, was not conclusive, a preliminary search for other metals was determined on; and as a guide, Mr. R. J. Friswell was requested to prepare two lists, showing broadly the chief chemical characteristics of the elements traced and not traced in the sun.

The tables showed that in the main those metals which had been traced formed stable compounds with oxygen.

The author therefore determined to search for the metals which formed strong oxides, but which had not yet been traced.

The result up to the present time has been that *strontium*, *cadmium*, *lead*, *cerium*, and *uranium* would seem with considerable probability to exist in the solar reversing layer. Should the presence of *cerium* and *uranium* be subsequently confirmed, the whole of the iron group of metals will thus have been found in the sun.

Certain metals forming unstable oxides, such as gold, silver, mercury, &c., were sought for and not found. The same was the case when chlorine, bromine, iodine, &c., were sought by means of their lines produced in tubes by the jar-spark. These elements are distinguishable as a group by forming compounds with hydrogen.

It is observed that certain elementary and compound gases effect their principal absorption in the most refrangible part of the spectrum when they are rare, and that as they become dense the absorption approaches the less refrangible end; that the spectra of compounds are banded or columnar, the bands or columns lying at the red end of the spectrum; that the absorption spectra of chlorine, iodine, bromine, &c., are columnar, and that these are broken up by the spark just as the band spectra of compounds are broken up: and that it is probable that no compounds exist in the sun. The following facts, gathered from the work already accomplished by Rutherford and Secchi are stated:—

There are three classes of stars:—

1. Those like Sirius, the brightest (and therefore hottest?) star in the northern sky, their spectra showing only hydrogen lines very thick, and metallic lines exceedingly thin.

2. A class of stars with a spectrum differing only in degree from those of the class of Sirius, and to this our sun belongs.

3. A class of stars with columnar or banded spectra indicating the formation of compounds.

The question is asked whether all the above facts cannot be grouped together in a working hypothesis, which assumes that in the reversing layers of the sun and stars various degrees of "celestial dissociation" are at work which prevents the coming together of the atoms which, at the temperature of the earth, and at all artificial temperatures yet attained here, form the metals, the metalloids, and compounds.

In other words, the metalloids are regarded as *quasi* compound bodies when in the state in which we know them; and it is supposed that in the sun the temperature is too great to permit them to exist in that state in the reversing layer, though they may be found at the outer portions of the chromosphere or in the corona.

It is suggested that if this hypothesis should gain strength from subsequent work, stony meteorites will represent the third class of metalloidal or compound stars, and iron meteorites the other, or metallic stars.

The paper concludes as follows:—

"An interesting physical speculation connected with this working hypothesis is the effect on the period of duration of a star's heat which would be brought about by assuming that the original atoms of which a star is composed are possessed of the increased potential energy of combination which this hypothesis endows them with. From the earliest phase of a star's life the dissipation of energy would, as it were, bring into play a new supply of heat, and so prolong the star's life.

"May it not also be, if chemists take up this question, which has arisen from the spectroscopic evidence of what I have before termed the plasticity of the molecules of the metalloids taken as a whole, that much of the power of variation which is at present accorded to metals may be traced home to the metalloids? need only refer to the fact that, so far as I can learn, all so called changes of atomicity take place when metalloids are involved, and not when the metals alone are in question.

"As instances of these, I may refer to the triatomic combinations formed with chlorine, oxygen, sulphur, &c. in the case of tetrad or hexad metals. May not this be explained by the plasticity of the metalloids in question?"

"May we not from these ideas be justified in defining a metal, provisionally, as a substance the absorption spectrum of which is generally the same as the radiation spectrum, while the metalloids are substances the absorption spectrum of which, generally, is not the same?

"In other words, in passing from a hot to a comparatively cold state, the plasticity of these latter comes into play, and we get a new molecular arrangement. Hence are we not justified in asking whether the change from oxygen to ozone is but a type of what takes place in all metalloids?"

Abstract of paper "On the Quantitative Analysis of certain Alloys by means of the Spectroscope," by J. Norman Lockyer, F.R.S., and William Chandler Roberts, Chemist of the Mint.

The authors, after referring to experiments which showed clearly that the spectroscope might be employed to detect minute differences in the composition of certain alloys, proceed to give an account of the researches which they had instituted with a view to ascertain the degree of accuracy of which the method is capable.

The image of an electric-spark passing between the unknown alloy and a fixed electrode being thrown by means of a lens on the slit of the spectroscope, the phenomena observed were found to vary with the composition of the alloys; and further, by arranging them together with known check-pieces on a suitable stand, and bringing them in turn under the fixed electrode, the composition of the unknown alloys was determined by comparison with the known check-pieces.

The shape of the electrode ultimately adopted was stated; the pieces were held in their places by suitable metallic clips. Special attention was then directed to the adjustment of the length of the spark, which was found to materially influence the phenomena. The method adopted consisted in placing the variable electrode in the field of a fixed microscope having a 3- or 4-inch objective, and adjusting the summit of this electrode to coincide with the spider lines of the eye-piece.

After a series of experiments on alloys of zinc and cadmium of various compositions, the results of which were shown on a curve, more extended trials were made with the gold-copper alloy employed in coinage, which was peculiarly suited to these researches in consequence of the known method of assay having been brought to so high a state of perfection (the composition being determined with accuracy to the $\frac{1}{10000}$ part of the original assay-piece of about 7 grains), and from the fact that reliance can be placed on its homogeneity. The paper is accompanied by a series of four curves, which show the results of experiments, and in which the coördinates are given by the ordinary method of assay, and by the spectroscopic readings.

The chief practical advantage which appeared to flow from this inquiry was that, if it were possible to replace the parting assay by the spectroscopical method, a great saving of time in ascertaining the value of gold bullion would be effected.

Institution of Civil Engineers, Dec. 9.—T. Hawksley, president, in the chair.—"On the Geological Conditions affecting the Constructing of a Tunnel between England and France," by Mr. Joseph Prestwich, F.R.S. The author reviewed the geological conditions of all the strata between Harwich and Hastings on one side of the Channel, and between Ostend and St. Valery on the other side, with a view to serve as data for any future projects of tunnelling, and to show in what directions inquiries should be made. The points considered were the lithological characters, dimensions, range and probable depth of the several formations. The London clay, at the mouth of the Thames, was from 200 feet to 400 feet thick, while under Calais it was only 10 feet, at Dunkirk it exceeded 264 feet, and at Ostend it was 448 feet thick. He considered that a trough of London clay from 300 feet to 400 feet, or more, in thickness extended from the coast of Essex to the coast of France, and, judging from the experience gained in the Tower Subway, and the known impermeability and homogeneity of this formation, he saw no difficulty, from a merely geological point of view, in the construction of a tunnel, but for the extreme distance—the nearest suitable points being 80 miles apart. The lower Tertiary strata were too unimportant and too permeable for tunnel work. The chalk in this area was from 400 feet to 1,000 feet thick; the upper beds were soft and permeable, but the lower beds were so argillaceous and compact as to be comparatively impermeable. In fact, in the Hainaut coal fields they effectually shut out the water of the water-bearing tertiary strata from the underlying coal measures. Still, the author did not consider even the lower chalk suited for tunnel work, owing to its liability to fissures, imperfect impermeability, and exposure in the Channel. The gault was homogeneous and impermeable, but near Folkstone it was only 130 ft. thick reduced to 40 ft. at Wissant, so that a tunnel would hardly be feasible. The Lower Greensands, 260 ft thick at Sandgate, thinned off to 50 ft. or 60 ft. at Wissant, and were all far too permeable for any tunnel work. Again, the Wealden strata, 1,200 ft. thick in Kent, were reduced to a few unimportant rubbly beds in the Boulonnais. To the Portland beds the same objections existed as to the Lower Greensands, both were water-bearing strata. The Kimmeridge clay was 360 ft. thick near Boulogne, and no doubt passed under the Channel, but in Kent it was covered by so great a thickness of Wealden strata as to be almost inaccessible; at the same time it contained subordinate water-bearing beds. Still, the author was of opinion that, in case of the not improbable denudation of the Portland beds, it might be questionable to carry a tunnel in by the Kimmeridge clay on the French coast, and out by the Wealden beds on the English coast. The oolitic series presented conditions still less favourable, and the lower beds had been found to be water-bearing in a deep artesian well recently sunk near Boulogne. The experimental deep-boring now in progress near Battle would throw much light on this part of the question. The author then passed on to the consideration of the Palæozoic series, to which his attention was more particularly directed while making investigations, as a member of the Royal Coal Commission, on the probable range of the coal measures under the south-east of England. He showed that these rocks, which consisted of hard Silurian slates, Devonian and carboniferous limestone and coal measures, together 12,000 ft. to 15,000 ft. thick, passed under the chalk in the North of France, outcropped in the Boulonnais, were again lost under newer formations near to the coast, and did not reappear until the neighbourhood of Frome and Wells was reached. But, although not exposed on the surface, they had been encountered at a depth of 1,032 ft. at Calais, 985 ft. at Ostend, 1,026 ft. at Harwich, and 1,114 ft. in London. They thus seemed to form a subterranean table land of old rocks, covered immediately by the chalk and Tertiary strata. It was only as the southern flank of this old ridge that the Jurassic and Wealden series set in, and beneath these the Palæozoic rocks rapidly descended to great depths. Near Boulogne these strata were already 1,000 ft. thick; and at Hythe the author estimated their thickness might be that or more. Supposing the strike of the coal measures and the other Palæozoic rocks to be prolonged from their exposed area in the Boulonnais across the Channel, they would pass under the Cretaceous strata somewhere in the neighbourhood of Folkestone, at a depth estimated by the author at about 300 ft., and near Dover at about 600 ft., or nearly at the depth at which they had been found under the chalk at Guines, near Calais, where they were 665 ft. deep. These Palæozoic strata were tilted at high angles, and on the original elevated area they were covered by horizontal Cretaceous strata, the basement beds of which had filled up the interstices of the older rocks as though with a liquid grouting. The overlying mass of gault and lower chalk also formed a barrier to the passage of water so effectual, that the coal measures were worked without difficulty under the very permeable Tertiary and upper chalk of the North of France; and in the neighbourhood of Mons, notwithstanding a thickness of from 500 ft. to 900 ft. of strata charged with water, the lower chalk shut the water out so effectually that the coal measures were worked in perfect safety, and were found to be perfectly dry under 1,200 ft. of these strata combined. No part of the Straits exceeded 186 ft. in depth. The author, therefore, considered that it would be perfectly practicable, so far as safety from the influx of the sea water was concerned, to drive a tunnel through the Palæozoic rocks under the Channel between Blanc Nez and Dover, and he stated that galleries had actually been carried in coal, under less favourable circumstances, for two miles under the sea near Whitehaven. But while in the case of the London clay the distance seemed almost an insurmountable bar, here again the depth offered a formidable difficulty. As a collateral object to be attained, the author pointed to the great problem of the range of the coal measures from the neighbourhood of Calais in the direction of East Kent, which a tunnel in the Palæozoic strata would help to solve. These were, according to the author, the main conditions which bore on the construction of a submarine tunnel between England and France. He was satisfied that on geological grounds alone, it was in one case perfectly practicable, and in one or two others it was possibly so; but there were other considerations besides those of a geological nature, and whether or not they admitted of so favourable a solution was questionable. In any case, the author would suggest that, the one favourable solution admitted, it might be desirable, in a question involving so many and such great interests, not to accept an adverse verdict without giving all those considerations the attention and deliberation which the importance of the subject deserved. Granting the possibility of the work in a geological point of view, there were great and formidable engineering difficulties; but the vast progress made in engineering science during the last half century, led the author to imagine that they would not prove insurmountable, if the necessity for such a work were to arise, and the cost were not a bar.

Royal Astronomical Society, Dec. 14.—Prof. Cayley, president, in the chair.—Prof. Pritchard gave a verbal account of the Physical Observatory about to be established at Oxford. He said that the University authorities had been induced to grant a site for a physical observatory in the noble park of sixty acres, which they had recently thrown open to the public. He had been anxious that such a site should not be disgraced by an unsightly building such as observatories usually were. He found himself fortunately situated in having amongst his old pupils Mr. Barry, the well-known architect, who had furnished them with a design which he showed to the meeting, and had devised, amongst other things, a dome with a fine broad shutter, which he trusted would be really ornamental as well as useful. There would be a central tower of three rooms, one above the other; the basement room would be used for storage; above would be the professor's room; and in the floor above that would be mounted the noble reflector which had been presented to the University by Dr. De La Rue. In a side wing there would be a transit instrument to be used for educational purposes, and another telescope which he hoped would be well worked by members of the University. Mr. Barry informed the society that their new rooms at Burlington House would probably be ready by the middle of April.—Capt. Noble mentioned to the society that in the new volume of the *Nautical Almanac* for 1877 tables of Uranus were given, but it was no credit to England that we should have been kept waiting for them until they were presented to us from across the Atlantic by the labour of Prof. Simon Newcomb.

Entomological Society, Dec. 1.—H. T. Stainton, F.L.S., vice-president, in the chair.—Mr. Bond exhibited a hybrid specimen between *Clostera curtula* and *C. reclusa* partaking of the characters of both parents.—Mr. Jenner Weir exhibited specimens of a minute Hymenopterous insect (a species of *Psen*), which he had observed in large numbers (probably 150) in June last on a pear leaf at Lewes. They had congregated together on the surface of the leaf like a swarm of bees, though it was not apparent what motive brought them together.—Mr. Dunning read extracts from a letter from New Zealand stating that the red clover had been introduced into that colony, but that they had no humble bees to fertilise the plant. Also that certain Lepidopterous insects had been accidentally imported into the islands, but that the corresponding Ichneumon flies were wanted to keep down their numbers. It was suggested that the nests of humble bees might be imported, when the bees were in a dormant condition, keeping them in that state (by means of ice) during the voyage.—Mr. Baly communicated a paper on the Phytophagous Coleoptera of Japan, being a continuation of a former paper on the same subject.—Mr. Bates communicated a supplementary paper on the Longicorn Beetles recently brought from Chontales, Nicaragua, by Mr Thomas Belt.—Mr. W. H. Miskin, of Queensland, communicated criticisms on Mr. Masters' Catalogue of the described species of Diurnal Lepidoptera of Australia.—A fourth portion of the catalogue of British Insects, now being published by the society, was on the table. It contained the Hymenoptera (*Oxyura*), by Rev. T. A. Marshall, M.A.

PARIS

Academy of Sciences, Dec. 8.—M. de Quatrefages, president, in the chair.—The president announced the death of M. Cl. Gay, member of the Botanical Section; and the Perpetual Secretary also announced the death of the well-known mineralogist, C. F. Naumann, Corresponding Member of the Mineralogical Section.—The following papers were read:—An answer to M. Pasteur's paper on the origin of beer yeast, by M. A. Trécul. The author contradicted M. Pasteur's statement that the development of *Penicillium glaucum* from *putrid* yeast was an admitted fact. On the contrary, it had been observed to develop itself from perfectly healthy yeast.—On the vitreous substances found included in Santorin lava, by M. F. Fouqué.—On the determination of the ratio of two specific heats by the compression of a limited volume of gas, by M. E. H. Amagat.—On the distribution of the neolithic populations in the department of the Oise, by M. R. Guérin.—On the habits of the *Phylloxera* (continued), by M. Max. Cornu.—A further notice on the connection of storms and sunspots as observed at Paris and Fécamp was received from M. Poey.—Preliminary note on the elements existing in the sun, by Mr. Norman Lockyer. M. Berthelot then criticised the paper. He held that the phenomena of specific heat, &c., indicated that the elements, so-called, were on a very different basis from the compounds, and that the phenomena they presented in this respect could not be explained if they were not regarded as actually simple bodies. M. Dumas thought that, as he had himself maintained before the Academy, elements ought only to be regarded as elements in relation to human experience and not as absolute elements, a fact which he considered Lavoisier to have established. He considered that modern experiments tended to confirm this opinion.—Note on the identity of Cauchy's formulæ for the determination of the conditions of convergence of Lagrange's series with those given by Lagrange himself, by M. L. F. Ménabréa.—On the November meteors, by M. Wolf.—Note on Faye's periodic comet and on the discovery and observations of twenty nebulæ made at the Marseilles observatory, by M. E. Stephan.—On the movement of an elastic wire one end of which has a vibratory motion, by M. E. Mercadier.—Observations on the action of certain poisons on sea fish, by MM. A. Rabuteau and F. Papillon.—On the embryo cell of the egg of osseous fish, by M. Balbiani.—On the age of the dental follicle in the *mammifera*, by MM. E. Magitot and Ch. Legros.—On the use of electrical cauterisation in surgical operations, by MM. Ch. Legros and Onimus.—On the Ostreacious marl of Fresnes-lès-Rungis (Seine), by M. Stan. Meunier.—Note on a meteor observed at Versailles on Dec. 3, by M. Martin de Brettes.—New analysis of the water of St. Thiébaut's fountain at Nancy, by M. P. Guyot.—Studies on certain combustibles from the basin of Donetz and Toula, Russia, by MM. Scheurer-Kestner and Meunier-Dollfus.

BOOKS RECEIVED

ENGLISH.—Guide to Latin Prose: R. M. Millington (Relfe).—Wild Animals: Wolf (Macmillan & Co.)—Problems of Life and Mind: George Henry Lewes (Trübner & Co)—Theory of Attraction 2 vols.: Todhunter (Macmillan & Co)—The Borderland of Science: R. A. Proctor (Smith, Elder & Co)—Memoir of Mary Somerville: Martha Somerville (John Murray)—Manual of Comparative Anatomy and Physiology: J. M. Bradley (Simpkin and Marshall)—The River Amazon: H W. Bates (John Murray)—The Chase: Somerville (W. Tegg).—Virgil's Eclogues. Translated: Millington (Longmans).—Quantitative Chemical Analysis. 6th edition: Fresenius (Churchill)—Nautical Almanac, 1877 (John Murray)—The Simplicity of Life: Dr. Ralph Richardson (H K. Lewis)—Introduction to Quaternions: Kelland and Tait (Macmillan)—Free-thinking and Plain Speaking: Leslie Stephens (Longmans).—United States Geological Survey. 6th Annual Report. F. W. Haydyn (Trübner & Co)—Harvest of the Sea: Bertram (John Murray).—Mountain, Meadow, and Mere: G. C. Davies (H. S King & Co.)—Legal Handbook for Architects. Jenkins and Raymond (H. S. King & Co.)—The Conservation of Energy: Balfour Stewart (H. S. King & Co)—Telegraphic Journal, vol i. (Gillman)—Primer of Geology: A Geikie (Macmillan & Co.)—Darwinism and Design: G. St Claire (Hodder & Stoughton).—From January to December (Longmans).—Pheasants for Coverts and Aviaries: W. B. Tegetmeier (Horace Cox).

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THURSDAY, DECEMBER 25, 1873

QUATERNIONS

A MATHEMATICIAN is one who endeavours to secure the greatest possible consistency in his thoughts and statements, by guiding the process of his reasoning into those well-worn tracks by which we pass from one relation among quantities to an equivalent relation. He who has kept his mind always in those paths which have never led him or anyone else to an inconsistent result, and has traversed them so often that the act of passage has become rather automatic than voluntary, is, and knows himself to be, an accomplished mathematician. The very important part played by calculation in modern mathematics and physics has led to the development of the popular idea of a mathematician as a calculator, far more expert, indeed, than any banker's clerk, but of course immeasurably inferior, both in resources and in accuracy, to what the "analytical engine" will be, if the late Mr. Babbage's design should ever be carried into execution.

But though much of the routine work of a mathematician is calculation, his proper work—that which constitutes him a mathematician—is the invention of methods. He is always inventing methods, some of them of no great value except for some purpose of his own; others, which shorten the labour of calculation, are eagerly adopted by all calculators. But the methods on which the mathematician is content to hang his reputation are generally those which he fancies will save him and all who come after him the labour of thinking about what has cost himself so much thought.

Now Quaternions, or the doctrine of Vectors, is a mathematical method, but it is a method of thinking, and not, at least for the present generation, a method of saving thought. It does not, like some more popular mathematical methods, encourage the hope that mathematicians may give their minds a holiday, by transferring all their work to their pens. It calls upon us at every step to form a mental image of the geometrical features represented by the symbols, so that in studying geometry by this method we have our minds engaged with geometrical ideas, and are not permitted to fancy ourselves geometers when we are only arithmeticians.

This demand for thought—for the continued construction of mental representations—is enough to account for the slow progress of the method among adult mathematicians. Two courses, however, are open to the cultivators of Quaternions: they may show how easily the principles of the method are acquired by those whose minds are still fresh, and in so doing they may prepare the way for the triumph of Quaternions in the next generation; or they may apply the method to those problems which the science of the day presents to us, and show how easily it arrives at those solutions which have been already expressed in ordinary mathematical language, and how it brings within our reach other problems, which the ordinary methods have hitherto abstained from attacking.

Sir W. R. Hamilton, when treating of the elements of the subject, was apt to become so fascinated by the metaphysical aspects of the method, that the mind of his disciple became impressed with the profundity, rather than the simplicity of his doctrines. Professors Kelland and Tait in the opening chapter (II.) of their recently published work* have, we think, successfully avoided this element of discouragement. They tell us at once what a vector is, and how to add vectors, and they do this in a way which is quite as intelligible to those who are just beginning to learn geometry as to the most expert mathematician.

The subject, like all other subjects, becomes more intricate as the student advances in it; but at the same time his ideas are becoming clearer and more firmly established as he works out the numerous examples and exercises which are placed before him.

The technical terms of the method—Scalar, Vector, Tensor, Versor—are introduced in their proper places, and their meaning is sufficiently illustrated to the beginner by the examples which he is expected to work out. The pride of the accomplished mathematician, however (for whom this book is not written), might have been somewhat mollified if somewhere in the book a few pages had been devoted to explaining to him the differences between the Quaternion methods and those which he has spent his life in mastering, and of which he has now become the slave. He is apt to be startled by finding that when one vector is multiplied into another at right angles to it, the product is still a vector, but at right angles to both. His only idea of a vector had been that of a line, and he had expected that when one vector was multiplied into another the result would be something of a different kind from a line, such, for instance, as a surface. Now if it had been pointed out to him in the chapter on vector multiplication that a surface is a vector, he would be saved from a painful mental shock, for a mathematician is as sensitive about "dimensions" as an English schoolboy is about "quantities."

The fact is, that even in the purely geometrical applications of the Quaternion method we meet with three different kinds of directed quantities: the vector proper, which represents transference from A to B; the area or "aperture," which is always understood to have a positive and a negative aspect, according to the direction in which it is swept out by the generating vector; and the versor, which represents turning round an axis.

The Quaternion ideas of these three quantities differ from the old ideas of the line, the surface, and the angle only by giving more prominence to the fact that each of them has a determinate *direction* as well as a determinate magnitude. When Euclid tells us to draw the line A B, he supposes it to be done by the motion of a point from A to B or from B to A. But when the line is once generated he makes no distinction between the results of these two operations, which, on Hamilton's system, are each the opposite of the other.

Surfaces also, according to Euclid, are generated by the motion of lines, so that the idea of motion is an old one, and we have only to take special note of the direction of the motion in order to raise Euclid's idea to the level of Hamilton's.

With respect to angles, Euclid appears to treat them as if they arose from the fortuitous concourse of right lines;

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* "Introduction to Quaternions, with numerous Examples" By P. Kelland, F R S, formerly Fellow of Queen's College, Cambridge, and P. G. Tait, formerly Fellow of St. Peter's College, Cambridge; Professors in the Department of Mathematics in the University of Edinburgh. (Macmillan, 1873.

but the unsatisfactory nature of this mode of treatment is shown by the fact that in all modern books on trigonometry an angle is represented as generated by motion round an axis in a definite direction.

There are thus three geometrical quantities having direction, and the more than magical power of the method of Quaternions resides in the spell by which these three orders of quantities are brought under the sway of the same system of operators.

The secret of this spell is twofold, and is symbolised by the vine-tendril and the mason's rule and square. The tendril of the vine teaches us the relation which must be maintained between the positive direction of translation along a line and the positive direction of rotation about that line. When we have not a vine-tendril to guide us, a corkscrew will do as well, or we may use a hop-tendril, provided we look at it not directly, but by reflexion in a mirror.

The mason's rule teaches us that the symbol, as written on paper, is not a real line, but a mere injunction, commanding us to measure out in a certain direction a vector of a length so many times that of the rule. Without the rule the symbol would have no definite meaning. Thus the rule is the unit of the Quaternion system, while the square *reminds* us that the right angle is the unit versor.

The doctrine of the unit is a necessary part of every exact science, but in Quaternions the application of the same operators to versors, vectors, and areas is utterly unintelligible without a clear understanding of the function of the unit in the science of measurement.

Whether, however, it is better to insinuate the true doctrine into the mind of the student by a graduated series of exercises, or to inculcate it upon him at once by dogmatic statements, is a question which can only be determined by the experience of a new generation, who shall have been born with the extraspatial unit ever present to their consciousness, and whose thoughts, guided by the vine-tendril along the Quaternion path, shall turn always to the right hand, and never to the left.

Prof. Kelland tells us in the preface to the work to which we have alluded that, whereas Sir W. R. Hamilton and Prof. Tait have written treatises on Quaternions for mathematicians, the time has come when it behoves some one to write for those who desire to become mathematicians. Whatever, therefore, advanced mathematicians may think of this book, they ought to reserve their judgment as to its difficulty till they have ascertained how it is assimilated by those for whom it is written—those in whom the desire to become mathematicians has not yet become alloyed with the consciousness that they are mathematicians. For while Prof. Kelland—as he has elsewhere told us—finds but little difficulty in teaching the elements of the doctrine of Vectors to his junior classes, Hamilton himself, the great master of the spell, when addressing mathematicians of established reputation, found, for his Quaternions, but few to praise and fewer still to love.

Prof. Kelland, by the clearness and orderliness of his statements, and by boldly getting rid of everything which is unnecessarily abstruse, has done more than any other man towards rendering the subject easy to the student, and reconciling even the case-hardened mathematician to the new method, as applied to geometrical questions of old-established truth.

The other aspect of Quaternions, as a method which every mathematician *must* learn in order to deal with the questions which the progress of physics brings every day into greater prominence, is hinted at by Prof. Tait in the last chapter of the book. He there introduces us to the linear and vector function of the first degree under its kinematical aspect of a homogeneous strain. The importance of functions of this kind may be gathered from the fact that a knowledge of their properties supplies the key to the theory of the stresses as well as the strains in solid bodies, and to that of the conduction of heat and electricity in bodies whose properties are different in different directions, to the phenomena exhibited by crystals in the magnetic field, to the thermo-electric properties of crystals, and to other sets of natural phenomena, one or more of which the scientific progress of every year brings before us.

But as we believe that Prof. Tait is about to bring out a new edition of his treatise on Quaternions, in which this higher aspect of the subject will be brought more prominently forward, we reserve our remarks on Quaternions as an instrument of physical research till we have the subject presented to us by Prof. Tait in a form which adequately represents its latest developments.

MARKHAM'S "UNKNOWN REGION"

The Threshold of the Unknown Region. By Clements R. Markham, C.B., F.R.S., Secretary of the Royal Geographical Society, formerly of H.M. Arctic ship *Assistance*. (London: Sampson Low and Co., 1873).

HE must be a sorry story-teller who manages to make a traveller's tale uninteresting, especially if the traveller be a voyager, and still more if his voyages have led him into unknown regions. Of all forms of narrative we think it will be generally acknowledged that narratives of discovery are by far the most popular, as is testified by the abundance of this kind of literature, historical and fictitious, provided for the delectation of the young. No doubt this may be largely accounted for by the fact that a discoverer of new lands is continually unveiling the unknown to those who listen to his tale, thereby appealing to one of the strongest and most fruitful characteristics of the human mind, that of curiosity. Every step taken by a discoverer, every knot sailed by his "good ship," we know will lead him among fresh wonders. Once upon a time the Unknown Region—that is, the region unknown to those peoples who have had a thirst for knowledge to any fruitful extent—was in sooth wide enough, when first our Aryan forefathers left their eastern home, and had "all the world before them where to choose." Even four centuries ago the greater part of the earth waited the coming of the European descendants of those primitive discoverers who first turned their faces eagerly and inquisitively to the unknown west. But ever since then the boundary of the Unknown Region has been gradually pushed farther and farther back, until now there remains comparatively little to be found out in order to enable geographers to complete the configuration of the lands of the globe. The extent of our dwelling-place is now pretty well known, though there is yet abundance of work for many generations of explorers ere the contents of land and water be anything like fully disclosed.

Of all narratives of discovery, those relating to Arctic regions bear, in our estimation, the palm for intensity of interest, and we are sure there are many who think along with us in this matter. It would be difficult to say briefly why this is so. It may be mainly that there the mystery of the unknown, so far as relates to the surface of our globe, is concentrated. No doubt, also, there is a weird fascination around those eerie, rugged, ice-bound regions of the far north, which have been the scene of a greater number of deeds of heroic daring for noble and disinterested purposes, than any other region of the globe of equal extent. There is also a general though perhaps vague, yet we believe, well-founded belief, that within these regions lie solutions to many of the yet mysterious problems of science; that if once all the phenomena that lie within the yet unlifted veil were exposed and understood, they would afford us the means of tracing with something like certainty the history of our earth through many geological ages. In more senses than one, we are there on the threshold of the unknown.

There is somehow not the same attractiveness about Antarctic exploration, though, as Dr. Neumayer has well shown, it is certainly calculated to yield valuable results to Science, and indeed has already done so. This may partly arise from the scarcity in these regions of land and of life of all kinds, which are abundant enough in certain regions of the known north. Indeed, the tract around which the interest of Arctic discovery is concentrated may be regarded as but a continuation of the great American Continent.

We are sure that all who read this immensely interesting volume of Mr. Markham's will agree with what we have said. No more attractive subject for a work exists than the history of Arctic discovery; no man knows this subject better than Mr. Markham; and few could have written a volume on the subject more full of interest and of valuable information clearly arranged than the one before us. The object of the volume, Mr. Markham tells us, "is to give the public a correct knowledge of the whole line of frontier separating the known from the unknown region round the North Pole, to recall the stories of early voyagers, to narrate the recent efforts of gallant adventurers of various nationalities to cross the threshold, to set forth the arguments in favour of a renewal of Arctic exploration by England, and to enumerate, in detail, the valuable and important results to be derived from North Polar discovery." Mr. Markham's main design is evidently to show that the only certain gateway to the Pole is by the Smith Sound route, and this design he accomplishes in a way that cannot fail to convince any unprejudiced reader, by going over the whole story of Arctic discovery from the time that that hardy Norseman Lief, the son of Eric the Red, in 1001, made his abortive discovery of North America, down to the present year, when the world was astounded by the news of the discoveries and adventures of the ill-equipped but remarkably successful *Polaris* expedition. One's blood is once more stirred by the story of these fearless early English and Dutch adventurers, Burrough and Pett and Jackman and Barentz and Hudson and others, who dared to face the dangers of Arctic navigation in mere "cock-boats" of 20 and 40 and 80 and 100 tons. The story of Barentz and his companions especially is told with considerable fulness, and it is with a very strange kind of feeling that one reads of the discovery, in 1871, of the very hut in which these stout-hearted Dutchmen passed the winter of 1596-7, and goes over the long catalogue of "Barentz relics" found therein.

Mr. Markham recounts the principal attempts that have been made to pierce through the formidable barrier of ice that guards the North Pole. "There are three approaches by sea to this land-girt end of the earth: through the wide ocean between Norway and Greenland, through Davis' Strait, and through Behring's Strait—one wide portal and two narrow gates." At present no one seems to think of attempting the last-mentioned route, advocates of Arctic exploration being divided between the Spitzbergen route, as the wide sea between Greenland and Novaya Zemlya is called, and the Smith Sound route, the route through the winding passages that lie between Greenland and the American continent. Mr. Markham, in considerable detail, recounts the various expeditions which, from the days of Barentz down to our own time, have charged the barrier that hems the Pole between the east coast of Greenland and Novaya Zemlya. He states with perfect fairness and with all necessary fulness the progress made by each expedition, and the invariable result, so far as the attempt to approach the Pole is concerned, has been failure. The highest latitude attained by this route was that reached by the well-equipped sledge expedition of Parry in 1827, 82° 45′ N.; but the difficulties which the expedition had to encounter were so stupendous as, when combined with what is known of the conditions which influence the movements of the pack in this direction, to utterly forbid any hope of attaining the desired goal by the Spitzbergen route. The inevitable conclusion to be derived from the many fruitless attempts which have hitherto been made by this route is, "that by the Spitzbergen route, in a bad season, nothing whatever can be done; and in a favourable season a steamer may possibly press one or two, or even more degrees farther north than has hitherto been reached, and obtain some valuable deep-sea soundings and temperatures, but no other scientific results in the absence of land. The Spitzbergen route cannot be recommended, because there is no sure prospect of exploring an extensive unknown area, and because no valuable results in geology, botany, ethnology, or geodesy could be obtained under any circumstances." On this point Arctic authorities are all but unanimous, as they are also on the point that by the Smith Sound route a well-equipped Government expedition, if sent out next spring, would be almost certain to return within three years with the mystery of the "Polynia" cleared up, and with results in nearly all departments of Science not only invaluable from a purely scientific point of view, but of the highest practical importance. The very last attempt that has been made by the Smith Sound route seems to us to prove triumphantly that it would at present be folly to attempt to reach the pole by any other route, and that if the meagrely equipped and badly disciplined *Polaris* expedition accomplished so much in a very few days, an expedition such as Government will, we hope, feel bound to send out, will be sure to accomplish the remaining 400 or 500 miles that lie between Hall's farthest north point and the Pole.

True, there are a few unhealthy croakers, as there always have been, and will be, we fear, for many generations to come, who ask What is the good of incurring so much danger and expense, for the mere gratification of curiosity, or, at best, to satisfy the wishes of a few men of science? But we feel confident that the great body of the English people will ask no such questions, but would hail with enthusiasm the decision of the Government to crown the glory which England has hitherto gained in Arctic exploration by sending out one more expedition whose task it would be to return with the long-sought-for secret in its keeping. It is beginning to dawn upon the ordinary English mind that, after all, the apparently unpractical researches of scientific men are frequently pregnant with results of the most important practical bearing on the welfare of the country and the race.

As for the element of danger, Mr. Markham convincingly shows by unimpeachable statistics, that the loss of men by the Smith Sound route, from causes connected with the climate and the peculiarities of the service, is almost incredibly small. One of the most distinguished medical officers who has served in the Arctic regions declares, that "of all seas visited by men-of-war the Arctic have proved the most healthy. . . . The risk by climate and disease which is run in a voyage to the Arctic seas—such as a Royal Expedition necessitates—is not greater than that which a ship like the *Challenger* will incur in her voyage of discovery." The dangers, or rather difficulties, which have to be faced are only such as brave men are eager to confront, and the service is one which our naval officers and men glory in in time of peace, and is certainly an infinitely better use to put them to than to keep them idling at home or on foreign stations. As to the question of expense, the article in a recent number will show that the less said by Government on this score the better.

All these and many other points in connection with Arctic exploration will be found fully and clearly discussed in Mr. Markham's volume, in which the invaluable results, scientific and practical, in nearly all departments of Science to be obtained from a Government Expedition are set forth with great fulness, clearness, and force. The volume concludes with an account of the interview that took place last year between the Arctic deputation and Mr. Lowe, the result of which was such as to give good grounds for expecting that this year Government will feel bound to organise an adequate expedition to leave our shores next spring to find its way to the Pole by the Smith Sound route.

The numerous maps by which the volume is illustrated are beautifully drawn, and are of the greatest assistance in enabling the reader to understand the interesting story of Arctic discovery so well told by Mr. Markham. As a mere story the work is a masterly one; and if anyone wants to know within short space what has already been done in the discovery of the Arctic regions, what still remains to be done, and what results are to be expected from further exploration, he could not do better than read Mr. Markham's "Threshold of the Unknown Region."

OUR BOOK SHELF

Annual Record of Science and Industry for 1872; edited by Spencer F. Baird, with the assistance of eminent men of Science. (New York: Harper and Brothers, 1873.)

THE praise which we were able to bestow on the first of Prof. Baird's Annual Records, that for 1871, can be fully repeated with regard to its successor. The only method of "reviewing" a work of this kind, is to refer in general terms to its scope, and to the degree to which the compiler appears to have fulfilled the promises of his programme. On these points we can speak in the most favourable terms. As far as a cursory glance through the pages of the volume enables us to speak, we believe that purchasers of the book will find it a most useful addition to their library shelves. The paragraphs refer to the most noteworthy additions to scientific knowledge or observation made during the year, and have been compiled with commendable terseness and perspicuity from a large range of English, American, and Continental sources. A carefully *raisonné* table of contents, and an alphabetical index, will enable the student to turn without difficulty to any desired subject. Although absolute freedom from errors, typical and otherwise, can hardly be expected in a work with so large a scope, the American "Record" contrasts most favourably in this respect with some similar volumes published in this country. We do not know where to find a more complete record of the science of the year; and we shall hope to see a long series of these useful volumes.

The Borderland of Science. By Richard A. Proctor. (London: Smith, Elder, and Co. 1873.)

THESE Essays are reprinted from the *Cornhill Magazine.* The titles are as follows:—"The Herschels and the Star-Depths;" "A Voyage to the Sun;" "A Voyage to the Ringed Planet;" "A Giant Planet;" "Life in Mars;" "A Whewellite Essay on the Planet Mars;" "Meteors—Seed-bearing, and otherwise;" "A Recent Star-shower, and Star-showers generally;" "News from the Moon;" "Earthquakes;" "The Antarctic Regions:" "A Few Words about Coal;" "Notes on Flying and Flying-Machines;" "Gambling Superstitions;" "Coincidences and Superstitions;" "Notes on Ghosts and Goblins."

Sommario delle Lezioni di Fisica, date dal Professore Enrico Dal Pozzo di Mombello, nella Libera Universita di Perugia. (Foligno: Pietro Spariglia, 1873.)

GANOT'S Treatise on Physics has been translated into Italian and is no doubt largely used in the country; also in 1870 Prof. Cantoni, of Milan, published a course of Physical Lectures. The work before us by Prof. Dal Pozzo is to some extent based upon that of Cantoni; it is a summary of two courses of lectures delivered in the free University of Perugia. The University (founded in 1307) is one of the oldest in Europe, and possesses a good library, botanical gardens, and mineralogical collections. We cannot at this moment call to mind any scientific associations connected with the place, as with Pisa, Bologna, and Pavia. The town itself has been mentioned any time for two and twenty centuries, and it is a noted school of music.

We can scarcely judge of the science of Perugia from the work before us. The students must be very clear-headed men if they can follow Prof. Pozzo's arrangement. It is certainly most novel. It may have its advantages. He begins cleverly enough with an account of the "Energy of the Universe," embracing some general properties of bodies, actual and potential energy, conservation and dissipation of energy. The author uses the terms *forza attiva* and *forza di positione*, in place of our more usual terms. We are glad to find him acquainted with the works of Thomson, Balfour Stewart and Rankine. The lecture on Energy is followed by one on the Dynamical Theory; which embraces to some extent the relations of different forces, and the "varied modality" of chemical, thermical, and electrical action. The next lecture relates to Molecular Dynamics. Then in succession :—Electromotive force produced by Chemical Action, by Heat, by mechanical means, and by Induction. Mutual action of Currents and Magnets, Terrestrial Magnetism, Polar Auroræ; Atmospheric Electricity; Diamagnetism; Rhumkorff's Coil; Winds; Marine Currents; the Sun; the Doctrine of La Place; the Doctrine of Lyell; Thermogenesis; Atmolysis and Osmosis; Capillarity; the Doctrine of Mayer. The second course treats of electricity, undulations, sonority, musical timbre, echoes, photometry, dispersion and the spectroscope, chromatism, vision, luminous undulations, diffraction, polarisation, radiant heat, action of electricity on organic bodies, the muscular current, electrical nervous phenomena, electrical fishes.'

The arrangement is really wonderful. What can possibly warrant the following order for lectures :—diamagnetism, Rhumkorff's coil, winds, marine currents; or again—thermogenesis, atmolysis, capillarity? One lecture ends with "Che cosí mirabilmente si svolgono dall' evoluzione Darwinianæ;" and the next commences "E impossible proseguire un corso di Fisica e piu ancora quella parte, che tratta delle azioni senza prima definire le parole, atomo, molecula." The Prof. Pozzo can scarcely be expected to lecture on all science: to pass from the sun to an atom, from Darwinism to electro-dynamics, from geology to elliptical polarisation. If he is, the system is a bad one, and his students may get a smattering of many things, and know nothing well. Mechanical philosophy seems to be almost ignored. The book is devoid of mathematics, and without woodcuts; and we imagine the youth of Perugia must yawn over it; and, if the lectures are as dry as the book, spend much of the time which ought to be given to physics in saying "felicissima notte" to each other. G. F. R.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Proposed Alterations in the Medical Curriculum

THE remarks made in your number of December 18 by my friend Prof. Balfour are founded on the mistake he has made in supposing that it is proposed to abolish the regulation requiring attendance on the courses of lectures on Botany and Zoology. There is no question raised between mere examining boards and teaching institutions, between compulsory and optional attendance on professors' lectures. It is simply that the candidate for medical degrees be allowed to take the examination in Zoology and Botany earlier than is at present permitted. At present the examination in these subjects in Edinburgh University is fixed by ordinance at the end of the second of the four years of medical study, and in this University, while the Botany comes at that time, the Zoology is actually not till the end of the third year, so that our case is even worse than that of Edinburgh. Prof. Balfour says, "The student might be encouraged to take his science examination at an early period of his curriculum, say at the end of his first year of study." That is exactly the result practically aimed at here, and I am quite at one with him on the subject. But why prevent the student from taking the examination in Botany and Zoology before entering on his medical curriculum proper, if he has attended the professor's class and is ready for it? Very few would at present do so, as it would imply a preliminary year of attendance at the Universities to obtain the courses of Zoology and Botany. But is it not a very desirable thing, from every point of view, to encourage this? So far from lowering the standard in these subjects, or promoting cramming, it would do exactly the reverse. It would enable real study to take the place of the cramming which is inevitable when these subjects are left over to be mixed up with medical studies proper.

For some time there has been a strong feeling here that the examination in Zoology and Botany should take place not later than the end of the first year, and the Lord Rector of our University in taking this matter up, instead of tinkering as to particular dates, has announced the sound general principle that the student should be encouraged to take the subjects of Botany and Zoology before beginning his medical curriculum proper, with the view both of promoting a more real study of these sciences, and of clearing the subsequent medical curriculum for a more real study of the subjects which belong to it. I see nothing in the resolutions which our distinguished Lord Rector has laid before the University Court either suggesting or implying abolition of compulsory attendance on the professors' courses of Zoology and Botany, and Prof. Balfour might well have taken it for granted that the mere fact of the proposal emanating from Prof. Huxley is security enough that the object could not possibly be to lower the position of the natural sciences or to promote cramming instead of real study. Our Lord Rector has as yet only intimated his resolutions, but when the oracle speaks we shall no doubt hear such good reasons for them that even so enthusiastic a botanist as Prof. Balfour will have his alarm turned into joy.

Will any of those who are so strong on the point of compulsory attendance on courses of Zoology and Botany tell us why they do not say a word for Natural Philosophy? Including such subjects as heat, light, electricity, hydrostatics, pneumatics, optics, acoustics, it is surely of more importance than either of the other two, whether regarded educationally or in its bearing on modern medicine. Yet in the Scotch Universities there is no compulsion to attend a course of lectures on Natural Philosophy, and it is relegated to the preliminary examination in general education. The day is past for laying on additional compulsory courses of lectures, but it is surely not too much to say that the student might be allowed to profess and be examined in Natural Philosophy instead of one of the other two.

Aberdeen University, Dec. 20 JOHN STRUTHERS

The Distribution of Volcanoes

SOME of the correspondence in your paper has latterly been so caustic, that timid people may be pardoned for shrinking from writing letters which bring down upon them the hammers of scorn and contempt so vigorously.

Notwithstanding this, the discussion between Mr. Mallet and Dr. Forbes about volcanoes tempts me to write to you on a side issue of that controversy in which I have been interested for some time. What I have to say may not be new, although I believe it to be so. At all events it is not commented upon in the books accessible to me. I will premise that, caring little for laurels of any kind but a good deal for instruction, that if it be discovered that what I say is stale and old, I hope I may be treated as an ignorant scholar, willing to learn, and not as a rival to be crushed, and further, that my results having been obtained independently, they support and make more sure the position of my predecessors.

You were good enough, some months ago, to print some letters of mine on the current elevation of the circumpolar regions of the earth. I have since accumulated much new matter on this subject, which will be shortly published in part in the Journal of the Geographical Society. The general result of my inquiry is, that all the large land surfaces of the earth, the large continental and insular surfaces, are more or less in process of gradual or rapid elevation. There are a few small areas of depression on the outskirts and borders of the great land-masses, but these are very local and unimportant. And with this slight exception the continents of North and South America, Asia, Europe, Africa, and Australia, are all more or less rising. This rise of the land-surfaces necessitates a corresponding sinking, either an absolute or a relative sinking, in the surfaces covered with water. It is comparatively easy to test where a land surface is gradually protruding from the water. It is not such a simple matter always to know whether this rise is relative or absolute, for the same effect may be produced by the sinking of the sea-floor as by the actual rising of the land. One thing only we know, that when our measure is water, there must be a corresponding sinking either relative or absolute where there is a rising elsewhere. Direct evidence of the sinking of the sea-bottom is not very easy to find, but such does exist. Students are familiar with the facts collected by Darwin and others, showing from the growth of coral islands, &c., that the Pacific is an area of depression; other evidence consists in the disappearance of well-known rocks, vigias of navigators in different parts of the greater oceans. From this and other evidence, I am very well satisfied that not only the Pacific, but also the North and South Atlantic and the Indian Oceans, are areas of depression.

Having thus roughly mapped out the world, it becomes an interesting problem to correlate the distribution of volcanoes with that of the rising and sinking land. If the older theory of volcanoes be the true one, that they are the direct results of the eruptive forces of the interior of the earth, we ought surely to meet with them in profusion in those large areas where we know the earth to be relatively rising, where in fact the eruptive force of which volcanoes are the supposed violent proofs is concentrated. Is this so? On the contrary; and it is this that forms the burden of my present letter. The fact is that we shall search in vain among the large areas of upheaval except along their boundaries and fringes for any active volcanoes. Take the northern circumpolar region, the most typical area of rising land in the world, and there is absolutely no volcano in it. The Iceland volcanoes and Jan Mayen happen to be outside the area of upheaval, and in a part of the Atlantic which is notoriously sinking. North America, another large area of rising land, is similarly bare of volcanoes. So is South America, save on the very verge of the Pacific, and that part of the Pacific which I believe to be sinking most rapidly. Australia is probably now rising faster than any area in the world save Spitzbergen, and there we have no volcanoes. Europe is similarly free except in that part of it which is sinking, namely, the Mediterranean border. Lastly, there is the vast continent of Asia, a large part of whose northern surface seems, from all the evidence we can collect, to have been quite recently under water and to be still rising. About Asia I wish to enlarge somewhat.

It was one of the peculiar fancies of Alexander Humboldt, the great authority on the Physical Geography of Asia, that there was a large active volcanic region in the Altai Mountains, &c., and he brought together a great deal of plausible matter to support this view.

As this volcanic region would be in the midst of one of the largest areas of elevation on the earth's surface, it would conflict materially with the evidence elsewhere and with the theory of the distribution of volcanoes for which I am arguing. Luckily for me it has been recently shown, so far as the negative results of those who have been to find Humboldt's volcanoes and have not found them, goes, that is, so far as the only scientific witnesses who have surveyed the region may be allowed to dogmatise, that Humboldt was entirely mistaken. I will quote the accounts of the Russian surveyors as they have been translated for the Geographical Society.

"It now remained for me," says Semenof, "to prove by actual observation the existence or otherwise of volcanic phenomena in Djungaria and in the Celestial Mountains, to which Humboldt in his works so often alludes. I started on my journey, firmly persuaded that I should find the conjectured volcanoes, or at all events some volcanic forms, and sought diligently (as Schrenck did on Lake Ala-kul) to establish the correctness of Humboldt's surmises with respect to the existence of volcanic phenomena in Central Asia, by which confirmation I knew a traveller would gain greater credit than by any incomplete refutation of the supposition. I was even aware that Humboldt was rather displeased with the researches of Schrenck, who clearly showed that the island of Aral-Tübe on Lake Ala-kul was not of volcanic origin. The opinions entertained by Humboldt on the subject of the existence of volcanoes in Djungaria were favourite ones with him, and I regret that I was not able to confirm his cherished theory. Küllok Peak, another of Humboldt's mistaken volcanoes, was found to have no volcanic origin whatever. The hot springs and the non-congelation of Lake Issyk-kul were not accompanied by any volcanic forms in the Tian Shan; and furthermore, all the native accounts of phenomena which from their description might be supposed to be volcanic proved unfounded, and were at once disposed of on my examination of the localities where they were declared to occur. The result, therefore, of my researches on this point was that I became convinced of the complete absence of volcanoes, typical volcanic phenomena, or even volcanic forms, throughout the Celestial Mountains. It is true that there existed in Djungaria at one period some solfatara, or smoking apertures, from which there was a discharge and deposit of sulphur, and that some of these fissures, out of which the Chinese obtain sulphur, emit smoke even at the present day. But a careful inspection of one of the extinguished pits satisfied me that, at all events in that case, there was no volcanic affinity. In the neighbourhood of the pits discovered by me in the Kater Mountains and in the Ili Valley, I could trace no volcanic forms. . . . The whole process of the formation of sulphur can then in my opinion be reasonably explained by the combustion of some coal seams in this basin, which would at once set at rest the question of supposed volcanic agency. . . . The observation of a single portion of the Tian Shan visited by me cannot serve as positive evidence of the absence of volcanoes and volcanic forms in other parts of this mountain chain. My conclusions on this question generally have already been made public in the letter referred to, but I must likewise observe in addition that all Asiatic accounts of phenomena which might be volcanic in appearance should be treated by men of science with great circumspection, as many of these accounts have already proved fallacious. I would here also remark that the impression produced on me personally by Djungaria and the Tian Shan leaves great doubts in my mind as to the existence of volcanoes in this part of Asia; and as I am the only traveller who has visited the Tian Shan, I cannot accept the belief in their existence as an axiom requiring no proof or confirmation. My conclusion on this point, though only negative, is one of the most important results of my journey." ("Djungaria and the Celestial Mountains," by P. P. Semenof, Journal of the Royal Geographical Society, 35–213.) Again, I will quote a later traveller, Mr. Severkof. He says—"There are no volcanic formations in the western portions of the Tian Shan which I surveyed. From eastern sources, Humboldt refers to evidences of volcanic action farther south in the Ak-tan, but even these are doubtful. Fire may be produced in the mountains even by the ignition of the seams of coal as well as of the carburetted hydrogen gas filling the caverns of the seams. This conjecture is supported by the circumstance that Messrs. Bagaslouski and Lehmann discovered, on their journey to Bockhara, a burning seam of coal in the mountains of the upper Zaraphan, a little to the south of the Ak-tan. Speaking generally of volcanic action in the Tian Shan and the surrounding regions, the geological surveys hitherto made from Khan-tengir (east of Issyk-kul, near the sources of the Tonta, Djergalan, Tekes, and Kegen) to the extreme western limits of the system, have given only negative results. To the east of Khan-tengir there are again seams of coal—for instance, at Kuldja, and perhaps also at Urumchi—the ignition of which is quite sufficient to create explosive gases. Whether the seams of coal were ignited at Urumchi by volcanic agency, or accidentally at their denudations, is a question that cannot be settled without close observation. It can only be said that the demonstrations in favour of volcanic action adduced by Humboldt are not sufficient proof of the volcanic origin of the Tian Shan, excepting only as regards the lava which, according to Chinese records, flowed from the Peshan mountain during the 6th century. But a single crater—even if the fact of its existence in an extensive mountain system extending, as the Tian Shan does, for 3,000 versts, can be proved—does not make the whole of the range volcanic. (Severkof's "Journey to the western portion of the Tian Shan," Royal Geographical Journal, 40, 395–6.)

This evidence, to my mind, completely refutes Humboldt, and makes it very clear that his volcanic region is non-existent. With the disappearance of this, disappears the only exception I know to the rule that volcanoes, instead of being found chiefly on areas of elevation, are invariably found in areas of depression, or on or close to the boundary lines which separate them from the areas of elevation. The meaning of this lesson, as I read it, I will reserve for another letter.

In conclusion, I wish to thank one of your correspondents in Tasmania for the fact he communicated to you about the rise of that island. I shall be very grateful to anyone who will send me other facts about areas of upheaval and subsidence, and their communications shall be cheerfully acknowledged when I publish them.

Derby House, Eccles HENRY H. HOWORTH

Spectra of Shooting Stars

IT may interest observers of shooting stars who attempt to obtain views of their spectra by the use of suitably adapted meteor-spectroscopes to indicate a peculiarity which seems to distinguish the larger meteors of the December star-shower, radiating annually from the direction of a point near θ Geminorum on the nights of the 10th, 11th, and 12th of December. Two such small bolides of this stream which appeared to me on December 9th, 1864, and on Thursday night last, the 11th inst., were characterised by a beautiful pale-green colour, like that of the thallium flame in purity of tint, but perhaps of a slightly paler or lighter hue, and it remained uniform like the brightness of these meteors as long as they remained in sight, strongly suggesting that either copper, barium, thallium, silver, or some other element giving, in some of its combinations, an intensely green spectrum, was undergoing vivid ignition in their flame. As each of these bright meteors presented a sensibly round disc (the first several times brighter, and the second a little brighter than the planet Jupiter), without visible sparks or train of any other colour than that of the head which could give rise to the green colour by the effect of contrast, and yet the green hue was much more distinct than I have noticed in any other meteors, not omitting some bright ones accompanied by very ruddy streaks in the principal displays of November 14, it appears to be a distinguishing feature of the brighter meteors of the annual star-shower of December, to which it would be very useful on occasions of its future return to direct particular attention. The meteors of this star-shower are, however, seldom of very considerable brightness, and the occurrence of one such during its recent appearance not improbably marked its return during the present year with somewhat more than ordinary intensity. The meteor was simultaneously observed at Glasgow and at Newcastle upon Tyne, and its apparent paths among the constellations at those places, directed from the usual radiant point in Gemini, with the duration of its flight, will enable the real height and the speed of motion of one of the principal meteors of the shower to be pretty exactly ascertained.

During many hours of repeated observations under the most favourable conditions of the sky on the nights between the 23rd and the 30th ult., and again on the 5th of this month, when observers for the return of the shower of meteors belonging to Biela's comet were on the watch for its appearance at different places in England, Scotland and Ireland, the reports of their observations which have hitherto been communicated to the Luminous Meteor Committee of the British Association have been entirely negative, scarcely a single meteor of the few which were observed being recognised as belonging to the well-known radiant-point of the shower, which was so conspicuous last year in Andromeda. At various times during the night of the 27th of November itself, when the sky was generally clear, no meteors of this description were visible, and their absence on all the other nights when the state of the sky permitted a watch to be kept for them scarcely leaves any reasonable grounds for the supposition that even a comparatively insignificant return of last year's meteor-showers of the 24th and 27th of November has this year been visible in England on the same or on any very nearly adjacent dates.

A. S. HERSCHEL

Newcastle-on-Tyne

Meteor Shower

FROM the reported weather in England it seems improbable that the Geminid meteor shower was well observed in England, and as the return was rather above the average a few particulars of what was seen here may at least be interesting.

The nights of the 10th and 11th, when the watch was kept, were exceedingly clear. Except for a quarter of an hour at the commencement of the first watch there was only one observer, then there were two. The position taken was a window, N.E., whence all was visible from about 3° from the zenith to the hills opposite (perhaps 10° or 15°), behind which only one meteor disappeared, whilst only one was noticed whose course was part hidden by the roof. The average per hour on the 1½ hours' watch on the 10th must have been about 38, and on the 2 hours' watch on the 11th, 60. But the rate in the second hour was much in excess of the first; taking the two thus the result is, from 10–11, about 30, from 11–12 about 88. In all probability the rate would have not been much below in the morning hours, but having a cold I did not stop longer.

The brightness was, I think, rather below the average, but as tabulated it was as follows:—

Bright as	Jupiter	1
,,	Sirius	2
,,	1 magn. *	9
,,	2 ,,	17
,,	3 ,,	10
,,	4 ,,	23
,,	5 ,,	2

A comparison with the radiant points in Mr. R. P. Grey's list makes it seem that the meteors were distributed over at least *nine*, and that two of these are ones not included by him. Of these one seems pretty certainly fixed about R.A. 57° N.8 6°, and to this I have assigned 14 on the two nights. The other is more doubtful, two nearly parallel meteors appeared on the 10th, opposite in direction to the others; their point may be about R.A. 275° N.δ. 60°.

An apparent discrepancy in the total seen and the tabulated numbers is explained by the fact that some meteors were not well enough seen to be entered. But on the regular watch of the 11th I had the unusual success of entering every one seen, in consequence I believe of the position I had assumed, *i.e.* seeing less than half the heavens and *lying* on my back. Several cases of almost, or perfectly, simultaneous meteors appeared, but of these only 4 pairs were from the same radiant.

J. EDMUND CLARK

Heidelberg, Dec. 16

THE LATE PROFESSOR DE LA RIVE

SWITZERLAND has in one month been shorn of two of her most distinguished ornaments. De La Rive and Agassiz have died within a fortnight of each other, and the "Académie des Sciences" has thus been deprived in the same month of a fourth of its Foreign Associates. Agassiz will no doubt find, both in Switzerland and America, more than one pen competent to describe his labours in the field of science; but a few lines on the life and researches of de la Rive are due to this distinguished philosopher, and will be read with interest in this country, which he has often visited, and in which he had many friends.

Born at Geneva in 1801, of an old family closely connected with Cavour, Auguste de la Rive inherited from his father the love of science in general, and more especially of electricity. After going through the usual course of studies with brilliant success, he was, at the early age of twenty-two, called to the Chair of Natural Philosophy in the Academy of Geneva, and took his seat amongst the distinguished men of that city.

Although de la Rive devoted his time principally to the study of the different branches of electricity and their numerous applications, his acquirements were not limited to that department of science. During the earlier part of his career the subject of specific heat, more particularly applied to gases, and a series of experiments on the temperature of the crust of the earth, were published by him conjointly with a friend and colleague. But electricity remained his favourite study to the end of his life. The treatise he published between the years 1853 and 1858, in three large octavo volumes on the subject of electricity, translated into English by Mr. C. Walker, F.R.S., and the numerous original articles which appeared in the well-known monthly journal, *Les Archives d'Electricité*, for many years under the direction of de la Rive, afford ample proof of the extent of his information on all subjects connected with his favourite pursuit. His original memoirs on electro-dynamics, magnetism, the connection of magnetism with electricity, the nature of the voltaic arc, and on the propagation of electricity in the interior of bodies, more especially through extremely rarefied media, and others too numerous to be quoted, ensured him a high European reputation, to which was soon added the title of Member or Correspondent of almost every scientific body in Europe. In 1840 he was named Correspondent of the French Académie des Sciences; in 1846, Foreign Member of the Royal Society, and finally in 1864 he was elected Foreign Associate of the Académie des Sciences, the highest honour to which a man of science can aspire.

It was de la Rive who first conceived the idea of applying the force of electricity, through the means of alkaline solutions, to the gilding of silver and brass, and he thus laid down the groundwork of the principle by which thanks to the practical improvements introduced soon after by Messrs. Elkington and Ruolz, electric gilding has gradually superseded the deleterious process of gilding by mercury. It was on this occasion that the grand prize of 3,000 fr. was awarded to de la Rive by the French Academie des Sciences.

A long and patient study of the phenomena which accompany the aurora borealis, and of their apparent connection, both with the properties exhibited by the flame of the Voltaic arc when under the influence of a magnet, and with the passage of the electric fluid through extremely rarefied gases, gradually led de la Rive to a new theory on the electric origin of the aurora. His theory was illustrated, and to a certain extent rendered plausible, by a series of beautiful experiments, reproducing in the lecture-room, through artificial means, the varied phenomena which characterise the aurora. These experiments were made first at Geneva, and some time after repeated at Paris before some of the most distinguished members of the French Institute.

But de la Rive's acquirements were not limited to science. The noble use he made of his fortune, the well-known hospitality which rendered his country house near Geneva for nearly forty years a centre of attraction to the most distinguished scientific and literary society of Europe, the high tone of his character, and the many services he rendered his country, more particularly when called upon in 1860 to use the influence of his name and position in obtaining from the English Government an effectual support for Switzerland against the threatened danger of French aggression, have secured to his memory a popularity which will long survive him.

VIVISECTION

THE advance of culture has brought with it an increased tenderness, and a more solicitous regard for the feelings of others, a regard extending slowly but surely to the feelings of animals also. It is to Science that this advance is mainly due. Only by gaining clear conceptions of natural sequences can men be brought to repress their native tendency to inflict pain as an exertion of power, or to feel ashamed of their thoughtless indifference when they see pain inflicted by others. It is demonstrable that Ignorance has ever been the most potent ally of Cruelty—on the small scale of boys torturing animals, and on the large scale of priests torturing heretics. The boy can only be made to feel that his act is vicious by having a vivid imagination of the fact that the animal organism is constructed like his own, and that the animal suffers as he suffers. The holy inquisitor, or enthroned persecutor, can only be made to see that his attempt to combat heresy by an *auto-da-fé*, is flagrantly at variance with all psychological experience. If the vast cruelties of persecuting "fanatics" have become intolerable in modern society, it is assuredly from no dogmatic teaching, no insistence on charity and love, but wholly from a moral enlightenment coming with a larger and more accurate understanding of natural sequences.

Not only has Science been a great agent in evolving the sympathies, and creating the intense desire to avoid giving pain, it has also created the means of alleviating pain. Is not the whole skill of the surgeon and the physician devoted to this end? How comes it, then, that physiologists who have to supply the surgeon and physician with accurate data, which they can only reach through Experiment, are supposed to be less sympathetic, less careful of the feelings of animals, than other men? A candid person would at once admit that this was not so; would admit that physiologists are quite as unwilling to inflict *unnecessary* pain as men of other classes. But because Vivisection is one of the branches of physiological Experiment, and because when the details of such vivisections are reported, the public reading these, and wholly unacquainted both with the purpose and the procedure, is shocked at what seems needless cruelty, a cry of indignation naturally escapes, and the experimenter is regarded as indifferent to the sufferings of animals.

Every thinking man will admit that the feeling which prompts this indignant cry is highly laudable, and every man who understands the real case will declare that this feeling is misguided by ignorance. For what is the fact? The fact is, that in the vast majority of experiments no pain is inflicted, the operations that are painful being performed under chloroform, and thus the animal which has undergone an operation which would have killed it, had it not been insensible, awakens from the coma and begins tranquilly eating the food before it, as if nothing but a sleep had gone before! In some cases, indeed, pain *is* unavoidable; in some it is part of the phenomenon investigated. But this procedure is not chosen in wantonness, or the thoughtlessness of cruelty. The operation is justified by its purpose. If the tender surgeon inflicts pain, it is to save pain; if the physiologist inflicts pain, it is to widen knowledge, and thus alleviate pain on a wide scale. This is very different from the pain inflicted for the sake of *sport*; very different from the measureless misery of wars, inflicted to gratify national vanity or commercial greed. The physiologist does not inflict pain for his own pleasure; he overcomes his repugnance to it, as he overcomes his repugnance to the sights of the amphitheatre and hospital, nerved by a sense of ulterior good.

Here we meet the question raised by "X.," whether man is justified in inflicting pain on animals to secure the good of fellow-men? I unhesitatingly answer, Yes. It is quite certain that man does assume and assert supremacy, eating, subduing, and exterminating animals, according to his needs; and I would ask whether human life would be practicable on this globe on other conditions? Why, there is seldom a spade thrust into the earth that does not cut some worm into writhing halves. If this be excused as a painful necessity, then also must vivisection be excused as a painful necessity; if the one is necessary to food, the other is necessary to knowledge. The physiologist is the judge of the necessity; on him rests the responsibility.

And now a word on the particular experiments which called forth X.'s protest. Obviously, since testing sensibility was the very purpose in view, Prof. Goltz, Prof. Foster, and myself were forced either to forego the inquiry, or to inflict more or less pain, and (if need were) excessive pain. Perhaps X. will say that such an inquiry ought not to have been pursued at such a cost. We thought otherwise. The point cannot be argued now; but I would illustrate what has been just said, by informing X. that even here anæsthetics were used where they could be used—when I removed the skin from the legs or the body of the frogs, or took out their brains, the animals were wholly insensible; and dreadful as it may seem to read of their limbs being pricked, and burned, we are assured that no pain whatever, not even the feeling of contact, was felt by the frogs.

In conclusion, I would urge upon the opponents of Vivisection, that it would be but fair to credit physiologists with the same repugnance to the infliction of pain as animates all enlightened classes; and to consider that if the repugnance is overcome in the pursuit of physiological knowledge, it does not the less exist, nor the less guide their conduct in other cases. For myself, I may be permitted to add that so far from acknowledging indifference to the feelings of animals, my sympathies are unusually active in the direction of animals; and it was my inability to witness pain which prevented my pursuing the profession of a surgeon. Nevertheless, I have performed hundreds of experiments; and in the very rare cases where great pain was inevitable, the performance has been very distressing; but in all cases I should vehemently protest against the accusation that it was indifference or cruelty which enabled the experiments to be performed.

It is but right that I should acknowledge that Prof. Foster's communication of December 11 has shown me the error of my interpretation of his hypothesis.

GEORGE HENRY LEWES

I WISH briefly to point out the grounds upon which persons who are every bit as tender-hearted and as sympathetic with Nature as any ante-vivisectionist may claim to be, justify what X. condemns. In order that a part of the order of Nature may be ascertained, it is necessary that vivisection be largely practised. Those who practise it do so under a sense of solemn and even sacred responsibility. To suggest the word "cruelty" in connection with their proceedings is an injustice which only profound ignorance and inability to realise the motives of other men can excuse. There is no lack of sympathy with the probable sufferings of animals experimented upon in the mind of the physiologist. He suffers with them, and, as I know of one eminent experimenter, is sometimes disabled by emotion from continuing a research. But the recognition of a higher duty than regard to his own transient impulses or the brief sufferings of a lower animal usually completely controls the experimenter's thought and action, and the mutual suffering of both vivisector and vivisected becomes a sacrifice offered up on the altar of Science. My conviction is that, especially in dealing with such animals as the dog, the experimenter is no less constrained to inflict suffering at which his feelings revolt, by the presence of a noble ulterior motive, than is the surgeon who does not flinch from subjecting his brother-man to the certainty of the direst pain and the imminent risk of death.

No one has a right to *assume* that any other man, still less a whole body of men, is so fiendish as to take any pleasure in the evidences of an animal's sufferings, or so dull as not himself to feel distress when viewing those sufferings. If man is willing to suffer this mental pain for a high end, may he not exact some contribution from the animal world, who after all will benefit as well as he by the progress of Science. It is futile to bewail "the tremendous cost" at which such progress is made. Nature is inconceivably costly, if we choose to put things in that way, for no progress is made without endless suffering and immense destruction. Our very dinner-tables reek with the evidences of "the tremendous cost"—the pangs of slaughtered sheep, the groans of over-worked horses, the disfigurement of Nature's sacred face by agriculture—by which our corporeal means of progress is attained. And are we to be so inconsistent as to refuse to undertake the very highest occupation of humanity, the ascertainment of the order of Nature, because it adds to this "cost" of our existence?

The attempt to raise the question of the "rights" of the animal world in this connection seems to me to involve a very large assumption. I am not prepared to admit that animals have *any* "rights" in the sense that men have them. I could never subject a human being to vivisection for the purposes of scientific progress for much the same reason that, if starving among the Arctic snows, I should feel bound to starve with my companion, rather than kill and feed on him. The recognition of the inviolability of one's fellow-man except under conditions authorised by the community, is the very foundation of human society, and our relations to animals cannot in the remotest degree be assimilated to the relation thus established between man and man. Our conduct towards animals, as towards other living and even inanimate things, must be determined in quite a different way, and by very different reasons. It is, I am inclined to believe, solely the consideration of how we ourselves are affected—whether injuriously or beneficially—by any particular line of conduct towards beings other than men, that can be allowed to guide us in such matters. Anything of the nature of cruelty is obviously thus condemned, and all wanton disrespect to the persons of both living and inanimate things, no less so.

Whilst thus refusing to admit anything like the "right" claimed by man from man, for lower animals, we are not led to regard them with less affection, nor to treat them with diminished tenderness. The conviction that they are ours with which to do what seems good to us, must even increase our disposition to kindly treatment.

Let cases of cruelty, whether from man to man, to woman or child, to horse, fox, or dog, rabbit or frog, be searched out, exposed, and the perpetrator condemned; but unless such persons as X. are prepared to accuse such men as Michael Foster and George Henry Lewes of specific acts of cruelty, they are not justified in making physiology the text for heart-rending appeals to a public imperfectly acquainted with the facts.

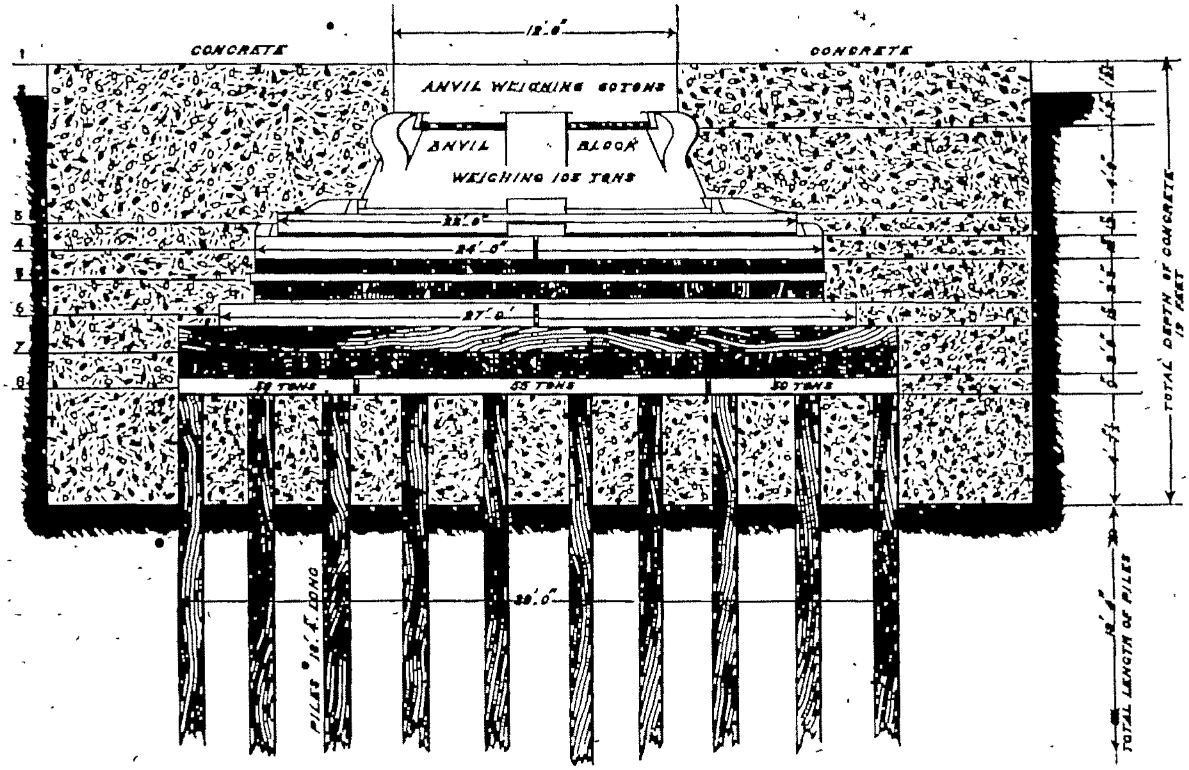
E. R. L.

THE THIRTY-TON STEAM-HAMMER AT THE ROYAL ARSENAL, WOOLWICH

FOR the past two years a stupendous undertaking has been in course of development at the Royal Arsenal, Woolwich, which bids fair to rival in point of solidity and grandeur of dimensions the works of ancient Egypt itself. We allude to the gigantic steam-hammer which is being erected in the gun factories, for the purpose of welding more swiftly and efficaciously than can possibly be done at present the coils of which such massive pieces of ordnance as our modern "Woolwich Infants" consist. The first phase in this undertaking, viz. the laying of an appropriate foundation for the hammer, has now been accomplished, and will be the subject of the present paper. The hammer itself, which is still in an unfinished condition, although rapidly approaching completion, will be treated of subsequently. It is out of the question, in the compass of a brief sketch, to give an adequate idea of all the labour and thought that has been expended upon these foundations, but an endeavour will be made to condense as far as possible the most interesting part of their history into a few words.

The foundations were commenced in a soft, spongy soil, which is the substratum upon which all the Arsenal has been built. A hundred piles of pine-wood shod with iron a foot square each, were driven into the earth so as to form an area of thirty feet square; and when the heads were sawn off to an even surface, their average length was 18 feet 4 inches. Concrete was then filled in all round to the top of the piles, and three cast-iron plates, weighing respectively 30, 55, and 30 tons, were placed upon the heads of the piles. But before proceeding further with the building up of the foundations, we must describe the nature of the castings alluded to. They were all run in the foundry of the Royal Gun Factories, and consisted of about one-fifth of Calder pig-iron to four-fifths of scrap metal containing old broken-up shell, and shot, &c. The metal, after being taken from a number of cupolas in which it was melted, was collected in huge reservoirs, called "sows," and kept in a liquid state during the time necessarily occupied in filling the sows by a quantity of firewood being piled on top, which of course was continually in a state of ignition. This process occupied some eight or ten hours. At a given signal the sows were tapped, and the iron run out into open sand moulds in the floor of the foundry. The removal of these gigantic castings to their destination was a matter involving considerable difficulty. Two sets of worn-out gun-trucks were laid down upon either side of the road, and planks of African oak, placed longitudinally upon these, thus forming a rude railway. Rollers consisting of the unworked tubes of guns were then obtained from the gun factories, and laid across the planks. A sleigh, composed of two massive bars of wrought-iron turned up in front, and attached together by baulks of timber, was then placed upon the rollers, and surmounted first by the cast-iron plate to be carried, then by a movable or revolving crane. The sleigh being drawn forwards by a crab-winch and tackling, as the rollers were successively passed over the crane lifted up those that were behind, and, swinging round, deposited them in front, presenting a fresh rolling surface upon each occasion. Thus the plates were each slowly moved from the foundry to the foundation pit. But there was another difficulty. As it was necessary to have "joggles," or projections npon the summit of several of the plates for the superincumbent ones to rest within, and in open castings it was impossible to cast them upon an upper surface, the joggles had to be formed upon the lower surface, and the plates to be reversed in position afterwards. This was done by casting trunnions upon the edges of the plates, nearer one end than the other, and then swinging the plates over the foundation pit by these trunnions, until the heavier half descended, drawing back the heavier portion by a crab-winch, and finally permitting the lighter portion gradually to descend, the trunnion supports being withdrawn, and the edge of the plate resting on the ground forming a fulcrum. The trunnions do not appear in our engraving, but the joggles may be seen upon the three upper sets of castings.

We will now revert to the laying of the foundations. Over the whole extent of the lower plates a thin layer of rock-elm planks was laid, this being the most indestructible kind of wood known, it being necessary to produce a perfectly even surface for the baulks of timber which come next. These were of oak, thirty feet long, and a foot square. Upon the baulks of oak rest two more plates of cast-iron, twenty-seven feet long, and thirteen feet six inches wide, and weighing each seventy-five tons. They are connected by huge dove-tails cast into the metal itself, as are also the two lower ones, and all the other plates which are in the same horizontal plane. A liquid called "grouting," formed of very thin watery concrete, is poured in between the joints of the plates so as to fill up all interstices, and holes are made in several places through the castings, so as to admit of the grouting freely percolating everywhere. Upon the two plates are more planks of rock-elm, and then a layer of oak stumps two feet three inches long, placed upright, and surrounded by a band of wrought-iron, six inches wide by two inches thick. All the remainder of the foundation pit was filled in with concrete as the work gradually proceeded upwards. Upon the oak stumps are two plates, weighing each sixty-five tons, and forming a square of twenty-four feet. A thin layer of rock-elm planks separates them from a huge single casting, twenty-two feet square, and weighing very nearly 100 tons. Wedges within the joggles of the 65-ton plates fix firmly the single one above, and it in its turn supports the enormous anvil block weighing 103 tons, over which will come the anvil itself, but that is not yet in position. The anvil block was cast in a closed mould, which rested upon a substratum of coke and bricks with passages left filled with straw for the exit of the gas generated; it took, nevertheless, *six months* to cool, and could not be removed until after the manufacture and removal of several subsequent castings. Such is a short review of the principal features in the construction of these foundations; all other information as to details in dimensions, &c., may be obtained from the accompanying engraving. About 660 tons of metal have been made use of in completing them.



Foundations for 90-ton Steam Hammer.

1, Proposed floor; 2, Present ground line; 3, Single block weighing 98 tons; 4, Two weighing 65 tons each; 5, Oak stumps on end with band; 6, Two blocks of 75 tons each; 7, Oak baulks crossed; 8, Three blocks.

THE COMMON FROG*

VII.

THE skull of the frog presents numerous points of interest, but only four of these can be here referred to, as other matters demand our attention.

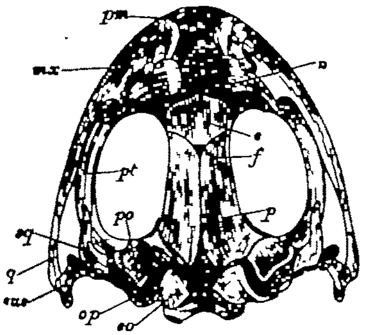


FIG. 37.—Upper Surface of the Skull of a Frog (after Parker). *e*, os en ceinture, or girdle-bone; *eo*, exoccipital; *f*, frontal part of frontoparietal bone; *mx*, maxillary bone; *n*, nasal; *op*, opisthotic; *p*, parietal part of fronto-parietal bone; *pm*, pre-maxilla; *po*, pro-otic; *pt*, pterygoid; *q*, quadrato-jugal; *sq*, squamosal; *su*, suspensorium of lower jaw.

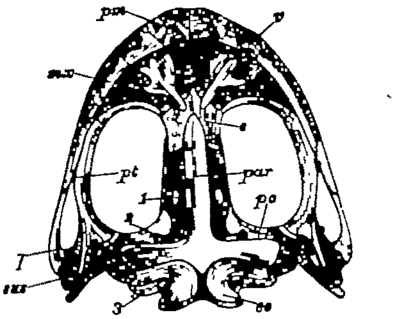


FIG. 38.—Under Surface of the Skull of a Frog (after Parker). *e*, girdle-bone; *eo*, exoccipital; *mx*, maxilla; *par*, parasphenoid; *pm*, pre-maxilla; *po*, pro-otic; *pt*, pterygoid; *q*, quadrato-jugal; *su*, suspensorium of lower jaw, the lower end of which represents the quadrate bone; *v*, vomer; 1, optic foramen; 2, foramen ovale; 3, condyloid foramen.

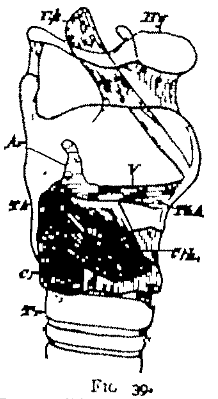


FIG. 39.

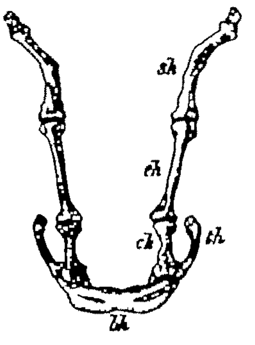


FIG. 40.

FIG. 39.—Diagram of the Larynx of Man, the thyroid cartilage being supposed to be transparent, and allowing the right arytenoid cartilage (*Ar*), vocal ligament (*V*), and thyro-arytenoid muscle (*Th A*), the upper part of the cricoid cartilage (*Cr*), and the attachment of the epiglottis (*Ep*), to be seen. *C th*, the right cricothyroid muscle; *Tr*, the trachea; *Hy*, the body of the hyoid bone. The right lesser cornu appears as a very small process, extending upwards and backwards from the body of the hyoid behind the letters *Hy*, and in front of the Epiglottis. The right great cornu is shown extending backwards from the body of the Hyoid and terminating beneath the letters *Ep*

FIG. 40.—Extracranial portion of hyoidean apparatus of Dog, front view: *sh*, stylohyal; *eh*, epihyal; *ch*, ceratohyal (these three constitute the "anterior cornu"); *bh*, basihyal, or "body" of hyoid; *th*, thyrohyal, or "posterior cornu." (From Flower's "Osteology.")

The first of these four relates to its mode of articulation with the vertebral column. As has been said the first vertebra presents a pair of concavities to the skull. The skull develops from its hinder (or occipital) region a corresponding pair of articular convexities or "condyles." Now in this matter the frog differs from both birds and reptiles, every member of those classes possessing a single median (occipital) condyle for articulation with the vertebral column.

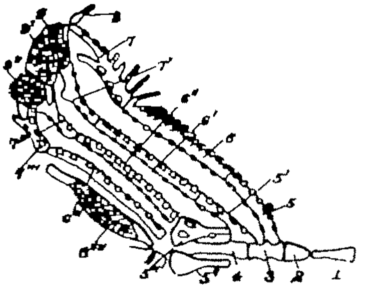


FIG. 41.

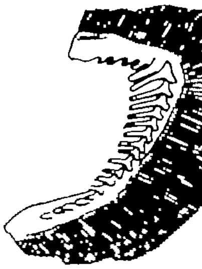


FIG. 42.

FIG. 41.—Skeleton of left series of Branchial Arches of a Perch, seen from above. 1, glosso-hyal; 2, 3, and 4, basi-branchials; 5, hypo-branchials; 6, cerato-branchials; 7, epi-branchials; 8, styliform pharyngo-branchials; 9, pharyngo-branchials; $6''''$, inferior pharyngeal bone; $9'$ and $9''$, superior pharyngeal bones; 5, 6, 7, and 8, first branchial arch; $5'$, $6'$, $7'$, and 9, second branchial arch; $5''$, $6''$, $7''$, and $9'$, third branchial arch; $5'''$, $6'''$, and $7'''$, fourth branchial arch; $6''''$, fifth branchial arch.

FIG. 42.—First three Branchial Arches from the left side of a Perch. On the outer (convex) side of each branchial arch the series of closely-set gill filaments (or leaflets or lamellæ) are seen to be attached. On the inner (concave) side of the first branchial arch are the series of elongated processes (supporting minute denticles) which help to prevent particles of food, or other foreign bodies, passing from the mouth to the gill chamber.

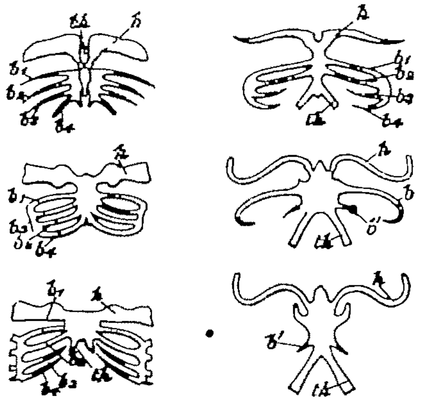


FIG. 43.—Diagram of the changes undergone by the hyoid in a Frog in passing from the Tadpole stage to the adult condition (constructed from Parker's Memoir). Uppermost left-hand figure, the youngest condition; lowest right-hand figure, the adult. *h*, the hyoidean arch, ultimately the corniculum; b_1—b_4, the four branchial arches which become gradually atrophied, the cornua (or thyro-hyal), *th* being their representative in the adult; *b′*, another branchial rudiment; *bh*, the body of the hyoid

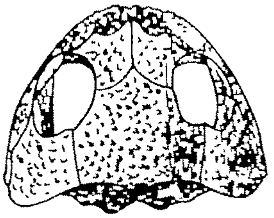


FIG. 44.—Dorsal view of skull of *Pelobates*, showing bony lamellæ behind the orbits.

Yet every member of the frog class, not only every toad and newt, but also every species of the Ophiomorpha, and even every one of the long extinct Labyrinthodons (with the doubtful exception of the probably immature and larval *Archegosaurus*) has a similar pair of occipital condyles. The interesting matter is that man and all beasts have also two occipital condyles. Is this then a mark of affinity, and can we, as it were, reach beasts by a short cut through Batrachians, leaving all the reptiles and birds on one side, as a special outstanding and diverging development?

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* Continued from p. 110.

We shall presently see that other yet more striking facts may be brought forward in support of the latter view. Nevertheless it must be remembered that there are fishes, though very few and exceptional, which also possess a pair of occipital condyles, and that in one respect most fishes are more like mammals than are any Batrachians since they, like mammals, have a well ossified median bone at the base of the skull in the occipital region, a structure which all Batrachians, without a single exception, are destitute of.

The second point of interest concerns the lower part, or base, of the skull, which exhibits a striking agreement with the same part as developed in bony fishes.

This agreement consists in the fact that the middle of the floor of the skull is not formed as in all beasts, birds, and reptiles, by a deposition of bony substance in pre-existing gristle (ossification of cartilage), to which name *Basi-sphenoid* is applied, but, as in bony fishes, by a great bone called *Parasphenoid*, which shoots forwards and also extends backwards to the hinder end of the skull floor, but is formed by the deposition of bony substance in pre-existing membrane. (Fig. 38, *par.*)

Although this great membrane bone is constant in Batrachians and bony fishes, and is represented, if at all, only by minute rudiments in higher vertebrates; nevertheless in serpents we once more meet with a far-reaching and well-developed parasphenoid.

Yet it can hardly be conceived that serpents have carried off from Piscine ancestors and carefully preserved this peculiarity of structure which all their other class fellows have lost. It seems much more probable that this structure has independently appeared through the action of peculiar conditions, and hence that we have here again a remarkable instance of the independent origin of similar structures.

The third peculiarity of the frog's skull consists in the form and conditions of the bony supports of the tongue.

It would not be easy to find a better example of the need of widely extended observations in order duly to understand structures apparently very simple indeed.

The bone of the tongue in man—the *os hyoides**—is a small structure, and one to all appearance of little significance. It is placed at the root of the tongue and above the larynx, and consists of a body with a pair of processes on each side, one large (the posterior or great cornu), and one small (the anterior or lesser cornu, or corniculum).

Even in man's own class (mammalia) the relative development of the parts may vary greatly. Thus the cornicula may be large and may each be represented by two or three distinct applications as in the dog and horse.

The cornua also may take on a development very much greater than that existing in man as is the case in some other Mammals. These facts may prepare us to expect much greater divergences in lower forms; and yet throughout the two great classes of birds and reptiles (as well as beasts)—though varying conditions as to the proportions of the parts present themselves—the os hyoides continues essentially the same in structure, and even in the adult frog this bone exhibits nothing but a rather wide "body" with two long and slender "cornicula" and a pair of shorter "cornua."

Let us now pass for a moment to the other end of the vertebrate sub-kingdom. We find in fishes a complex framework for the support of the gills, or structures, by which they effect their aquatic respiration. This framework consists of a number of arches (placed in series one behind another) extending on each side of the throat upwards towards the backbone, and supporting on their outer sides the gills or branchiæ, on which account they are called the *branchial arches*. In front of these arches and forming as it were the first of the series, is an arch which ascends and becomes connected with the skull.

Turning now to those Batrachians which breathe throughout their lives in the manner of fishes, we find a corresponding system of branchial arches. Thus in the Siren we find a series of gill-supporting branchial arches, placed behind another arch which is connected with the skull.

But the frog passes the first part of its life in a fish-like manner, and in the tadpole accordingly we find an apparatus similar to that of the Siren. There are, in fact, on each side of the throat, four branchial arches, placed behind another arch, which is connected with the skull. As development proceeds these *branchial* arches become gradually absorbed and all but disappear. Relics of them, however, exist even in the adult condition, and thus serve to indicate the true nature of parts which otherwise would be little understood.

The central portion of the structure—that from which arches diverge on each side—increases in relative as well as absolute size, and becomes the "body" of the *os hyoides*. That arch on each side which is connected with the skull and is placed immediately in front of the branchial arches, continues to be so connected and becomes one of the two "cornicula," while the rudimentary relics of the branchial arches which persist become what we have seen in the adult as the cornua of the *os hyoides*.

Thus the anatomy of the tongue-bone of the frog, studied in its progressive changes, reveals to us that otherwise unsuspected relations exist in certain parts of the tongue-bone of man. It exhibits to us the coruna of his *os hyoides* as related to those large and complex branchial arches which play so important a part in the fish and form so relatively large a portion of its skeleton.

The fourth circumstance (the last here to be noticed) connected with the frog's skull concerns the relative position and size of certain of its enveloping bones.

When the skull of the frog is viewed from above, a large vacuity is seen to exist on each side, between the brain-case and the great arch of the upper jaw. In the hinder part of this space is situate the temporal muscle, which by its contraction pulls up the lower jaw and closes the mouth; and the hollow in which this muscle lies is called the temporal fossa.

In a certain frog before noticed, called *Pelobates*, as also in *Calyptocephalus*, a similar view of the skull exhibits no such great vacuity. The reason of such absence is that the temporal fossa in these animals is roofed over and enclosed by the meeting together of bony lamellæ developed from the bones surrounding it, which thus bound the orbit posteriorly, and give to the cranium an altogether false appearance of great capacity.

This very singular structure is found to exist also in the marine turtles, amongst the Chelonians, and here we have another striking resemblance between the Chelonia and the Anoura, apparently reinforcing the argument for the existence of real affinity derived from the presence of such bony dorsal shields in both those two orders. The importance of this character might seem the more unquestionable, since no other reptiles and no birds or beasts whatever were known to exhibit a similar structure.

Quite recently, however, Prof Alphonse Milne-Edwards has described a beast from Africa (*Lophiomys*) belonging to the Rodent (rat, rabbit, and squirrel) order, which has a skull, the temporal fossa of which is similarly enclosed by bony plates.

This unexpected discovery completely destroys any weight which might be attached to this character as an evidence of genetic affinity. It does so because it is inconceivable this Rodent should have directly descended from a common progenitor of frogs and of Chelonians through a line of ancestors which never lost this cranial shield, though the ancestors of all other beasts, all birds, and all reptiles, except turtles, *did* lose it. It is inconceivable, for if it were true a variety of the lowest mammals (Marsupials* and Monotremes†) must have less diverged from the ancient common stock than have the members of the Rodent order, and nevertheless these lowest mammals exhibit no trace whatever of such a cranial shield.

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* So named from its resemblance to the Greek letter υ.

Here then we have an undoubted example of the independent origin of structures so similar that at first sight their similarity might well have been deemed a conclusive evidence of affinity.

Here, also, we have a memorable caution against hasty inferences from structural similarities. If this resemblance and that of the dorsal shields are, when taken together, no signs whatever of special genetic affinity—it is difficult to say what structural likenesses are to be deemed unquestionable evidences of a common ancestry.

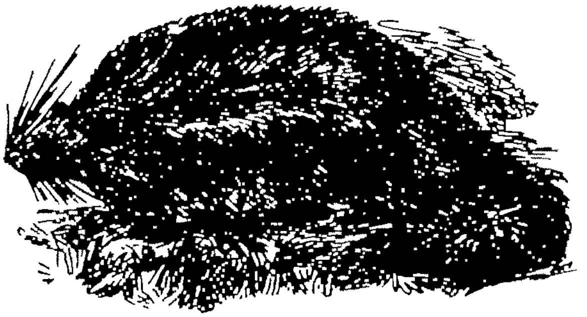
FIG. 45.—External form of *Lophiomys*

Passing now to the skeleton of the limbs, we come to a character of great significance, as it is one which serves to distinguish all the limbed species of the frog's class from lower vertebrates. The character is very significant, because all Batrachians, in spite of their numerous and important fish affinities, differ from all fishes, and agree with all higher classes in that they—if they have limbs at all—have them divided into those very typical segments which exist in man; namely, shoulder-bones, arm-bones, wrist-bones, and hand-bones; and into haunch-bones, leg-bones, ankle-bones, and foot-bones respectively. It is difficult, then, to avoid the belief that in the Batrachian class we come upon the first appearance of vertebrate limbs, differentiated in a fashion which thenceforward becomes universal.

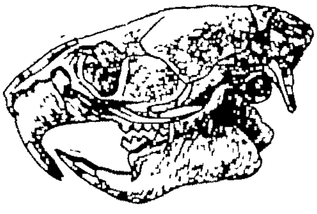
FIG. 46.—Lateral view of skull of *Lophiomys*, showing bony lamellæ behind the orbit.

The bones of the wrist in the frog, again, present a nearer resemblance to those in man than do those of most reptiles, and this is still more the case in some other members of the frog's class, *e.g. Salamandra* and other Efts. Nevertheless, there are certain reptiles, and, strange to say, they are once more Chelonians, which agree in this resemblance—as may be seen in the hand of the tortoise—*Chelydra serpentina*.

The bones of the fingers show, moreover, a greater likeness, in certain respects, to those of beasts than to those of reptiles. No finger has a greater number of joints than three, while, in some lizards, the fourth digit may have as many as five joints.

In the frog the wrist-bones (called respectively the magnum and unciforme) which support the third, fourth, and the little fingers, are formed together into a single ossicle. The same condition, however, sometimes occurs even in the orang. On the other hand, the single bone which in man and beasts supports both the "ring" and the "little" fingers, may be represented by two ossicles in the frog's class (or *e.g.* in *Salamandra*) and in some reptiles (as in *e.g. Chelydra*).

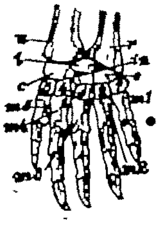

FIG. 47.

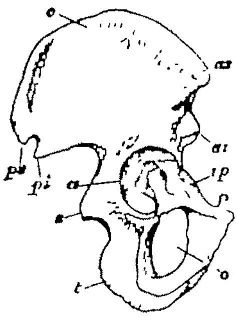

FIG 48.

FIG 47.—Dorsal surface of skeleton of right hand of the Tortoise, *Chelydra* (after Gegenbaur). *c*, cuneiforme, *in*, intermedium (or centrale), *l*, lunare, *m*1—*m*5, metacarpals, *r*, radius, *s*, scaphoides, *u*, ulna, 1—5, the five distal carpals, namely—1, trapezium; 2, trapezoides, 3, magnum, 4 and 5, divided unciforme

FIG. 48.—Outer side of right os innominatum of Man. *a*, acetabulum, *ai*, anterior inferior spinous process of the ilium; *as*, anterior superior spinous process of the ilium, *c*, crest of the ilium, *ip*, ilio-pectineal eminence, *o*, obdurator foramen, *p*, pubis—its horizontal ramus; posterior inferior spinous process, *ps*, posterior superior spinous process, *s*, spine of the ischium, *t*, tuberosity of the ischium.

No member of the frog's class which has an arm at all, bears less than two fingers (as does *Proteus*) upon it. Thus we meet with a number as small as that which is developed amongst beasts in ruminants, but not so small a number as in the horse.

In the rudimentary condition of its thumb the frog participates in a very common defect, since this member is absent in very many forms. It is so even in creatures as highly organised and as like man in bodily structure as monkeys, since both the spider-monkeys of America and certain long-tailed monkeys (*Colobi*) of Africa, are thumbless.

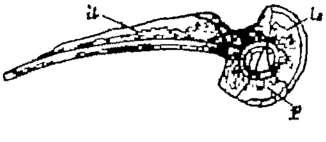

FIG 49.

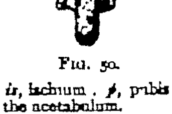
FIG. 50.

FIG. 49.—Right side of Pelvis of Frog. *il*, ilium, *is*, ischium. *p*, pubis The three bones meet at the uppper margin of the acetabulum.

FIG. 50.—Dorsal view of pelvis of Frog, showing the narrow ends of the ilia for attachment to the backbone, and also the close approximation of the acetabula.

In man, when standing, the weight of the body is transferred to the limbs by a large bony girdle, which, from its basin-like shape, is called the *pelvis*,

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* *i.e.* opossums, kangaroos, &c.

† The Ornithorhynchus and Echidna.

This basin consists of the two haunch bones which meet together in front, but behind are separated by the lower part of the backbone (called the sacrum), to which the haunch bones are attached, and which forms the hinder portion of the pelvis. The pelvis has a depression, or "socket," on each side, into which fits the head of one of the thigh bones. Each "haunch bone" consists of three parts, which are, in man, primitively distinct, but afterwards anchylose together, and all three elements (in each haunch bone) take a share in the formation of the bony thigh-socket, or *acetabulum*. These three elements are named—1, *ilium*; 2, *ischium*; and 3, *pubis*. It is the ilium which is adjoined to the sacrum. The pubis, in man, meets its fellow of the opposite side in the middle line in the front of the body. The two ischia (one to each haunch bone) support man's body when in a sitting posture.

The pelvis of man is often quoted as one of the most peculiar and characteristic parts of his skeleton, and its shape in him *is* very peculiar. Nevertheless the pelvis as it exists in frogs and toads is a far more exceptional structure. It is so in the extraordinary elongation, yet small vertebral attachment, of the haunch bones *ilia*, as also in the fact that these bones as well as the other pelvic elements (*ischia* and *pubes*) are all closely applied to each other in the middle line of the body. Thus these elements form a bony disc, and the two sockets (*acetabula*) destined, respectively, for the heads of the two thigh bones, come to be closely approximated one against the other. The great elongation and small attachments of the ilia allow the pelvis as a whole to be bent upon the backbone. Thus the hinder part of the body is moveable and forms as it were an additional common root segment for the two limbs.

ST. GEORGE MIVART

(*To be continued.*)

SOUNDINGS IN THE NORTH PACIFIC

OVER a year ago the United States Congress authorised preliminary measures for laying a submarine cable from the west coast of America to Japan. The United States steamer *Tuscarora*, then on duty off the Isthmus of Darien, was despatched on this business, and started September 22, 1873, from San Francisco for the Straits of Juan de Fuca. Reconnaissances off Victoria, Vancouver's Island, discovered a gradually shelving bottom in all respects suitable for a cable landing. The steamer coaled at Nanaimo. Coal is also found at Newcastle Island, which is not far distant. It may be mentioned that the coal of this region is semi-bituminous, and that recent discoveries have largely increased its product.

The line of soundings extended along a great circle drawn from Cape Flattery to Oonalaska Island. At lat. 53° 58′ N., long. 153° W., within about 400 miles of Oonalaska, the coal was exhausted, and the vessel returned to Victoria. The ocean bed sank rapidly from Cape Flattery to lat. 48° 54′ N., long. 126° 21′ W., then rapidly and steadily to lat. 49° 26′ N., long. 128° 37′ W., then more rapidly to lat. 49° 46′ N., long. 129° 27′ W., at which point the depression was 1,452 fathoms. Thence a peak rose in the sea bottom, with a summit at 1,007 fathoms depth, in lat 51° 40′ N. long, 137° 32′ W. Its rise was fully as rapid as the depression preceding it, and the depression beyond it, the side being equally steep, was somewhat greater. The slope after the western bottom of this submarine mountain was reached was exceedingly gradual, and somewhat undulating. Perhaps the following estimates, roughly made from a sketch, will give a clearer notion of the ground surveyed. At about 100 miles from Cape Flattery, depth about 400 fathoms; at 150 miles, 1,000 fathoms; 170 miles, 1,400 fathoms; 200 miles, 1,000 fathoms; 300 miles, 1,600 fathoms; 400, 1,900 fathoms; 500, 2,000 fathoms; 600, 2,000 fathoms; 700, 2,100 fathoms; 800, 2,200 fathoms; 900, 2,300 fathoms; 1,000, 2,300 fathoms; 1,100, 2,500 fathoms.

During soundings on the return voyage to San Francisco, another submarine mountain was discovered in lat. 41° 30′ N., long. 127° 11′ W., the depth at its summit, which the sounding instruments showed to be of a rocky character, being only 996 fathoms. Around it, at distances of 20 miles, the depth was between 1,600 and 1,700 fathoms.

The water temperatures along the line of soundings for the cable, at depths of over 1,000 fathoms, varied from 0° 45′ C. to 2° 43′ C.; surface, 10° 35′ C. to 14° 15′ C. In lat. 53° 58′ N., long. 153° 00′ W., the increase from 50 fathoms to surface, was gradual; but at 50, 100 and 200 fathoms the same temperature was found as at 2,500 fathoms.

The conclusion has been reached in the course of a series of observations made during the return voyage, and subsequently, that what is known as the "California coast current," is really a warm, and not as hitherto supposed, a cold stream. The observations determined the existence of a warm current, presumably a continuation of the "Great Japanese Circle Current," setting toward the south and east, of a surface temperature averaging 15° C., between the positions lat. 48° 36′ N., long. 126° 36′ W., and lat. 50° 34′ N., long. 131° 38′ W. Outside of this current the temperatue was but 10° C. Its width, between what is known as "Fleurier's Whirlpool" and the coast of California, is about 700 miles; its depth in lat. 44° 54′ N., long. 125° 13′ W. is about 200 ft.; its speed, one to two knots per hour. Under-currents below this stream have been determined, setting to the north and west. The counter-current does not appear to extend more than 30 to 35 miles from shore, moving at a half to one knot per hour, with a depth of 200 to 300 fathoms.

The expedition was equipped with a great variety of sounding apparatus, of which only a few instruments gave perfect satisfaction, and several proved quite useless. The vessel carried 32,000 fathoms line, of which 21,000 were $1\frac{1}{4}$ in., carbonised. Among the satisfactory instruments, Prof. Thomson's is mentioned. This is worked by hand, winding No. 22 piano wire, capable of resisting a strain of 200 pounds. It has a registering indicator and a dynamometer attached. For bringing up material from the bottom, Belknap's cylinder, No. 2, gave the best results, the lower half of the cylinder being usually filled with about three ounces of sea-bottom material, and the upper half with water that had rested on the sea-bottom. The material is brought up secured in the case of a "Sand's cup" by a cylindrical sleeve. The latter is held by a spiral spring, in a position just covering a small orifice in the hollow cylindrical case. On striking bottom, the sleeve is forced up, permitting the material of the ocean bottom to enter the orifice. The instrument is driven into the bottom material by a weight which carries it down with great velocity. This weight, consisting of two hemispheres of iron attached just above the spring, is automatically detached when bottom is struck, by the slackening of the line. Upon drawing up the line, the spiral spring again forces the sleeve down, covering the orifice. The material drawn from the greatest depths was the usual chalky, pasty mud, smooth and homogeneous, rarely containing sand, chiefly composed of casings of diatoms and foraminifers, with here and there the spiculæ and siliceous skeletons of the smaller sponges and *polycystina*.

Although the expedition met for the most part with unsettled and unfavourable weather which interfered with its work, that which it has accomplished is regarded as eminently satisfactory. There is little doubt but that the route upon which the soundings have been made, will be the one selected for the cable; and next spring the work will be extended from the point at which it was discontinued.

NOTES

To the large number of his paleontological discoveries Prof. Owen has quite recently added that of a most peculiar bird from the London clay of Sheppey, which he has named *Odontopteryx toliapica*. This new form, known only from the skull, though perfectly ornithic in general structure, and exhibiting many of the characters of the Steganopodes (Gannets and Cormorants), presents a peculiarity not found in any existing bird. The trenchant margins of the bones of both jaws, instead of being simple, are provided with long conical bony processes, like the serrations in a coarse saw. The posterior of these serrations, which are alone preserved, are directed somewhat forwards; the anterior were probably less inclined, or even directed backwards like the homologous horny processes in the Goosander. The theoretical importance of this new form is great; for it is as good an example as can be brought forward of the loss in modern times, from a persistent type of animals, of a well-developed specialised structure. Many who criticise the evolution hypothesis appear to assume that progress, or what is the same thing, development in the individual of a maximum number of specialised organs, is an indispensable element of the Darwinian hypothesis. Such, however, is certainly not the case after a certain degree of elaboration has been reached. For, taking *Odontopteryx* as an example, it is evident that though this bird had in the struggle for existence acquired a dentigerous mouth, in which point it was in advance of all other members of the bird type, nevertheless its being thus able to obtain food which others could not hold, did not render it in the least less liable to be exterminated by many of the other accidents associated with existence. The upheaval of the sea-bottom, for instance, in its accustomed haunts, would have been destructive to it as to any other of its kind, and probably more so; for the specialisation of the jaws is certain to have been attended with a similar modification in the limbs, resulting in the loss of the power of flight, which would not allow of its removing to a new locality on the change in the physical geography of its home. So with the equally modified Moa, Dodo and Auk, the term of existence of the *Odontopteryx* was a short one, because the tendency of its development was too much towards a degree of uniformity in surrounding circumstances, which the human mind alone knows is not justified by facts.

THE autumn show of the larger fungi at the Royal Horticultural Society has so steadily increased in interest and popularity, that it is intended to considerably extend it next year (1874). The following extract from the recently issued official schedule states the classes which are admissible, and the number and value of the prizes, which are entirely open to all competitors :—Wednesday, Oct. 7.—Class 1, Collection of Fungi, arranged according to their botanical affinities. Neat arrangement and correct nomenclature will be taken into account in awarding the prizes. The edible and poisonous species are to be so marked on their respective labels; the edible species being named on white labels, the poisonous on red ditto, the rest on yellow ditto. Prizes : 5*l*., 3*l*, 2*l*. Class 2, Collection of Edible Fungi. These should be shown, when possible, in juxtaposition with specimens of similar but noxious species. Prizes : 3*l*., 2*l*., 1*l*. Class 3, Collection of New or Rare Fungi. Prizes : 3*l*., 2*l*., 1*l*. Class 4, Cultivated Edible Fungi. This class is intended for species likely to be useful as esculents, but which are not now known in the cultivated state. Prizes : 3*l*., 2*l*., 1*l*.

THE following has been announced as the Cambridge Natural Science Tripos :—First Class.—Martin, Christ's; Balfour, Trinity; (a) Bettany, Caius; (a) Hartog, Trinity; (a) Sollas, John's; Koch, John's—those marked (a) being equal in merit. Second Class.—(a) Balderston, Caius; (a) Davies, John's; (a) Jukes-Browne, John's; (a) Ogilvie, Trinity; (a) Salomons, Caius; Coe, Sidney; Ds. Fletcher, St. Peter's; Ds. Myers, Trinity: Symons, Trinity; Ds. Vinter, Caius; Ds. Yonge, Trinity Hall, the last six equal in merit as well as the first five Third Class.—Ds. Hawker, Trinity; Lighton, Trinity, equal. The undermentioned acquitted themselves so as to deserve an ordinary degree :—Crallen, Emmanuel; Mogg, Pembroke; Slater, St. Catharine's.

MR. ROBERT E. BAYNES, B.A., Wadham College, has been elected to a Lee's Readership in Physics at Christ Church, Oxford. Mr. Baynes gained a First-Class in Mathematical Moderations in Trinity Term 1871; and a First-Class in the School of Natural Science, Michaelmas Term 1872. The stipend of the Lee's Reader is 300*l*. per annum for the first four years after election, 400*l*. for the next three years, and 500*l*. after seventh year from election. He has also a right to occupy rooms in college rent free.

AMONG the more important of the numerous current publications of the United States Hydrographical Office, under Commodore Wyman, is the first volume of a "Coast Pilot" of the coast of Brazil, prepared by Lieutenant Gorringe, and covering the region from Cape Orange to Rio Janeiro, forming a volume of nearly 400 pages, in which the peculiarities of that portion of the coast are detailed with great minuteness, and accompanied by numerous profile sketches of the shores as observable from the vessel at sea. Another report of a very practical bearing is the result of the observations made by the United States steamer *Narragansett* during a cruise between Honolulu and Sidney, conducted between July 6 and September 7, 1872. The points visited were Christmas Island, the Gilbert group, Mulgrave Islands, the Disappointment and Duff Islands, and the Vanikoro Islands.

THE Council of the Society of Arts have given notice of their intention to provide a short course of lectures suitable for a juvenile auditory during the Christmas holidays. For this purpose arrangements have been made with Mr. Frank Buckland, M.A., Her Majesty's Inspector of Salmon Fisheries, to deliver two lectures "On the Structure and Habits of Beasts, Birds, and Fishes, as showing Beauty and Design," on Friday, January 2, and Friday, January 9, at 8 P.M. The lectures will be illustrated by specimens. It is intended to make every effort to obtain an entirely juvenile audience, and the notice in the Society's *Journal* impresses strongly upon the members the fact that only children, not adults, are wanted. The plan is, as far as the Society of Arts is concerned, quite a new one; though the Royal Institution have before now had courses of juvenile lectures.

WE are glad to hear that the course of lectures by Mr. J. E. Taylor, F.G.S., F.L.S., at Ipswich, on "Physical Geography and Geology," has been so successful that the place of meeting has had to be changed to a larger building. The average attendance, we believe, has been 500.

"THE Fifth Annual Report of the Trustees of the Peabody Academy of Science for the year 1872" (Salem, U.S.) is a very cheerful one. The collections in the museum of the Academy are mainly in Natural History and Archæology, and to both departments very large additions were made during the year 1872; the museum, indeed, promises to become one of the most valuable collections in the United States. By the indefatigable researches of Dr. C. C. Abbott a collection of 3,000 implements of the stone age has been brought together, all obtained from the immediate vicinity of Trenton, N.J., on the banks of the Delaware and adjoining fields and hills. The greater part of the present Report is occupied by a number of papers by Mr. A. S. Packard, jun., the Curator of the Articulates. These papers are :—"Synopsis of the Thysanura of the Essex County, Mass., with descriptions of a few extralimital forms," "Descriptions of New American Phalænidæ," "Notes on North American Moths of the Families Phalænidæ and Pyralidæ in the British Museum," "On the Cave Fauna of Indiana," and "Record of American Entomology for 1872."

THE *Dundee Advertiser* is a daily paper of wide circulation and of considerable influence in the north, and is, therefore, we presume, able to keep a competent "London Correspondent." That gentleman, however, in writing in a recent number of the *Advertiser* about Mr. Prestwich's paper on tunnelling the Channel, is made to make the extraordinary statement that "in order to get under the chalk to the *Palæozoic* rocks the Company would have to go to a *depth of ten miles* on either side!" We had recently occasion to point out that science is at a discount in Dundee.

THE Bordeaux district branch of the French Association for the Promotion of Science has resolved to hold its meetings weekly on Mondays.

GOVERNMENT has sanctioned the appointment of a Professor of Physical Science at the Madras Presidency College, on a salary of 500 rs., rising to 700 rs. per month.

THE New York papers have lately contained quite a number of articles urging the propriety of establishing an Aquarium in Central Park, on the same scale as that at Brighton.

WE learn from *La Revue Scientifique* that two specimens of the Ibis, a bird found only in Egypt and at the mouths of the Danube, were recently shot by a hunter in the department of the Somme.

WE can only briefly refer to the following new books and new editions:—"Where there's a Will there's a Way; or, Science in the Cottage" (Hardwicke), is the title of a little volume by Mr. James Cash, containing an interesting collection of lives of persons in humble life who have to some purpose pursued the study of science, especially of Natural History.—"Mountain, Meadow, and Mere, a series of Outdoor Sketches of Sport, Scenery, Adventures, and Natural History," by Mr. G. C. Davies (Henry S. King & Co.), is a series of articles which originally appeared in the *Field* and some magazines. The sketches are generally graphic and racy, and contain information that, we should think, would be valuable to sportsmen of various kinds, with occasional observations on the natural history of the districts referred to by the author.—Mr. John Murray has just published third editions of Mr. H. W. Bates' "Naturalist on the River Amazons," and Mr. J. G. Bertram's interesting work, "The Harvest of the Sea, including Sketches of Fisheries and Fisher Folk." The latter work, which has been the means of doing good service to our fisheries, has been revised, and a considerable amount of new matter added.

A *Times* telegram dated Rome, Dec. 20, states that Colonel Gordon, who has accepted from the Khédive the leadership of a scientific expedition into Upper Egypt, is furnished by his Highness with a credit of 100,000*l*.

WE are glad to hear that a Section for Microscopical Investigation has been formed in connection with the Leeds Naturalists' Field Club and Scientific Association, one of the most efficient of local scientific societies. An excellent microscope has been purchased by liberal subscriptions among the members.

WE have received the Report of the 16th Session, 1872-3, of the Birkenhead Literary and Scientific Society, which numbers 134 members. The Report, among other papers, contains an address by the President, the Rev. G. H. Hopkins, on "the Insufficiency of Facts to establish a Scientific Law," characterised by considerable acuteness and knowledge. A paper read before this Society by Dr. Ricketts, F.G.S., on "Fissures, Faults, Contortions, and Slaty Cleavage," has been printed in a separate form.

THE Annual Report for 1872 of the Birmingham Natural History and Microscopical Society, is on the whole very satisfactory. Prefixed to the Report is a very able and extremely interesting address by the retiring President, the Rev. H. W. Crosskey, F.G.S., on some of the general principles on which geology as a science depends. Some of his illustrations are very forcible and ingenious.

THE Mining Commission, consisting of Savot Bey and Osman Bey, sent into the district of Lom, in the Danubian viceroyalty of Turkey in Europe, has been compelled, on account of the winter, to bring its labours to a close. It has, however, discovered two good coal mines, one ten and the other twenty miles from Lom. There are other mines of iron, copper, and bitumen.

ON November 26, at 11 P.M., a smart shock of earthquake was felt at Prevesa, in European Turkey. Though reported to have been violent, the shock only lasted a few seconds and did no damage. The earthquake of November 10, in Anatolia, extended to Ak Hissar, where it did some damage, and also in the village of Suleimanieh. On October 11, there was a slight shock at Lima, in Peru.

THE following statistics relating to Swedish Universities are from the *Medical Record*:—In the University of Upsala there are 52 ordinary and 2 extraordinary professors, 24 ordinary and 2 extraordinary assistant-professors, and other teachers, making a total of 109 persons engaged in instruction. The number of pupils is 1,607, of whom 172 belong to the faculty of medicine. The University of Lund has 64 teachers, including 28 ordinary and 1 extraordinary professor, and 28 ordinary assistant-professors. There are 545 students, of whom 33 are medical.

THE *Journal of the Society of Arts* informs us that from a recent report to the Congress by the Inspector-general of Public Instruction in Chili, some idea of the educational condition of that republic may be formed. There are 1,190 schools in Chili, of which 726 are public and 464 private. It appears from the latest census that the population of the towns is 520,668, being at the rate of one school for every 1,769 inhabitants; and in the country, with a population of 1,298,560, there would be one school for every 3,020 inhabitants. In 1872 these schools were attended by 82,162 children and young persons of both sexes, and the amount expended by the Government for education purposes amounted to 414,127 piastres. The number of teachers in the primary schools was 1,544, of which 896 were male and 657 female teachers.

ACCORDING to the "Reports of the Mining Surveyors and Registrars," the yield of gold in the colony of Victoria for the quarter ending June 30, was:—from Alluviums 123,643 oz. 6 dwt.; from quartz reefs, 159,604 oz. 17 dwt; total 283,248 oz. 3 dwt.

WE have received No. 3 of Albert Muller's "Contributions to Entomological Bibliography," up to 1862.

THE additions to the Zoological Society's Gardens during the last week include an Alpaca (*Lama pacos*) from Peru, and a Pileated Parrakeet (*Platycercus pileatus*) from Australia, purchased; a Violaceous Plaintain-cutter (*Musophaga violacea*) from West Africa, received in exchange; a Puma (*Felis concolor*) from America, and two Tuberculated Iguanas (*Iguana tuberculata*) from the West Indies, deposited.

SCIENTIFIC SERIALS

THE *Journal of the Franklin Institute*, November 1873.—In this number Mr. Richards, mechanical engineer, communicates the first part of a treatise on "The Principles of Shop Manipulation for Engineering Apprentices;" the points dealt with being these: plans of studying (and here he advocates the order, first, machine functions, next, plans or adaptations of machines, third, construction of machines), nature of mechanical engineering, engineering as a calling, and the conditions of apprenticeship.—Dr. Coolley, in a lecture-extract, shows how convection may be usefully applied in detection of heat. He has an instrument somewhat like a Coulomb electrometer; in a glass case, a thin glass tube with black pith ball at one end is suspended horizontally by a silk fibre over a graduated disc. A heated body is introduced near the ball, which immediately swings towards it; while a cold body will repel the ball; these effects being due to air currents. The experiments Dr. Coolley makes, show that this forms a very sensitive thermoscope.—An account is furnished of the Cleveland Waterworks Tunnel, just completed, and which is similar to the one at Chicago. The shore section and lake section were carried on simultaneously, 40 ft. to 70 ft. below the bottom of the lake; the starting-points being a mile and a quarter apart. The work was somewhat disturbed by quicksands, but the sections met on an exact level. The capacity of the tunnel is 60 to 70 million gallons daily; though the average daily consumption is at present only about 6 million gallons.—A new process is described for utilising coal waste. The inventor uses, as a cement, only yellow clay with some milk of lime, but no bituminous or resinous matter; merely waterproofing the surface with a solution of rosin. From first to last no handling is required; and the lumps are delivered, in shape and size like hen's eggs. The process is highly commended.—We find notes on American machinery abroad, friction of screw propellers in water, &c., and, among other novelties of construction described, a planing bar, a compound beam engine, an antifriction journal, an irrigating machine, and a new optical toy (Prof. Dolbear).

Annalen der Chemie und Pharmacie. Band 169, Heft 1, u. 2.—We notice that in this number Liebig's name disappears from the list of editors, and the title is changed to *Justus Liebig's Annalen der Chemie und Pharmacie.* The following papers are published:—Hubner and Post on the constitution of bromtoluol in relation to its hydrogen atoms. The authors give a collection of minor papers by various authors, dealing with the substitution of different hydrogen atoms in the formula by various radicles.—On the estimation of nitrogen, by S. W. Johnson. The author finds that a mixture of sulphate or carbonate of sodium with slaked lime can be employed instead of the soda-lime usually used in Varrentrapp's and Will's processes. The mixture, when heated, of course, yields sodic hydrate and sulphate or carbonate of calcium. Experiments made with such mixtures are described.—On the nitro derivatives of naphthalin, by F. Beilstein and A. Kuhlberg. The mono-, di-, and tri-nitro compounds are described.—On atacamite, by E. Ludwig. The author proposes some alteration in the ideas of the constitution of this mineral advocated by Rammelsberg and others, his suggestions being based upon the way in which the substance gives up its water at different temperatures; he also makes some suggestions as to formula of brochantite.—On the action of sulphocarbonyl chloride on amidogen compounds, by B. Rathke and P. Schafer.—Note on a polyacetone, by W. Heintz.—On the production of alanin by means of potassic cyanide, and on a by product of the reaction by W. Heintz. The author gives details of the preparation of alanin, the by product is lactyl-urea.—On the constitution of natural silicates, by Dr. K. Haushofer, is a lengthy paper dealing with the probable constitutional and graphic formulæ of these bodies.—On the polyolenes and on the change of ethylene into ethyl alcohol, by W. Gorialnow and A. Butlerow.—On protein substances, by H. Hlasiwetz and J. Habermann.—On the compounds of the camphor group, by J. Kachler. The author describes pimelinic acid, $C_7H_{12}O_4$, and many of its salts.—On the isomers of amylene obtained from the amylic alcohol of fermentation, by F. Flavitzky.—On the synthesis of anthracene and dimethyl-anthracene, by W. A. van Dorp.—On coerulignon and its derivatives, by C. Lieberman. The author regards coerulignon as a quinone.—On pentabrom resorcin and pentabromorcin, by C. Lieberman and A. Dittler.—The number concludes with an abstract from M. L. d'Henry's late paper in the *Comptes Rendus*, on the use of the sodium flame for observing litmus tints in alkalimetry.

Verhandlungen der k. k. geologischen Reichsanstalt. Nos. 1 to 6. (1873.) Amongst many other papers of interest contained in these numbers of the Proceedings we note the following:—On the occurrence of a new genus and new species of palm seed-vessel (*Lepidocaryopsis Westphaleni*) in the cretaceous sandstone of Kaunitz in Bohemia, by D. Stur.—Notices of the earthquake at Vienna on the 3rd January, by Dr. G. Stache.—Hugo Rittler's sketches of the rothliegende in the environs of Rossitz, by D. Stur.—On the analogies of the three carboniferous resins, anthracox, middletonite, and tasmanite, and their probable origin, by O. Feistmantel.—On the geological position and distribution of the silicified woods in Bohemia, by the same author.—The usual literary notices and other matters accompany each part of the Proceedings.

Ocean Highways, December. This number commences with an appreciative memoir of the late Sir Robert Maclure. An article entitled "The Straits of Magellan" contains some very interesting information concerning the little known region in that quarter of the world, and what has been done recently for the settlement of the mainland-coast of the straits. The paper recommends to emigrants Sandy Point, the Chilian settlement at which most of the steamers touch on their way to and from the West Coast, and which "is admirably situated on Brunswick Peninsula, nearly on the line of demarcation between the dense forests which cover the whole western end of the Straits, and the naked, rolling Pampas, which spread uninterruptedly northward to the very shores of the river Plate."—H. H. Giglio sends a letter, with some remarks from Dr. Beccari, on the latter's Exploration of Papuasia. Three small maps of parts of New Guinea illustrate the discoveries of Beccari, D'Albertis, Moresby, Cerruti, and Meyer.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 11.—"On the Action of Heat on Gravitating Masses," by William Crookes, F.R.S.

The experiments recorded in this paper have arisen from observations made when using the vacuum-balance, described by the author in his paper "On the Atomic Weight of Thallium,"* for weighing substances which were of a higher temperature than the surrounding air and the weights. There appeared to be a diminution of the force of gravitation, and experiments were instituted to render the action more sensible, and to eliminate sources of error.

After discussing the explanations which may be given of these actions, and showing that they cannot be due to air-currents, the author refers to evidences of this repulsive action of heat, and attractive action of cold, in nature. In that portion of the sun's radiation which is called heat, we have the radial repulsive force possessing successive propagation required to explain the phenomena of comets and the shape and changes of the nebulæ. To compare small things with great (to argue from pieces of straw up to heavenly bodies), it is not improbable that the attraction now shown to exist between a cold and a warm body will equally prevail when, for the temperature of melting ice is substituted the cold of space, for a pith ball a celestial sphere, and for an artificial vacuum a stellar void. In the radiant molecular energy of cosmical masses may at last be found that "agent acting constantly according to certain laws," which Newton held to be the cause of gravity.

Dec. 18.—"On Double Refraction in a Viscous Fluid in motion," by Prof. J. Clerk Maxwell, F.R.S.

According to Poisson's† theory of the internal friction of fluids, a viscous fluid behaves as an elastic solid would do if it were periodically liquefied for an instant and solidified again, so that at each fresh start it becomes for the moment like an elastic solid free from strain. The state of strain of certain transparent bodies may be investigated by means of their action on polarised light. This action was observed by Brewster, and was shown by Fresnel to be an instance of double refraction.

In 1866 I made some attempts to ascertain whether the state of strain in a viscous fluid in motion could be detected by its action on polarised light. I had a cylindrical box with a glass bottom. Within this box a solid cylinder could be made to rotate. The fluid to be examined was placed in the annular space between this cylinder and the sides of the box. Polarised light was thrown up through the fluid parallel to the axis, and the inner cylinder was then made to rotate. I was unable to obtain any result with solution of gum or syrup of sugar, though I observed an effect on polarised light when I compressed some Canada balsam which had become very thick and almost solid in a bottle.

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* Phil. Trans. 1872.

† Journal de l'Ecole Polytechnique, tome xiii. cah. xx (1829)

It is easy, however, to observe the effect in Canada balsam, which is so fluid that it very rapidly assumes a level surface after being disturbed. Put some Canada balsam in a wide-mouthed square bottle; let light, polarised in a vertical plane, be transmitted through the fluid; observe the light through a Nicol's prism; and turn the prism so as to cut off the light; insert a spatula into the Canada balsam in a vertical plane passing through the eye. Whenever the spatula is moved up or down in the fluid, the light reappears on both sides of the spatula; this continues only so long as the spatula is in motion. As soon as the motion stops, the light disappears, and that so quickly that I have hitherto been unable to determine the rate of relaxation of that state of strain which the light indicates.

If the motion of the spatula in its own plane, instead of being in the plane of polarisation, is inclined 45° to it, no effect is observed, showing that the axes of strain are inclined 45° to the plane of shearing, as indicated by the theory.

I am not aware that this method of rendering visible the state of strain of a viscous fluid has been hitherto employed; but it appears capable of furnishing important information as to the nature of viscosity in different substances.

Among transparent solids there is considerable diversity in their action on polarised light. If a small portion is cut from a piece of unannealed glass at a place where the strain is uniform, the effect on polarised light vanishes as soon as the glass is relieved from the stress caused by the unequal contraction of the parts surrounding it.

But if a plate of gelatine is allowed to dry under longitudinal tension, a small piece cut out of it exhibits the same effect on light as it did before, showing that a state of strain can exist without the action of stress. A film of gutta percha which has been stretched in one direction has a similar action on light. If a circular piece is cut out of such a stretched film and warmed, it contracts in the direction in which the stretching took place.

The body of a sea-nettle has all the appearance of a transparent jelly, and at one time I thought that the spontaneous contractions of the living animal might be rendered visible by means of polarised light transmitted through its body. But I found that even a very considerable pressure applied to the sides of the sea-nettle produced no effect on polarised light, and I thus found, what I might have learned by dissection, that the sea-nettle is not a true jelly, but consists of cells filled with fluid.

On the other hand, the crystalline lens of the eye, as Brewster observed, has a strong action on polarised light when strained, either by external pressure, or by the unequal contraction of its parts as it becomes dry.

I have enumerated these instances of the application of polarised light to the study of the structure of solid bodies as suggestions with respect to the application of the same method to liquids so as to determine whether a given liquid differs from a solid in having a very small "rigidity," or in having a small "time of relaxation,"* or in both ways. Those which, like Canada balsam, act strongly on polarised light, have probably a small "rigidity," but a sensible "time of relaxation." Those which do not show this action are probably much more "rigid," and owe their fluidity to the smallness of their "time of relaxation."

"On the Period of Hemispherical Excess of Sun-spots, and the 26-day Period of Terrestrial Magnetism." By J. A. Broun, F.R.S.

It appears from the interesting communication to the Royal Society, June 19, by Messrs. De La Rue, Stewart, and Loewy,† that the difference of the area of spots on the visible northern and southern quarter-spheres of the sun seems, during periods of considerable solar disturbance, to obey a law such that the difference is a maximum in the same quarter-sphere during several successive rotations of the sun, the difference being a maximum alternately in the northern and southern hemispheres; the time from maximum to maximum, for the same hemisphere, being variable between 18 and 32 days, but having a mean value of about 25·2 days.

It occurs at once that if the variations of the mean terrestrial magnetic force are connected in any way with the solar spots, or the causes which produce them, we might here find some explanation of the magnetic period of 26 days, the difference of spot area in one hemisphere from that in the other being related to a difference of the solar magnetic action.

In order to determine whether such a connection existed, I projected first the curves of excess of spot-area given in the paper of Messrs. De La Rue, Stewart, and Loewy, and below them the daily mean horizontal force of the earth's magnetism during the same periods. The conclusion from these projections is, that there is *no relation whatever between the two classes of curves*. The maxima and minima of the one agree in no ways with those of the other; the greatest excesses of sun-spot area in the one hemisphere over those in the other occur when the earth's magnetic force is the most constant; the greatest variations of the earth's magnetic force from the mean occur in several instances when the sun-spot area is equal in the two visible quarter-spheres.

It should be remembered, in considering the curves of sun-spot excess, that the minima and maxima are in some cases only relative; sometimes the one, sometimes the other being really cases in which there is neither maximum nor minimum; that is to say, cases in which the sun-spot area is equal, or nearly so in the two visible quarter-spheres.

It would be hasty to conclude from this comparison that the variations of the mean magnetic force are really unconnected with the mode of distribution of the sun-spots. Other methods of grouping the spots may perhaps be employed with advantage relatively to this and other questions, for example, were the position of the centre of gravity of the sun-spots determined for the visible quarter-spheres and hemisphere, giving each spot a spot-weight in proportion to its area, the variation of these positions in latitude and longitude and their weights, might give a more satisfactory base for this comparison and for other deductions.

It will be obvious also that this investigation refers only to one *visible* hemisphere of the sun; an approximation to the spot-distribution on the other hemisphere will, however, be frequently possible.

"On the Nervous System of *Actinia*," Part I., by Prof. P. Martin Duncan, F.R.S.

"On certain Discrepancies in the published numerical value of π," by William Shanks.

Mathematical Society, Dec. 11.—Prof. Cayley, F.R.S., V.P., in the chair.—Prof. Clifford gave an account of his paper on the graphic representation of the harmonic components of a periodic motion. The paper was an application of a theorem of Fourier's, which asserts that any motion having the period P may be decomposed into simple harmonic motions having periods P, $\frac{1}{2}$ P, $\frac{1}{3}$ P, &c., and assigns the amplitudes and phases of these motions by means of definite integrals.—Prof. Cayley next spoke on the subject of Steiner's surface. The author stated that he had constructed a model and drawings of the symmetrical form of Steiner's surface, viz. that wherein the four singular tangent planes form a regular tetrahedron, and consequently the three nodal lines (being the lines joining the middle points of opposite edges) a system of rectangular axes at the centre of the tetrahedron. He then described the general form of the surface, and finally discussed its analytical theory.—Lord Rayleigh, Mr. Roberts, Prof. Clifford, and Prof. Cayley made further extempore communications to the Society.

Linnean Society, Dec. 18.—G. Bentham, F.R.S., president, in the chair.—Dr. Hooker exhibited a magnificent zoophyte from Bermuda, sent by General Lefroy; also a six-lobed Seychelles cocoa-nut (*Lodoicea Seychellarum*) and two tazzas made from the shell of a Seychelles cocoa-nut sent from the Seychelles by Mr. Swinburne Ward to the Kew Museum; also some small boxes from Mauritius and Madagascar made from some grass-haulm; and two walking-sticks from Bermuda made of the "cedar-wood" of commerce (*Juniperus bermudiana*).—Mr. Bowring exhibited an inflorescence of an orchid with a remarkable smell, probably a *Bulbophyllum*.—The following papers were then read, viz.:—"Contributions to the Botany of the *Challenger* Expedition," No. 2, by H. N. Moseley, M.A. On the Vegetation of Bermuda and the surrounding sea. About 160 species of flowering plants were gathered on the island; but of these, not more than 100 were certainly native. Those of West-Indian origin were probably brought, as Grisebach had suggested, by the Gulf-stream or by cyclones, there being no winds blowing directly from the American coast which would be likely to carry seeds, which might, however, be conveyed from the Continent by migratory birds. A note by Prof. Thiselton Dyer appended to the paper stated that 162 species sent over by Mr. Moseley had been determined at the Kew Herbarium, of which 71 belong to the Old World, while 2, an *Erythræa* and a *Spiranthes*, were plants hitherto known as confined to single localities in the United States.—"Changes in the Vegetation of South Africa, caused by the introduction of the Merino Sheep," by Dr. Shaw. The original vegetation of the colony is being in many places destroyed or rapidly deteriorated by over-stocking and by the accidental introduction of various weeds. Among the most important of the latter is the *Xanthium spinosum*, introduced from Europe, the achenes of which cling to the wool with such tenacity that it is almost impossible to detach them, and render it almost unsaleable. It spreads with such rapidity that in some parts legislative enactments have been passed for its extirpation; and where this is not done, it almost usurps the place of the more useful vegetation. The president stated that the *Xanthium* has in the same manner deteriorated the pastures in Queensland; whilst in the south of Europe, where it is equally abundant, it does not appear to cause such injurious results. Though generally distributed through Europe, the plant is probably of Chilian origin.—Extract from a letter from Osbert Salvin, F.R.S., to Dr. Hooker, dated Guatemala, Oct. 6. Mr. Salvin is engaged in collecting plants on the slopes of the Volcan de Fuego, 5,000 ft. in elevation, and within an easy ride of a volcano 13,000 ft. above the level of the sea. He hopes to secure all the plants between the elevations of 3,500 and 8,500 ft. Many of the species appear to have a vertical range of as much as from 2,000 to 3,000 ft.

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* The "time of relaxation" of a substance strained in a given manner is the time required for the complete relaxation of the strain, supposing the rate of relaxation to remain the same as at the beginning of this time.

† Proc. Royal Soc. vol. xxi. p. 399.

Meteorological Society, Dec. 19.—Dr. R. J. Mann, president, in the chair.—The following papers were read:—"On an improved form of aneroid for determining heights with a means of adjusting the altitude scale for various temperatures," by Mr. Rogers Field. In this aneroid the scale is adjustable for different temperatures. The principle of the adjustment depends on the fact that when the scale is shifted it becomes inaccurate for the temperature for which it was laid down, and therefore practically accurate for some other temperature, so that the scale has only to be shifted into certain different fixed positions to obtain a series of different scales suitable for different temperatures of the air.—"On the North Atlantic hurricane of August 20 to 24, 1873, which did much damage at Halifax, Nova Scotia, and elsewhere," by Capt. H. Toynbee, F.R.A.S. The author alluded to various data which had come into the Meteorological Office respecting this gale, especially to a chart of its track, and important remarks from Mr. J. R. H. Macfarlane, R.N., Naval Sub-Lieut. H.M.S. *Plover*. This data proved that it was a hurricane, and its route was traced from a position to the south-east of Bermuda to Halifax, showing its probable track for four days. The author then went on to say that if the circular theory for hurricanes were correct, little more could be done, though it would be very interesting to trace so hard a gale from its formation to its breaking up. But he said if Mr. Meldrum's "Notes on the form of Cyclones in the Southern Indian Ocean"* were correct, then it was incumbent on the meteorologists of the northern hemisphere to institute a similar inquiry, as the form of cyclones in the southern hemisphere worked out from facts by Mr. Meldrum, made it necessary to modify the rules in use amongst seamen for avoiding their centres. An enlarged copy of Meldrum's diagram (reversed to adapt it to the northern hemisphere) was exhibited. The paper concluded with a suggestion that the August gale of 1873 would afford the means for inquiry into the shape of the northern hemisphere cyclones, and that data for that month should be collected from all parts of the North Atlantic, and worked up into daily synoptic charts, which suggestion the author hoped would be carried out either by America or England.—On a mercurial barometer for the use of travellers, filled by the spiral-cord method, by Staff-Commander C. George, R.N.

Geologists' Association, Dec. 5.—Henry Woodward, F.R.S., president, in the chair.—"On the Yorkshire Oolites," by W. T. Hudleston, F.G.S. The district occupied by beds of Oolitic age in north-east Yorkshire, constitutes a mass of elevated land divided into two very unequal lobes by a triangular depressed area known as the Vale of Pickering, towards which the beds incline. A diagonal of thirty-one miles, from N.E. to S.W., exhibits the beds of the Moorland range resting on the Lias of Robin Hood's Bay, whence they incline towards the Vale of Pickering, newer beds being continually met with as far as the "Kimmeridge Clay" of the vale. Crossing this vale towards the Howardian Hills, the previous beds or their equivalents are repeated in inverse order, until the Lias of the Vale of York is reached. Dealing with the Lower Oolites only, the group is essentially arenaceous. At the eastern termination of the moorland range (coast section) these beds have a thickness of 700 ft., mostly sands and shales, nearly devoid of marine mollusca, but rich in plant remains. There are, however, four distinct zones of marine life (well pointed out by Dr. Wright in 1859) which may be made out on the coast and identified in the transverse valleys of the moorland range. (1) The Dogger and its associated Land-rock, magnificently developed at Blue Wyke a sandy oolite, altered into an iron-stone, calcic carbonate being replaced by ferrous carbonate in the case of the shells, the original material being now replaced by siderite, very unequally developed, sometimes resting on 40 ft. of "striatulus beds," sometimes directly on the Upper Lias. (2) "The Millepore Bed." At the point of their maximum development 300 ft. of sands and shales intervene between the Dogger and this bed, which, north of Scarborough, is usually an arenaceous ironstone, but a few miles south of that town becomes the most important calcareous member of the Lower Oolites. (3) 100 ft. of sands succeed and then we have the "Scarborough Limestone" series, consisting of grey marly limestones alternating with marly shales and varying in thickness from 50 ft. at Mundall to 3 ft. at Gristhorp (distance 9 miles). Above the Scarborough Limestone series occurs 160 feet of shales and sandstones; some of these beds exhibit casts of myaciform shells. (4) The fourth fossiliferous zone is usually referred to the cornbrash. More complete marine conditions are apparent. Brachiopoda are abundant. *Ammonites Herveyi* plentiful in this bed, which yielded a fine suite of fossils. It forms the last of the Lower Oolites. In the inland chain south-west of the Vale of Pickering, the Lower Oolites are much attenuated, amounting to no more than 150 feet in the Derwent Valley, The types, too, are much altered.

Chemical Society, Dec. 18.—Dr. Odling, F.R.S., president, in the chair.—A paper on the preparation of standard trial plates to be used in verifying the composition of the coinage was read by the author, Mr. W. C. Roberts, Chemist of the Royal Mint. After giving a sketch of the variation in composition of the English gold and silver coins from the earliest times, he noticed the various trial plates which had been prepared since 1660, showing that they sometimes varied considerably from the standard of 916·66 parts in 1,000 for the gold and 925·0 for the silver. He then proceeded to describe the process employed and the difficulties to be overcome in the preparation of the new standard trial plates. These were exhibited at the meeting, and also a magnificent specimen of pure crystallised gold.—Researches on the action of the couple on organic bodies, Part iv., on iodide of allyl, by Dr. G. H. Gladstone and Mr. A. Tribe, being a continuation of their investigations on this subject.—On tetranickelous phosphide, by Dr. R. Schenck.—On ferrous anhydrosulphate, by Mr. T. Bolas. The compound, which is crystalline, is precipitated on mixing an aqueous solution of green vitriol with about nine times its volume of concentrated sulphuric acid.—On the hydrochloride of narceine, by Dr. C. R. A. Wright.

Royal Horticultural Society, Dec. 3.—Scientific Committee.—A. Smee, F.R.S., in the chair.—Dr. Masters, F.R.S., exhibited part of a poplar (sent by Mr. G. T. Saul), which, while apparently healthy, had during the past summer, within twenty-four hours, shed the whole of its leaves and never recovered. The Rev. M. J. Berkeley pointed out that the specimen was visibly attacked by fungus mycelium. No doubt, the tree had long been diseased unsuspected; the healthy bark would probably be reduced to a narrow strip, and when this failed the tree would die apparently quite suddenly.—Prof. Thiselton Dyer exhibited a drawing of a luminous *Didymium* from St. Kitt's.—Mr. McLachlan, F.L.S., inquired as to the possibility of introducing humble-bees into New Zealand; the red clover, which had also been introduced, was not fertilised for the want of them

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* Mr. Meldrum's paper has been published as "Non-official, No. 7" by the Committee of the Royal Society who manage the Meteorological Office

The chairman thought there could be no difficulty about it; the Rev. Mr. Cotton had taken bees out to New Zealand by keeping them at a low temperature, and consequently in a dormant condition, by means of ice.—Mr. McLachlan further wished the opinion of the committee with respect to another New Zealand inquiry by Captain Hutton; Aphides were now becoming very common in New Zealand, but were probably not indigenous. Could the golden-winged fly (*Chrysopa*) be advantageously introduced to check them. The chairman thought that it would be far better to send out dormant lady-birds (*Coccinella*). Mr. Wilson, F.R.S., pointed out the necessity of caution in these introductions; sparrows and hares were far from a boon in Australia.—Prof. Thiselton Dyer read a letter from Mr. Scott, F.R.S., Director of the Meteorological Office, with respect to a change in the climate of Scotland recently insisted on by Mr. McNab. He stated that it was an opinion too general to be lightly disregarded that our winters are warmer and summers cooler, on an average, than in the last century, but did not know where to find records which could be quoted with confidence in a discussion of the question.—Dr. Voelcker, F.R.S., mentioned that there was no doubt that it was quite possible to make wine from grapes ripened in this country; the often-repeated argument that our summers must be cooler because wine was not now made was manifestly fallacious.—Mr. A. W. Bennett, F.L.S., communicated a paper on pollen-eating flies of the group *Syrphidæ*.—Mr. Baker, F.L.S., sent capsules of *Lilium auratum* and *L. speciosum*.

Anthropological Institute, Dec. 9.—Mr. F. G. H. Price, F.G.S., in the chair.—Mr. J. Park Harrison gave a detailed description of two incised tablets, from Easter Island in the South Pacific, discovered by the French missionaries in one of the stone houses supposed to be formerly occupied by the chiefs. The signs appeared to be principally iconographic and to represent forms of life and incidents connected with islands several thousand miles to the west.—Prof. T. McK. Hughes described the results of his exploration of the rock-shelter known as Cave Ha, near Giggleswick, Settle, Yorkshire. In the upper deposits flakes and scrapers of chert and flint and other ancient remains in stone and iron were mixed up with the most recent works of art by the operations of badgers, rabbits, &c. In these beds the bones were found by Prof. Busk to be all of recent species, still, or till quite lately, common in the district. In the older deposits, which were composed chiefly of angular fragments of limestone, and, therefore, were not disturbed by burrowing animals, the remains of bear occurred associated with ox, goat or sheep, and dog; but as yet no traces of men. A point to which the author called special attention was the explanation found here of the occurrence in many ossiferous caves of such immense quantities of the bones of mice. The floor was in places strewn with broken up pellets of owls with here and there a few retaining their form, which, when the hair had decomposed away would exactly correspond to the layers and little bunches of the bones of mice in the underlying beds.—Prof. Hughes also read a joint paper by himself and Rev. D. R. Thomas, "On the occurrence of Felstone implements, of the Le Moustier type, in Pontnewydd Cave near St. Asaph, North Wales." After explaining by reference to sections, the position of the cave and of the deposits in it, the authors described a series of implements of felstone as similar to the common forms of Le Moustier as would be expected, allowing for the difference of material. They exhibited also a collection of bones from the same deposit which were referred by Prof. Busk to *Ursus spelæus*, *U. ferox*, *Hyæna spelæa*, *Rhinoceros hemitæchus*, and others, including a human molar which Prof. Busk pointed out was remarkable for its large size. As the rock, of which the implements were manufactured, occurred in that river basin in the boulder clay only, as the implements seemed to have been made from fragments such as occur in the drift, and are found associated with remanié drift mixed with tumble from the roof of the cave, the authors inferred that the deposit was post-glacial, while the forms of the implements, and the animal remains found with them would refer the beds to the earliest cave deposit in which human remains have been found.—A communication was made by Prof. Busk on a human fibula of unusual formation discovered in Victoria Cave, Settle, Yorkshire. The fragment lay at a considerable depth in the cave and beneath a thick layer of Boulder Clay, and was associated with bones of the two large species of cave Bear, *Hyæna*, *Rhinoceros tichorhinus*, *Bison* and *Cervus*. From its position, accompaniments, and other considerations, the deposit in which the specimen was found, had been regarded as of pre-glacial age.

The London Anthropological Society, Dec. 2.—Dr. R. S. Charnock, president, in the chair.—Causes which determine the Rise and Fall of Nations, by T. Inman, M.D. The paper embraced the whole historical range.—Western Anthropologists and Extra Western Communities, by J. Kaines, D.Sc. The paper shows what should be the moral attitude of the more civilised to the less civilised—what the latter has to teach the former—and the evils of western contact with the backward races.

Photographic Society, Dec. 9.—J. Spiller, F.C.S., V.P., in the chair.—On photo-collotype printing, by Capt. J. Waterhouse. The author recommended the use of citric acid as a clearing agent.—Lieut. Chermside, R.E., read a paper on photography in the Arctic Regions. Mr. Chermside accompanied Mr. Leigh Smith in his Arctic expedition last summer. The temperature at which pictures were actually taken was rarely less than 32° Fahr., but much difficulty was experienced in maintaining the solutions in proper order during excessive cold. The author gave some practical advice on the subject of overcoming actual difficulties inherent to photographic manipulations in high latitudes.

Paris

Academy of Sciences, Dec. 15.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the laws of the magnetisation of steel by currents, by M. Jamin.—An answer to a note read by M. Trécul at the meeting of the Dec. 8, by M. Pasteur. This was a reply to M. Trécul's criticism on the author's note on beer and displayed considerable acrimony, M. Pasteur of course sustained his well-known views of the nature of ferments.—M. Berthelot presented some new remarks on the nature of the chemical elements, which however could not be read on account of want of time. The author, it may be stated, admits the possibility of the elements being modifications of a fundamental substance, and stated that nothing renders it improbable that a discovery like that of the voltaic current might not give us power to still further simplify matter. His paper concluded thus:—We shall only be too happy if Mr. Lockyer, guided by stellar spectral analysis, can shed a new light upon these questions, and continue to investigate problems raised now forty years ago by M. Dumas in a work (*Leçons de Philosophie Chimique*) which has contributed so much to our scientific education.—Researches on new butyl derivatives by M. A. Cahours. The author dealt with the aluminium silicon tin and mercury compounds of butyl.—On the propagation of the *Phylloxera*, by M. H. Marès.—Report on Mr. Douglas Galton's paper "On the Construction of Hospitals," by M. Larrey, and General Morin:—Valuation in mechanical units of the quantity of electricity produced by an element in a battery, by M. Branly.—Hybernation of the *Phylloxera* on the branches and leaves of the vine, by M. Max. Cornu.—Action of the volcanic earth of the solfatara of Pouzzoles on the diseases of the vine, by M. S. De Luca.—On certain morphological changes observed in the genus *Cypripedium*, by M. R. Guérin.

BOOKS RECEIVED

American.—Catalogue of Stars observed in the United States Observatory, 1845–71: Rear-Admiral Sands (Washington).—Daily Bulletin of Weather Reports for September 1872. War Department (Washington).—Annual Record of Science and Industry: Dr. Spencer F. Baird (Harper, New York).—Elements of Logarithms: Pierce (Ginn Bros.).

Foreign.—Annalen der Sternwarte in Leiden: Dr F. Kaiser (Nijkoff).—Somario delle Lezioni di Fisica: Prof. Mombello (Foligno).—Zoologische Studien auf Capri: Dr. Theodore Eimer (Engelmann, Leipzig)

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THURSDAY, JANUARY 1, 1874

THE YORKSHIRE COLLEGE OF SCIENCE

NOW that a scheme for a College of Science at Leeds has been all but completed, under the chairmanship of Lord F. C. Cavendish, M.P., it seems somewhat surprising that such an institution in connection with Yorkshire has not been thought of long ere now. It is the largest county in England, carries on a greater variety of industries all more or less dependent for success on the results of scientific research, and boasts of a larger number of local scientific societies and field-clubs than any other county in the three kingdoms, as we have shown in our articles on that subject. However, "better late than never;" and to judge from the prospectus and subscription lists, a very fair start is likely to be made. The scheme proposed by the committee formed at Leeds in 1869 involved an expenditure of 100,000*l*., but it is not intended at present to carry out the whole of this scheme, but to commence on a smaller scale in temporary premises and with a limited number of professors. We have no doubt, from the hearty way in which the proposal has so far been met, that the college will be a success, and that ere long it will be possessed of a handsome building of its own, with a full staff of professors.

From what follows, it will be seen that the teaching will have a practical or technical aspect, having regard to the processes connected with the multifarious arts and manufactures which occupy the large population of Yorkshire. In the midst of an eminently practical people, there can be no fear of this consideration being neglected, but we hope that in the long run the claims of pure science will not be overlooked, for it is every day being more and more clearly proved that a preliminary training in pure scientific research is the best introduction to a "technical" education; and very many of the industrial applications of science have been found out by students who took no thought of the practical issues of their investigations. There is more than one institution in America which might, in this respect, be taken as models for a technical college.

The Yorkshire College of Science, the Prospectus tells us, is intended to supply an urgent and recognised want, viz.:—Instruction in those Sciences which are applicable to the Industrial Arts, particularly in their relation to Manufactures, Engineering, Mining, and Agriculture. It is designed for the use of persons who will afterwards be engaged in those callings as foremen, managers, or employers; and also for the training of teachers for ordinary Science Schools and Classes.

To carry out the object of the College, it is proposed to establish Professorships in (1) Chemistry and its application to Metallurgy, Manufactures, and Agriculture; (2) Civil and Mechanical Engineering; (3) Physics and Mathematics; (4) Geology and Mining.

The Provisional Committee seem to have right notions as to how scientific men ought to be treated. To obtain the services of eminent scientific men, they say, the payment to each Professor cannot be less than 300*l*. per annum, in addition to a proportion of the students' fees. A precarious income, if raised by annual subscriptions, would not secure Professors of high scientific qualifications, to whom the permanency of the scheme has to be assured. Besides the stipends of the Professors, sundry annual expenses for working and maintenance will be required, and these will be paid out of the general fund. The Committee therefore appeal for contributions upon a generous scale commensurate with the importance of the proposed scheme. This appeal has been well answered already; but we hope that the Committee will not rest until the whole of the original scheme has been realised.

The Committee refer to the sum raised for the Newcastle College of Science, 22,025*l*., with an annual contribution of 1,000*l*. from the University of Durham, and say with justice, that, considering that the wealth of the district over which the benefits conferred by the Yorkshire College of Science will extend is at least equal to the Newcastle district, it is to be hoped that the public spirit of Yorkshiremen in behalf of the College will be as freely expressed.

To the Owens College, Manchester, the sum of 13,500*l*. has been contributed by the engineering profession towards the endowment of the chair for Engineering; and the hope is entertained that towards the endowment of the Professorship in that subject in the Yorkshire College of Science, aid may be forthcoming from a similar source. The chair for Chemistry has also peculiar claims for support upon the manufacturers of the county whose business requires the aid of chemical science.

Arrangements will be made for the establishment of scholarships at the College. All donors of 500*l*. and upwards towards the College funds will be entitled to nominate to a free studentship for a term of years.

It is proposed to vest the government of the College in a board of governors, consisting of (*a*) all subscribers of 250*l*. and upwards; (*b*) fifty governors elected by the general body of subscribers; (*c*) two professors elected by the professorial staff. The governors shall hold two meetings in the year, shall appoint trustees, shall audit the accounts, shall receive the annual report from the council of the College, and shall constitute a court of appeal in certain cases. The ordinary administration shall be in the hands of a body called the council. This shall consist of fifteen members, including a chairman, to be elected out of and by the governors.

One of our wealthy City Companies, the Clothworkers' Company, we are glad to see, has generously come forward in the interests of the College as well as in the interests of the particular branch of manufacture with which the Company is connected, by endowing a Professorship of Textile Fabrics with 300*l*. a year. The subscription of the coal-owners alone amounts to some thousands of pounds, and we have no doubt, when the time comes to extend the sphere of the College and to give it a permanent building of its own, this wealthy class will see it to be their duty largely to add to this subscription. We hope also that others of our City Companies will see it to be their interest to lend a helping hand to the young institution. There are several such technical institutions on the Continent, and it is on this account that in several respects Continental manufactures are much superior to those of Britain. Let us hope that this may not be much longer the case, but that by the establishment of the Yorkshire

College of Science, and similar institutions in other districts, all who are in any way connected with our arts and manufactures may be trained to work on a method so really scientific that Britain shall in this, as she certainly is in some other respects, be foremost among the nations.

REFRACTION OF LIGHT MECHANICALLY ILLUSTRATED

IN preparing an elementary lecture on Light, intended to be given at the Taunton College School, I have had to consider how best to explain the somewhat abstruse principle of optical refraction. It is true that Sir John Herschel, in the sixth of his "Familiar Lectures on Scientific Subjects," giving the explanation of refraction on the undulatory theory, describes it as being "exceedingly simple." The fact is, however, that it involves conceptions of wave-motion, difficult for any but advanced students, and even they feel grateful to the eminent physicist for the help afforded by a familiar illustration with which he follows it. He desires his readers to imagine a line of soldiers marching across a tract of country divided at a straight boundary into two regions, the one level ground suited for marching, the other rough and difficult to walk over. Now if the line of soldiers march with their line of front oblique to the boundary, the men on the side just engaged in the heavy ground will be retarded as soon as they cross into it, so that if the line be kept unbroken, the consequence must be a change of front, which will leave the whole body of men marching across the heavy ground in a new direction—in a word, their direction of march will have been refracted. Now the light-waves emitted from a radiant point being compared to the circles spreading from a stone thrown into a pond, it is easily understood how a sensibly straight portion of such a light-wave, passing obliquely from one medium to another of different resistance, will be refracted in a new direction. This simple conception of change of front is at once apprehended by the learner, to whom refraction thenceforth ceases to be a molecular mystery, and becomes an intelligible mechanical act dependent on the resistance of the two media and the form of their limiting surface. Probably no point in all Herschel's lectures has fixed itself in the memory of so many intelligent readers.

Fig. 1

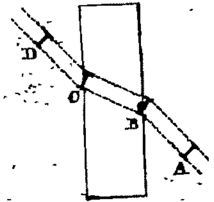
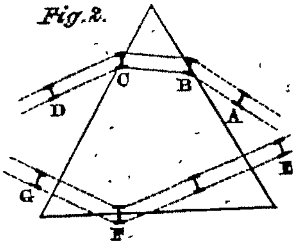


Fig. 2



In following up the train of thought started by Sir John Herschel's comparison, it occurred to me that an instrument made to perform refraction mechanically would be useful in teaching optics, and that such a contrivance would only require a pair of wheels running on a table, into and out of a resisting medium. After a number of trials, made with the help of Mr. R. Knight, a simple arrangement has been completed, which answers satisfactorily in showing the behaviour of a ray of light under the various circumstances of ordinary refraction. Pieces of a thick-piled velvety plush known as "imitation sealskin" are cut out to represent the sections of a thick plate, a prism, a convex and a concave lens, and glued on to smooth boards. The runner consists of a pair of boxwood wheels mounted loosely on a stout iron axle, and is trundled across the board, or still better, the board itself is tilted up, and the runner let go in the proper starting direction. The following figures show the path of the wheels, always from right to left of the page.

Fig. 3

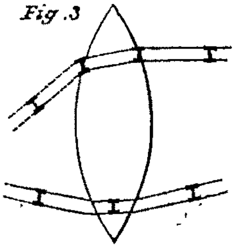
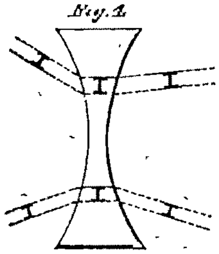


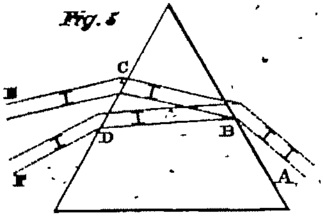
Fig. 4



In Fig. 1, the runner starting from A, enters the rectangle of velvet at B, where its left wheel being first retarded, it shifts round into the direction BC, till it reaches C, where the left wheel first emerging gains on the right, so as to bring back the runner to the ultimate direction CD. This illustrates the refraction of a ray of light in entering and quitting parallel plane surfaces of a resisting medium, such as a plate of glass. When the runner enters at right angles to the boundary, its direction is of course unchanged, as with the ray of light.

Fig. 2 shows the path ABCD of the runner across a triangle, corresponding with the course of a ray traversing a prism. Also, by causing the runner to enter at about a right angle near E, a direction is given to it which, if the surface of the board and the triangle were similar as to resistance, would make it emerge near F, at a small angle to the side. But the left wheel passing on to the smooth surface gains so much on the right wheel still in the velvet, that the axle slews round, the left wheel re-enters the velvet, and the runner goes off in the direction FG, thus illustrating the total reflexion which takes place when a ray of light is directed to emerge very obliquely from a more into a less resisting medium, as from a glass prism or a surface of water into air.

Fig. 5



The action of the double-convex lens in causing parallel or divergent rays to converge is shown by the path of the runner in Fig. 3, which requires no further explanation, nor does that corresponding to the divergent action of the double-concave lens, Fig. 4. By starting two runners at once from the right-hand side of the board, so as to traverse the upper and lower parts of the convex lens, they are made to run into one another, thus illustrating the meeting of rays in a focus.

Lastly, by using two runners with wheels of different diameters, as the refraction depends on the resistance to the wheels by the velvet, the apparatus may be so inclined as to show plainly their consequent difference of refractive angles. The courses of the two are seen in Fig. 5. This experiment, however, requires some nicety of arrangement

Now the separation of rays of different refrangibilities by a prism being due to a like cause, this experiment serves to illustrate mechanically the decomposition of white light. Let the large-wheeled runner represent the red ray, and the small-wheeled runner the violet ray, the principle of the prismatic spectrum becomes at once evident.

For the information of any who may wish to reproduce this simple apparatus, I may state the dimensions I have found convenient. The wheels may be 1¾ in. and 2 in., with rounded edges, mounted on a nearly half-inch iron axle, turned down to ⅜ in. at the ends. The boards may be 2 ft. 6 in. by 1 ft. 6 in., with velvet on each side. It is convenient to place the velvet nearer to one end of the board to leave room at the other for starting the runner; and care must be taken to cut the velvet so as to present a good resisting surface, as this varies with the direction of the pile. In using the apparatus for teaching, care in manipulation is required to neutralise the defects of the texture. Some kinds of "Utrecht velvet," to be had from the upholsterers, are more uniform than the "imitation seal-skin," and thus work more equally, but their effect is not so striking. Wet sand will answer equally well with the velvet, if metal wheels be used.

EDWARD B. TYLOR

THE FRESHWATER FISH OF INDIA AND BURMAH

Report on the Freshwater Fish and Fisheries of India and Burmah. By Surgeon-Major Francis Day, F.L.S. and F.Z.S., Inspector-General of Fisheries in India. 8vo. (Calcutta, 1873.)

IN the introductory part the author states that the present report is the result of investigations commenced by him in the year 1868, into whether a wasteful destruction of the freshwater fisheries is or is not occurring in India and Burmah. He comes to the conclusion that a wasteful destruction of fish is going on to a very great extent, that these fisheries are more and more deteriorating, and that immediate legislation is called for, to prevent the entire failure of a most important article of food.

The steps taken by the Inspector-General to ascertain the facts on which he bases his report were twofold. He personally inspected districts of various parts of the Indian Empire, and supplemented his own observations by collecting the opinions of European and Native officials, to whom he addressed a series of questions bearing upon the subject. Accordingly the book before us is divided into two parts:—(1) The report proper, pp. 1-118; and (2) A *résumé* of the answers returned, with marginal notes by the reporter, pp. i.-ccxxxvi. An article on "Fish as Food, or the reputed Origin of Disease," an Enumeration of the Indian freshwater fishes, and Notes on preserving specimens of fish, conclude the volume.

Europeans who have formed favourable ideas respecting Indian rivers and their abundance of fishes from the accounts which so frequently enliven the sporting papers of the day, will find them rudely dispelled by this report. It is true that not a few of the resident officials deny the decrease of fishes, and deprecate legislative interference altogether. Thus, for instance, the Commissioner of the Agra Division writes that there is no reason to apprehend that any wholesale destruction of fish goes on in these parts. A close-time might no doubt be introduced by law for the protection of fish during the breeding season, but it does not appear to him that it would be easy to carry out such a measure, or that there is any compensating object to be gained; that "it is a useful maxim—*de minimis non curat lex*—minute legislation is unbefitting our position in this country, and more likely to expose our Government to ridicule than to any results of important benefits to the people;" "it is in the highest degree undesirable that the public mind should be disturbed by gratuitous interference on the part of an alien administration, enforced by not very trustworthy agency." On the other hand, the Inspector adduces such incontrovertible evidence in favour of the conclusion he has arrived at, that we can but agree with him that in numerous districts the freshwater fisheries are in danger of being utterly destroyed, and this must appear to call for speedy interference by the Government all the more, as those districts are among the most populous, in which this article of food can be least spared.

Naturally one looks first for the causes by which the Indian fisheries are said to have been thus reduced; and it is not very flattering to be told by the author that this disastrous effect has been caused by the change from the Native to the British rule. He states that, under the former rule, fisheries formed royalties mostly let out to contractors, who alone in the district possessed the right to sell fish, and that they permitted the people, on payment, to capture fish for their own consumption; that the men who followed the occupation of fishing formed distinct crafts or castes, exercising their calling with certain restrictions and regulations. Under British rule the renting system was abolished; with the most philanthropic intentions, the British gave to the people liberty to fish when and where they pleased; where everybody could fish, fishing ceased to be a distinct calling; breedingfish were captured without regard to season; and when the supply of larger fish commenced to fail, it became the practice to catch undersized fish and fry. Add to this, that a number of irrigation weirs and dams were erected, preventing the fish from resorting to suitable spawning-beds, that fixed engines for the capture of fish are now used, where previously they were never permitted, and the natural result is the lamentable state as represented by the Inspector.

We need not enter at present into the remedial measures provisionally proposed by Mr. Day. His proposals, as well as the opinions of his opponents, will no doubt find due consideration on the part of the Indian Government. But I will not conclude this notice, without alluding to one or two of the reports of European officials, which will show that, however weighty their evidence may be as regards the practical side of the question, their opinions in scientific matters are open to criticism. Mr. Day had drawn attention to the destruction of fish by various kinds of crocodiles, very properly recommending that rewards should be paid for their eggs. To this one of the officials replies :—"Waging war against such fish-destroying animals as crocodiles appears to me absurd. I have no doubt at all but that a general destruction of crocodiles would directly frustrate the end hoped for by their destruction. Their very presence in numbers, it being given that they live on fish, shows that the supply of fish is abundant, which is all that anyone requires, and nature in these matters, if left alone, keeps the balance even, and resents interference." This is exactly the same view as that held by the modern advocates of a general preservation of birds, who would preserve even such as the sparrow-hawk and cormorant, and who forget that nature itself, in distributing animal life, does not always consult the convenience of man. In India, the presence of tigers, poisonous snakes and crocodiles, would appear to prevent this doctrine from being generally adopted by the European community. Another official refers to a "very exhaustive and carefully drawn up report" from a Civil Surgeon in his district; this report is accompanied by a list of the freshwater fishes, in which occur some species with Buchanan-Hamilton designations, others with Latin terms derived from a dictionary, a cod-fish, a john-dory, and "a very common fish, the scientific name of which is supposed to be *Lacerta scincus!*" Can anyone doubt after this that a comprehensive and well-illustrated hand-book of Indian Freshwater Fishes with an introductory treatise on the elements of Ichthyology is called for?

ALBERT GUNTHER

KOHLRAUSCH'S "PHYSICAL MEASUREMENTS"

An Introduction to Physical Measurements, with Appendices on Absolute Electrical Measurement, &c. By Dr. F. Kohlrausch. Translated from the Second German Edition by T. H. Waller, B.A., B.Sc., and H. R. Procter, F.C.S. (London: J. and A. Churchill, New Burlington Street, 1873.)

MESSRS. T. H. WALLER and H. R. Procter have furnished us with a translation of the second edition of Dr. Kohlrausch's "Physical Measurements," to which they have added several useful Appendices and Tables.

Their work is intended to serve as a text-book for students in experimental physics, and consists mainly of a collection of the formulæ used in correcting and applying the results of the simpler experiments in weighing and measuring, heat, light, electricity, and magnetism, accompanied in each case by such an account of the method of observation employed as may suffice to render them intelligible.

The limits which the author assigned to himself are very clearly laid down in the Translators' preface, in which we are informed that "descriptions of apparatus are but rarely given, as students mostly have instruments provided for them," and also that "the mathematical knowledge required is but very elementary, as the proofs of the formulæ are only given when they present no complex arguments," but it should perhaps have been added that, even in cases where the apparatus is simple, outlines of the mode of performing an experiment are generally alone supplied, the teacher being left to explain to his pupils the niceties of arrangement and manipulation.

Regarded as a syllabus of a course of physics, the book is incomplete, no account, for instance, being given of Favre and Silberman's Calorimeter, or, with the exception of saccharimetry, of experiments on polarised light; and if the author's plan be thought to justify the exclusion of these, the same reason can hardly account for the omission of methods for determining melting points, or the specific gravity of substances whose constitution is altered by exposure to the atmosphere, or the ratio of the intensities of the illuminations produced by two sources of light, or of all experiments relating to the capillary elevation of liquids in fine tubes.

It is, however, as a collection of formulæ that "Physical Measurements" is likely to prove most useful, and from this point of view the "Introduction" seems to us one of the best parts of the book. It contains the rules for finding the mean and probable errors of a set of observations, and for determining empirical constants by the method of least squares, together with hints as to how to shorten the labour often wasted in the calculation of corrections; points on which a short practical treatise like that here provided will afford great assistance to those who are not mathematicians.

The sections devoted to weighing and measuring are full and good, especially those which relate to the use of the balance, but heat and light are not treated of in an equally satisfactory manner.

The experiments on these subjects which are described are not numerous enough to satisfy the requirements of large laboratories. Moreover, sufficient attention seems scarcely to have been paid to the fact that students should be encouraged to apply corrections to the results of experiments which they perform, not so much on account of the more accurate numerical values thereby obtained, as for the sake of the excellent practice the necessary observations often afford, and the insight gained into the theoretical principles on which they are founded. A case in point is the omission in the article on the Determination of Specific Heats by the Method of Mixtures of any account of the correction employed by Regnault for the loss of heat by radiation.

We miss all mention of the optical bank, and the mathematical expressions for results involving the determination of distance in terms of differential measures on that instrument. In the article on the spherometer, which is in other respects incomplete, we see no instructions for finding the radius of a spherical surface too small to permit the instrument to be placed upon it; and omissions are made in the pages devoted to the spectrometer, the goniometer, and elsewhere, which combine to render the section on Light very imperfect.

Nearly one half of the book is given up to Electricity and Magnetism, subjects in the study of which assistance can be more readily rendered by the method of treatment here adopted than in those we have been discussing, as numerous mathematical formulæ are required which are in many cases obtained by calculations beyond the grasp of the less advanced pupils; and the Translators have considerably improved what was already good by several Appendices, among which one of the most important is that on Thomson's electrometer. Some preliminary sections are devoted to the reduction of observations made with the mirror and scale to angular measure, to the determination of the position of equilibrium and time of oscillation of a magnetic needle and similar topics, while the methods of reading the various magnetometers and galvanometers, and the measurement of resistance and electromotive force, are afterwards discussed.

On the whole the principal fault we have to find with the book is a want of fulness, especially in the earlier portions. It aims at supplying a want already felt, and which will become still more pressing as the number of those who make some progress in the study of Natural Science increases, and we are not aware of the existence of any manual which gives the information contained in it in an equally compact and handy form; while the tables, thirty in number, which fill the concluding pages, will often save time and trouble to those engaged in laboratory work. Although, then, as we have already pointed out, we consider it capable of very considerable improvement, yet probably most teachers of Experimental Physics will obtain some useful hints from its perusal, even if they do not adopt it as a text-book for their pupils.

A. R.

OUR BOOK SHELF

Pheasants for Coverts and Aviaries. By W. B. Tegetmeier, F.Z.S. (London: Horace Cox. 1873.)

ANY work on animals which appeals to so many different human weaknesses as the Pheasants, must be popular if the least effort has been made to do the subject justice. The one before us has merits which make it peculiarly acceptable. It is by the hand of an author who has devoted his life to the careful study of the natures and habits of the Gallinaceous birds and Pigeons, and who has long since made himself well known by works on some of the genera, which have become the standard literature of the points on which they treat. In the handsome volume before us Mr. J. W. Wood's excellent and truthful illustrations add greatly to its value, though the absence of coloration has made it more than difficult in some cases to produce an approach to the gorgeous appearance of some of the species depicted. Among those that suffer most from this deficiency, are the Japanese Pheasant (*Phasianus versicolor*), whose chief beauty consists in the richness and delicacy of the shades of its plumage, and the Golden Pheasant (*Thaumelia picta*), with its ally, the Amherst Pheasant (*T. amherstiæ*), whose resplendent hues even the best artist finds it difficult to represent. The Reeves' Pheasant (*P. reevesii*), and the Eared Pheasant (*Crossoptilon mantchuricum*), however, form excellent and most truthful pictures, colours in them not being such important features. Mr. Tegetmeier, besides describing each of those species which are the love of the sportsman and the pride of the aviary, devotes the earlier part of his work to the discussion of points of great practical interest. After a short history of the Pheasants as a family, from which it is clear that they were introduced into this country from Asia Minor, the native home of the common Pheasant (*P. colchicus*), as early as the reign of King Harold, and probably by the Romans, a series of chapters is given on the management of the bird in preserves and in confinement, together with an account of the diseases to which it is most liable. These are replete with practical detail that must be most valuable to the many who spend such large sums on preserving game, and to those who have the actual superintendence of the coverts themselves. Particular attention is drawn to the great difference between birds, like the common Fowl (*Gallus bankiva*), which are capable of domestication in the true sense of the word, and the Pheasants, which, though individuals are frequently known to become tame, can never be really domesticated; even the young ones taking to the woods on the earliest opportunity, whilst the opposite inherent peculiarities of the poultry have given rise to the proverb—"Curses, like chickens, come home to roost." Altogether this work supplies a long-felt want, and its perusal will well repay anyone who takes it up.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Wasps

PERHAPS it may be of interest to some of your readers, who make entomology their study, to know that the wasps in a nest about a mile from this were still tolerably active on the 13th of this month, when my attention was attracted by the loud buzzing of three or four wasps at the entrance, apparently ventilating it with their wings after the manner of bees. I again visited the place on the 23rd. There were at first no signs of life outside; but stamping on the ground above caused a considerable number to come out after a minute or a minute and a half and hover in the air above the entrance. I attribute this unusual circumstance to the mildness of the season (the minimum temperatures having been 26° in October, 25° in November, and 29° on the 10th and 11th of December, and the 13th having been mild, and so also the 23rd) and the bad conducting power of the nearly cut out peat bog in which the nest was situated.

Birr Castle, Parsonstown, Dec. 24, 1873

ROSSE

The Potato Disease and Lord Cathcart's Prize

NO one acquainted with botanists and botanical science can feel surprised at the decision of the committee in this matter, and it must be confessed that, however well meant, the offer of the 100*l*. prize was a great mistake which has only ended in producing ninety-four unsatisfactory essays and the loss of a year.

Little else could have been expected, for the Council of the Royal Agricultural Society must surely be aware that the men (in this country at least) who are competent to write anything *new* on the subject could certainly be counted in units, and these men could not enter into the competition for more reasons than one, not the least being the loss of status such a proceeding would entail.

It appears to me that the committee have even now hardly hit the right nail upon the head in recommending a grant of money to "some competent mycologist" to investigate the life history of the fungus during a certain period of its life. If the investigation is carried on by any *one man* it is sure to end in failure. It would be far better for the committee to recommend that *five or six* competent botanists should each write an essay on the subject from his own point of view, each essay to be published in the Society's journal. There are many reasons why this would be best. I will give one. Payen has figured and described certain ciliated bodies found in spent potatoes, and which Berkeley and other botanists have looked upon as the probable resting-spores of the *Peronospora*. Montagne has referred these same bodies to the *Sepedoniei*, whilst I am by no means sure that the objects do not belong to the *Stilbacei*, and are no other than *Volutella ciliata*. However this may be, I have met with the last in spent potatoes in immediate connection with the *Peronospora* itself. Where competent observers differ in opinion it is better to get the views of all. It would be very unwise to restrict the observations to any particular period of the growth of the plant, and very little would be added to our knowledge were the resting spores themselves found; for, resting spores or no resting spores, it is an ascertained fact that the living fungus has a season of rest underground, and whether in the condition of resting spores, a sclerotioid mass or a number of myceloid threads, the principal fact remains that the fungus lives through the winter in a state of rest. As to certain potatoes being able to resist the disease, I shall shortly be able to show that whilst certain breeds of potatoes *entirely resist* it in one place, they fall a *ready prey* to it in another.

Hence any experiments carried on in one place by one person, though valuable in themselves, must be inconclusive and imperfect.

The great question is, "How can the disease be evaded or destroyed?" and this can only be answered, if answered at all, by men who thoroughly know the fungus and its allies.

WORTHINGTON G. SMITH

The Denudation of Limestone Hills of Sarawak

THERE is an agency in the denudation of the limestone rocks of Saráwak which I do not think has been noted, but which is very efficient locally in its operation.

The limestone in question is a dark-blue compact rock (probably the oldest stratified formation in this part of Borneo) full of fissures and joints, and forming hilly tracts in Saráwak proper and Samarahan. It is a not uncommon occurrence during periods of unusual drought for the jungle clothing these hills to take fire in some unascertained way, and for large tracts of the vegetation to be destroyed before the conflagration dies out or is extinguished by rains. Such an accident took place two years ago on the Jambusan hill, and a short time previously on Gunong Angus (whence the present name, "Burnt Hill"), and on Marajáh, a large hill near Bidi; and I have been informed by natives that similar fires are known at the head of the Undup, where I have observed from a distance extensive masses of limestone.

When such a fire takes place, not only may we take for granted that a great deal of surface-rock is more or less calcined, so as to be easily removable by the heavy tropical rains; but, there being no covering of soil to speak of, and the exterior rock having been merely bound together by a matted network of roots and creepers, large masses of rock—long loosened by weathering, or freshly detached by the expansion of air and water in the fissures—keep falling from the higher parts of the hill as their supports are burnt away; whilst groups of burning trees go crashing down the scarps, assisting the work of degradation by collision with the inequalities in their paths.

It is, however, subsequently to the fire that its most important effects become apparent. For the next year or two fresh dislodgments of rock will be continually taking place, particularly when, after the almost daily rains, the sun shines out, striking on the bared rock with rays of tropical fervour. Many years elapse before sufficient soil collects in the crevices of the rock to support vegetation; and until the whitened face of the hill is once more shrouded in jungle, it remains immediately exposed to steady sub-aerial denudation; so that, bearing in mind the immense rainfall, the abundance of fissures and joints in the stone, and its solubility, I am inclined to believe that the degradation of these hills which goes on during the interval before they again become efficiently shielded with vegetation, is comparable to centuries of waste of the same rock under ordinary conditions.

Were the limestone hills of Saráwak more gently rounded and less scarped, their destruction through the agencies above described might not be noteworthy; but, owing to the frequency of lines of old sea-cliffs and mural precipices, nearly the whole of the detached rock passes at once to the bases of the hills, where it is again attacked by the rains, assisted now by running streams or standing water.

Saráwak, July 1 A. HART EVERETT

An Appeal to our Provincial Scientific Societies

Now that our provincial museums are yearly increasing in number, it appears desirable to draw the attention of the provincial scientific societies to their importance as the centres for the private collections illustrative of the local geology, natural history, and archæology which from time to time come into the market. We are entirely indebted to private energy for any British collections which we possess. How lamentable then is it that there is no public system for centralising them in our public museums, and thus saving them from dispersion by their passing into the hands of dealers or private collectors, or into the possession of foreign or metropolitan museums. Every year witnesses such losses, which are regarded with complete indifference by our local representatives of Science. It is unaccountable that not one of our provincial Societies has as yet had the public spirit, energy, or foresight to see the importance of this work and of raising a fund for the purpose of ultimately securing such collections for the district.

It is a question of national scientific importance. The collections which are formed during the present century may be said to represent the "pick" of the country. By-and-by, when localities are worked out, and the rarity and value of specimens greatly increased, we may awaken to a sense of the mistake we have made in not devoting our energies less to palæontological literature, and more to the formation of complete and exhaustive local series and collections, and thus smoothing the path of, and providing interest for, the investigators of our fossil and recent flora and fauna.

Such is the lack of originality displayed in this country, and precedent is so blindly followed, that everywhere we find narrow scientific cliques, so-called "Societies," apparently formed merely for the sake of having social gatherings and by means of a local periodical facilitating the cheap publication of the papers of such as contribute.

The energy thus expended is almost entirely thrown away. Indeed, so far as the journals of these "societies" are concerned, these societies are mere hindrances to the progress of Science, for, did they not exist, the papers which appear in their obscure journals (or "napkins," in which the "talents" of these societies lie hid) might be contributed to such as have a general circulation, and thus benefit the world at large. I would most earnestly impress on our scientific Societies the great importance of devoting their energies more to the formation and preservation of complete and exhaustive local collections. With such division of labour how much more accurate and rapid would be the progress of the sciences of Geology and Biology.

S. G. P.

The Killing of Entomological Specimens

A NOTE in a recent number of NATURE, reminds me of some experiments I made about 15 years ago upon the action of the vapours of volatile liquids (hydrocarbons, chloroform, &c.) on insects, my object being to find an expeditious and painless method of killing entomological specimens. Several vapours produced insensibility from which the insects recovered more or less rapidly, but bisulphide of carbon vapour killed them effectually.

My method of applying it was to place a few layers of blotting paper, lint, or cotton wool, on the bottom of a wide-mouthed bottle, pill box, or other convenient place of execution; then to pour a few drops of the liquid upon this and confine the insect in the receptacle, which on account of the great density of the vapour need not be very accurately closed. The action of the vapour must be continued a few minutes after signs of life have disappeared, or the insect will recover.

The most obstinate of beetles succumb without a struggle, and the most delicate of moths or butterflies are uninjured, provided the liquid itself does not touch them. Butterflies may be killed after they are pinned out, by simply placing a little cotton wool soaked with the bisulphide in a box near to them.

W. MATTIEU WILLIAMS

Lecture Experiments

THE result of convection in a liquid, tending to cause the upper part of the mass to be constantly at a higher temperature than the lower, may be well illustrated by the two following experiments:—

Two large glass beakers are placed in front of a sheet of white paper, one of them filled with cold the other with boiling water. A boiling-tube filled with freshly prepared starch solution which has been coloured deep blue by gradual addition of aqueous solution of iodine, and has then been heated until the colour just disappears, is plunged into the beaker of cold water; the blue colour, caused to return by the cooling of the solution, will appear first at the bottom of the tube and then gradually creep upwards, showing that the lower part of the heated liquid first becomes sufficiently cooled to cause the return of the colour. In order to insure the disappearance of this colour by heat, an excess of iodine must be carefully avoided.

In the boiling water contained in the other beaker is immersed a boiling-tube filled with the blue liquid obtained by adding caustic soda in excess to a solution of copper sulphate and tartaric acid, with which has been mixed a little grape sugar (a small quantity of "set" honey): the formation of yellow cuprous oxide commences at the surface of the liquid, and is seen gradually to extend to the lower parts, showing that the upper parts first attain the temperature requisite to cause the reaction to occur which precipitates cuprous oxide.

These experiments are easy of execution, and by the above arrangement, or still better by being projected on the screen, may be rendered visible at a considerable distance.

Queenwood College FRANK CLOWES

Mr. Garrod's Theory of Nerve-Force

THE thermo-electric theory of nerve-force propounded by Mr. Garrod (NATURE, vol. viii. p. 265) seems capable of extension. If a pole of metal, cased in a non-conducting sheath, were sunk in an artesian boring so as to reach from the level of constant temperature to the greatest depth attainable, how far would such pole fulfil the conditions of a sheathed nerve penetrating from the cool surface of an animal to the warmer interior? And with so little difference of temperature in so great a length, would its dynamic effect be at all appreciable?

A quarter of a mile of submarine cable let down the shaft of our Cambrea mine might represent a sheathed nerve; and any existing nerve-force might there be tested. Abandoned mine-shafts are the terrors of our Cornish moorlands. Is it within the power of Science to convert them into earth-nerves, say by lining their sides with non-conducting material, and then packing them tight with conductive slag or some kind of metallic refuse? And is it possible, even in theory, to make such earth-nerves work some kind of earth-muscle? For ignorant me to speak of this subject is ultracrepidism (NATURE, vol. vii. p. 262). Yet it seems a fair extension of Mr. Garrod's ingenious theory.

AUGUSTINE CHUDLEIGH

Cambrea, Cornwall

Genesis in Borneo

MR. CAMERON'S paper read at the Society of Biblical Archæology, testifies to the early diffusion of Semitic traditions by the agency, it may be inferred, of Moslem converts.

The same traditional coincidences recorded of Borneo are found in New Zealand and elsewhere, and would naturally accompany the diffusion of Malayan dialects throughout Polynesia, an influence the duration of which may be counted by centuries.

A. HALL

Dec. 11

Indian Snakes

IN a small treatise on Indian snakes by Dr. Nicholson, R A., the author states his belief that cobras will not feed in captivity unless forced to, starving themselves voluntarily to death. He thinks, also, that jugglers in this country either "feed their cobras with liquid nourishment, or else let them loose when their lives are in danger," recapturing them at a future time.

To test the correctness of this, I questioned a snake-charmer a few days ago, and he informed me that he fed his cobra every week with frogs. His snake had then been recently fed, so he was told to bring it to the bungalow again in a few days. A frog (*R. tigrina*) was procured, and placed in the small basket in which the cobra was kept. The latter seized it at once; but as I was anxious to see the whole process, which could not be done whilst the snake was coiled up in the basket, I requested the man to place the frog on the ground. As it struggled away (the hind limbs of the poor reptile had been broken) the cobra followed it eagerly, and again and again seized it. The want of fangs, and the size of the frog, which in its inflated state exceeded considerably the circumference of its enemy, rendered these attempts ineffectual; so a smaller frog was caught, and placed with the cobra in the basket. This was swallowed in a short time, the snake pushing its victim against its coils, and working down the hind limbs by a lateral motion of the lower jaw, very similar to that of a cow chewing the cud.

The large frog was now placed in the basket, and the cover put on, and in about half an hour had followed its companion. The cobra's appetite was now appeased, for. after seizing a third frog it let it go, on its croaking a remonstrance.

A laughable incident occurred whilst the snake was following the frog over the gravel path. A performing monkey belonging to the juggler, in a spirit of mischief, or perhaps fearing that its master's property was escaping, stepped gravely after the snake and laid hold of it by the tail. As a natural consequence, round came the cobra and menaced the monkey, which, retreating with sundry grimaces, took refuge with the juggler, in great alarm at the turn events had taken.

This cobra is a small one, and as it is one of those very pale, almost cream-coloured varieties, that finds no mention in Gunther's able work, I am anxious to examine it thoroughly The owner, however, affirms that he has to draw its fangs about once a month, and as he is most cautious in handling the reptile, it is probable that the fang matrix has not been destroyed, and examination will be safest just after the operation of extracting the fangs.

Mangalore, Sept. 12 E H. PRINGLE

CLASSIFICATION OF CLOUDS[*]

IN an essay on the "Modifications of Clouds, read to the Askesian Society in 1802, Howard first proposed his classification of clouds, which has since been the generally received authority on the subject. His system has thus stood its ground for more than half a century, in spite of its defects and of the misconstruction not unfrequently put on the two terms, "stratus" and "nimbus" since the publication of Kaemtz's Meteorology. These misapprehensions and the obscurity and confusion arising from them are pointed out by Prof. Poey, but the errors have not been followed so generally as is asserted, at least by British meteorologists. In a series of papers issued at intervals during the past eleven years, Prof. Poey has endeavoured to develop a new classification of clouds, of which the volume before us is the result.

The following is Poey's classification compared with that of Howard:—

POEY'S CLASSIFICATION		Cloud composed of
First type:	cirrus	spicules of ice.
Derived:	cirro-stratus cirro-cumulus pallio-cirrus	snow.
Second type.	cumulus	vesicular aqueous vapour
Derived:	pallio cumulus fracto-cumulus	

HOWARD'S CLASSIFICATION.	
First type:	cirrus
Derived.	cirro-stratus cirro-cumulus.
Second type:	cumulus
Derived:	cumulo-stratus.
Third type	stratus.
Derived from the three types.	nimbus

In forming his system, Prof. Poey first strikes out the "stratus" as being from Howard's own definition not a true cloud, but only "mist;" the "cumulo-stratus" as not differing really from the cumulus; and the "nimbus" as being not a single cloud, but rather a system of clouds. He retains the word "stratus" as part-descriptive of the "cirro-stratus," but in this case it is exclusively restricted to those instances where the cirrus arranges itself in a stratified form, and is not applied when the arrangement is an extended sheet or continuous layer of considerable thickness totally impervious to the sun's rays. To this latter condition, the new term "pallium" is applied.

In his classification Poey arranges the clouds in the order in which they severally appear, from the cirrus, the most elevated, its height being from 30,000 to 50,000 feet, to the fracto-cumulus, the lowest of all; and groups them into three divisions according as they are composed of ice-crystals, snowy particles, or vesicular vapour.

But the most fundamental change which he has introduced into the system is the *pallium* or *sheet-cloud*, in its two distinct forms of *pallio-cirrus*, and *pallio cumulus*, according as it is formed from the cirrus or the cumulus. The pallium is the greyish, or ash-coloured cloud which overspreads the whole sky, and from which rain falls continually for hours or days together. On the approach of rain the pallio-cirrus is formed by the rapid increase and thickening of the cirrus downwards from the enormous

[*] "Nouvelle Classification des Nuages suivie d'Instructions pour servir à l'Observation des Nuages et des Courants Atmosphériques" Par André Poey, Havane (Extrait des Annales hydrographiques, 1872) Paris, 1873 (17 Planches)

accessions of moisture that take place, by which this high ice-cold region of the atmosphere over a great extent and thickness, is brought to the point of saturation and condensation. Underneath this leaden-hued mass of cloud which uniformly covers the sky, but separated from it by a clear space, is extended the dense cloud covering of the *pallio-cumulus*, which is formed by the watery vapour of the atmosphere reduced to the points of condensation and precipitation. This is the true *rain-cloud*, and it is fed and increased by the rapid drifting in from below of torn masses of cumulus constituting the *fracto-cumulus* or *wind-cloud*. The fracto-cumulus may be of all sizes, has no determinate shape, is the lowest and swiftest moving of the clouds, and is whitish, greyish, or slate-coloured, as may be determined by the hygrometric condition of the air. On the return of fine weather accessions of vapour by the fracto-cumulus slacken and then cease, the pallio-cumulus diminishes in thickness and gradually clears away, showing through its intervals the pallio-cirrus above it, which in its turn is broken up, revealing still higher up the delicate tracery of the cirrus. The pallio-cirrus is negatively electrical, whilst the pallio-cumulus is positively electrical, the clear stratum between being neutral; and between these oppositely electrified strata, discharges frequently take place in thunderstorms.

The merits of Prof. Poey's work are very considerable, whether they be regarded as expository of Howard, or as a contribution to this difficult branch of meteorology; and it is just those meteorologists who have paid particular attention to the observation of the clouds who will be readiest to recognise its merits. It must, however, be conceded that, as a descriptive classification of clouds, as well as explanatory of the phenomena they present, Prof. Poey's work leaves the subject in a state still too incomplete to warrant us in recommending his system for general introduction. It is a step in the right direction, and will materially contribute to place this vitally important department of atmospheric physics on a satisfactory footing.

Toward this end, what is now urgently wanted is an extensive collection of the data of cloud-phenomena in all countries, particularly of those clouds interesting in themselves or from their known relations to weather changes. We have more than enough of unmistakeably pure typical forms scattered through the pages of weather-literature, but such do not greatly assist us, in describing and classifying many of the forms of clouds which occur. Hence what is required is faithfully accurate delineations of these forms in their different aspects, and systematic inquiries set on foot into the relations of the forms of clouds to the mode of their formation, to the states of the aqueous vapour which compose them, and to the varying elasticity, temperature, and electricity of the atmosphere.

In connection with this part of the subject, Prof. Poey investigated in 1862-64 by means of the thermo-electric pile, the temperature of different parts of the sky under different conditions, and of the clouds which passed across it. Among other highly interesting results, he has shown that the cumulus, properly so called, and the cumulo-stratus of summer are the clouds of highest temperature; then follows the fracto-cumulus, except when it comes after the rain which accompanies a thunderstorm, in which case it is of a whitish colour, very rapid in its motion, much torn at the edges, and partakes of the low temperature prevailing on such occasions. The cirro-cumulus is colder than the cumulus and the cirrus the coldest of all the clouds. These are very suggestive results. We are convinced that the key to the position in meteorology is a better knowledge of the vapour of the atmosphere in its various states and changes; and the science will not make the advances it is destined to make till meteorologists generally recognise the necessity of equipping their first-class observatories with the requisite appliances for carrying on those physical researches which are intimately allied to meteorology.

FERTILISATION OF FLOWERS BY INSECTS

V.

More conspicuous flowers adapted to cross-fertilisation, and less conspicuous ones adapted to self-fertilisation, occurring in different species of the same genus.

WHAT has been described in the two last articles as occurring in varieties of the same species (using the term "species" in its widest sense) we propose now to investigate as existing likewise in species of the same genus.

Malva sylvestris and *rotundifolia*

are two closely allied, but, as acknowledged by all botanists, undoubtedly good and distinct species, differing in their flowers in a manner similar to the two varieties of *Lysimachia vulgaris* and the other species previously considered. In both these species of Malva an oval mass of anthers in the first place occupies the middle of the flower, enclosing the stigmatic branches as yet undeveloped and lying close together (Fig. 23). At a later period the stigmatic branches, growing out of and overtopping the mass of anthers, spread and bend outwards and downwards so as to occupy nearly the same place as was before occupied by the anthers (Figs. 24, 25). Insects, therefore, seeking for the honey which is secreted and contained in five cavities between the lowest parts of the petals (*n*, Fig. 23) and covered by a fringe of hairs (*pr*), carry away on their hairy bodies the large prickly pollen-grains from younger flowers, leaving many of them on the stigmatic papillæ of the branches of the style of older flowers, which they can scarcely avoid grazing in seeking for the honey. Hence, in both species, whenever insects frequently visit these flowers, cross-fertilisation in the manner described is largely effected, whereas self-fertilisation can scarcely take place, neither spontaneously nor by means of insects, nearly all the pollen-grains having been removed before the unfolding of the stigmatic branches. Since, however, *Malva sylvestris* and *rotundifolia* grow for the most part in the same locality, and flower during several months at the same time, insects flying about and seeking for honey are much more likely to find out and visit the highly conspicuous flowers of *M. sylvestris* than the far less conspicuous ones of *M. rotundifolia*; the former, when fully opened, presenting bright rose-coloured bells of from 40 to 50 mm. diameter, the latter, on the contrary, light rose-coloured bells of only from 20 to 25 mm.

Direct observation, indeed, fully confirms this supposition, the flowers of *M. sylvestris* being always found in sunny weather visited by a variety of insects, whereas those of *M. rotundifolia*, especially when growing intermixed with *M. sylvestris*, are commonly overlooked by them all. Thus, during the six last summers, I have observed on the flowers of *M. sylvestris* and collected more than 50 species of insects, many of them very frequently (2 Lepidoptera, 3 Diptera, 5 Coleoptera, 40 Apidæ, some Ichneumonidæ); while in the same space of time I found on the flowers of *M. rotundifolia* but 5 species (4 Apidæ, 1 Hemipter), and those only in single or a few cases.

It is evident from these facts, that wherever our two species of Malva grow together in the same locality, *M. rotundifolia* would be rapidly extinguished, unless it were enabled to produce seed by self-fertilisation; *M. sylvestris*, on the other hand, is so commonly visited and cross-fertilised by insects that self-fertilisation, if it were possible, would never be effected, or only exceptionally. Accordingly natural selection must have preserved and accumulated those slight individual variations of *M. rotundifolia*, which afford facility for self-fertilisation, whereas in *M. sylvestris* the possibility of self-fertilisation being quite useless, might be lost, and, indeed, has been, completely or nearly lost. Thus in the flowers of *M. sylvestris*, when precluded from the visits of insects by covering them with a net, the anthers remain filled with pollen-grains, and never, or only exceptionally, come spontaneously into contact with the stigmatic branches, the free ends of their filaments at a later period bending downwards, and the branches of the styles remaining considerably above them (Fig. 24). Conversely in the flowers of *M. rotundifolia*, when the visits of insects are prevented, the anthers, filled with pollen-grains, remain in so high a position, and the stigmatic branches bend so far downwards as to come abundantly into contact with the pollen-grains, self-fertilisation being thus inevitable (Fig. 26).

Epilobium angustifolium and *parviflorum*

differ most strikingly in a similar manner. The flower of *E. angustifolium*, being of larger size, brighter colour, grouped in long splendid clusters, and exciting attention at a great distance, are so largely visited and cross-fertilised by insects[*] as never to have need of self-fertilisation, which has actually become impossible; the four stigmatic branches unfolding so long after the maturity of the eight anthers, and so far overtopping them, as to be completely shut out from the pollen of the same flower. The flowers of *E. parviflorum*, on the other hand, being of smaller size, lighter colour, and single, are so inconspicuous that insects but very rarely visit them. Accordingly, its four upper anthers so closely surround the four-lobed stigma, which is mature at the same time, as to cover it largely with their pollen, whilst the pollen-grains of the four lower anthers lying on the way to the honey, cannot reach the stigma of the same or of another flower unless transferred by insects.

Polygonum

Among the many species of the genus Polygonum which grow in our country there are two, *P. Fagopyrum* and *Bistorta*, most distinguished by their attractiveness for insects, which is due not only to the size and colour of the single flowers and to their collection into handsome spikes, but also, and even more perhaps, to their abundance of honey secreted by eight globular nectaries at the base of the filaments (*n*, Figs. 26, 27). With reference also to the frequent visits paid them by insects,[†] these two species have been adapted to inevitable cross-fertilisation by their visitors, self-fertilisation having at the same time become difficult or almost impossible. The manner in which this advantage has been attained being very different in the two species, it is evident that in this case the adaptation to cross-fertilisation by the visits of insects cannot have been inherited from the common parents of the genus, but must have been acquired by the single species during their evolution.

P. Fagopyrum has acquired, as shown in Figs. 26 and 27, the same kind of dimorphism which has been so fully explained by Darwin in Primula[‡] and Linum.[§] In both of the two kinds of flowers (which occur only on different plants) there are three styles and eight stamens, three of the stamens closely surrounding the styles and opening outwards, the five others inserted more outwards, alternating with the leaves of the perianth and opening inwards. An insect, therefore, visiting a flower for honey and pushing its head or proboscis between the inner and outer stamens into the base of the flower, cannot avoid being charged with pollen, especially in those parts of its body which, whilst it is sucking the honey, are pressed against the anthers. Now, the place occupied in one of the two kinds of flowers by the anthers, is occupied in the other kind by the stigmas, the same parts of the body of the insect which in the long-styled form were pressed against the anthers, come into contact in the short-styled with the stigmas, and conversely. Thus it is inevitable that insects effect chiefly what is called legitimate fertilisation, *i.e.* transmission of the pollen of the long-styled flowers to the stigmas of the short-styled, and of the pollen of the short-styled to the stigmas of the long-styled form. Fertilisation by pollen of the same form, however, and even of the same flower, is not impossible, and in the short-styled flowers even spontaneous self-fertilisation may happen, by pollen-grains falling down from the anthers upon the stigmas.

The same advantage which *P. Fagopyrum* has attained by dimorphism (Darwin) or heterostyly (Hildebrand), has been gained in the flowers of *P. Bistorta* by protandrous dichogamy, *i.e.* by the anthers so far preceding in their development the stigmas that in the first period of the flower (Fig. 28) only mature anthers, at a later period (Fig. 29) only mature stigmas are present, the anthers having then commonly fallen off. It is readily seen that such flowers also, when perseveringly visited by insects, are always inevitably intercrossed, no other mode of the transmission of pollen being possible than from younger flowers to the stigmas of older ones. It is only when the visits of insects are completely wanting during the first period and the anthers remain clothed with pollen while the stigmas attain their maturity, that self-fertilisation by insects or even spontaneous self-fertilisation is possible.

The least attractiveness for insects, on the contrary, among all native species of Polygonum is possessed by *P. aviculare*, its flowers (Figs. 30 and 31) being of small size, of greenish and white or reddish colour, standing singly on procumbent plants and offering only a small quantity of pollen to insects, but, as far as I have been able to see, no honey. No wonder that insects are induced only in very rare cases to visit and fertilise them,[*] and that, in compensation for the loss of cross-fertilisation, these little flowers regularly experience spontaneous self-fertilisation, the three inner anthers lying so close to the stigmas that their pollen-grains inevitably come into contact with them (Figs. 30 and 31).

Of the many other native species of Polygonum, which are all intermediate, as to their attractiveness for insects, between those now described, I will only remark briefly upon *P. Persicaria*, which is of more especial interest because of its flowers presenting great differences of structure. In this species, instead of eight nectaries there are only five developed, and these secrete a much smaller quantity of honey than those of *P. Fagopyrum* and *Bistorta*. Its spikes of flower, moreover, being less conspicuous than in those species, the visits of insects are somewhat rare, even in sunny weather, although far more frequent than in *P. aviculare*.[†] Fertilisation by insects, consequently, is by no means secured. Corresponding to this uncertain agency of insects the sexual organs of the flower are in a remarkably fluctuating condition, undecided, as it were, between adaptation to cross-fertilisation by the visits of insects, and to self-fertilisation. Thus, of the eight stamens, sometimes only the five outer ones are developed, the three others being reduced to rudimentary filaments; and this condition is apparently the most favourable to cross-fertilisation, as any honey-seeking insect must touch the anthers in every flower with one side of its proboscis, the stigma with the opposite side, to which it thus cannot fail to transfer pollen-grains from the flowers previously visited. Sometimes, also, the three inner anthers are developed, and, completely filled with pollen, closely surround and spontaneously self-fertilise the two (in rarer cases three) stigmas, cross-fertilisation being thus almost prevented. But most of the flowers show an intermediate condition, having only one or two of the three inner anthers developed.

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[*] On the flowers of *Epilobium angustifolium* I have hitherto observed 26 species of insects, 14 of them belonging to the family of bees, many of them very frequently; on those of *E. parviflorum* I found only once Meligethes, and once a butterfly (*Pieris rapæ* L.) repeatedly sucking the honey of its flowers

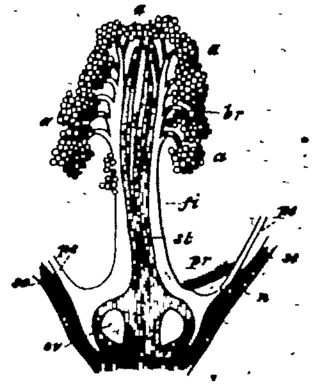
[†] On the flowers of *P. Fagopyrum* I have observed 41 species of insects, among them 21 Diptera and 12 Apidæ; on the flowers of *P. Bistorta* 18 species of insects, among them 9 Diptera and 3 Apidæ; many of the visitors of each species very frequently.

[‡] On the two forms or dimorphic condition in the species of Primula and their remarkable sexual relations (Proc. of the Linn. Soc. vi. (1862), Bot. pp. 77–79.

[§] On the existence of two forms and their reciprocal sexual relation in several species of the genus Linum, Ibid. 1863, pp. 69–83.

[*] After having repeatedly in vain watched *P. aviculare* in very hot sunny noons of the month of August 1871, I succeeded in observing some small Syrphidæ (*Ascia podagrica* F., *Syritta pipiens* L., and *Melithreptus menthastri* L.) visiting its flowers.

[†] I have observed in the flowers of *P. Persicaria* altogether 11 species of insects, among them 7 Diptera, and these as the most frequent visitors.



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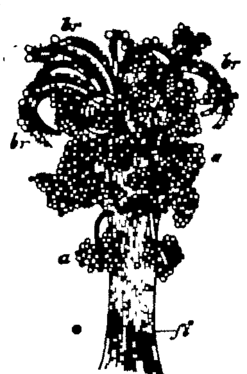

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FIG 23.—Sexual organs of *Malva rotundifolia*, in their first period, longitudinally bisected, seven times natural size. *a*, anthers; *br*, branches of the style (*st*); *pe*, petals; *n*, nectary; *pr*, protecting hairs; *se*, sepals; *ov*, ovary, *fi*, filament-cylinder.
FIG. 24—Side view of the same organs in their second period.

Without referring to many other genera which I have ascertained to contain species quite analogous to those just described,* we may, I think, admit as a summary of the recorded facts, the following propositions:—In many species the flowers vary and have always varied in size, colour, the quantity of secreted honey, and consequently in their attractiveness for insects. Whenever in such a varying species the one variety possesses such a degree of attractiveness for insects as to receive sufficiently frequent visits from them, those variations which afford

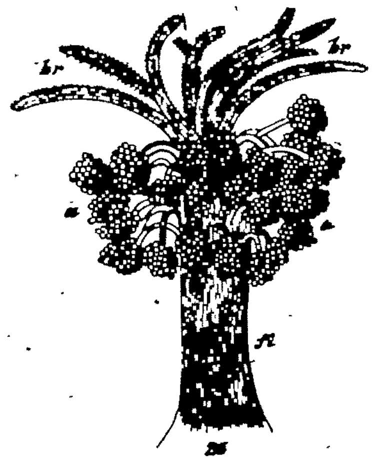

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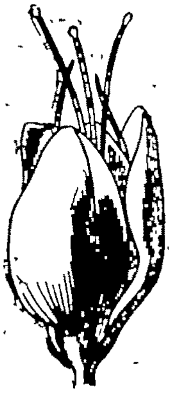
FIG. 25.—Side view of the sexual organs of *M. sylvestris*, seven times natural size.

facility for cross-fertilisation by insects have always been preserved and accumulated by natural selection, whereas the possibility of self-fertilisation has at the same time frequently been lost. Hence we may infer that cross-fertilisation is more advantageous to a plant than self-fertilisation. Whenever, on the contrary, another variety of the same species presents so little attraction for insects as to remain commonly overlooked by them, only such individual peculiarities as induce self-fertilisation have been preserved and accumulated by natural selection, whereas cross-fertilisation by insects has frequently become very difficult, although perhaps never quite impossible. Hence we may infer that self-fertilisation is by no means absolutely disadvantageous to a plant, but only when the offspring of self-fertilisation has to struggle for existence with the offspring of cross-fertilisation.



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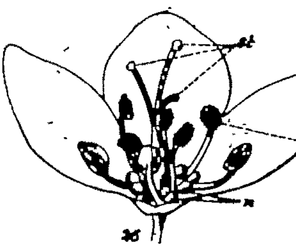

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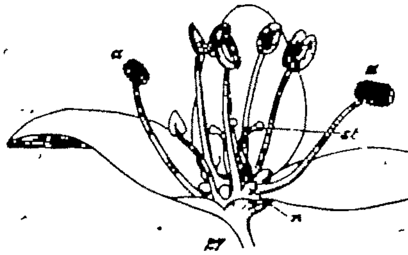

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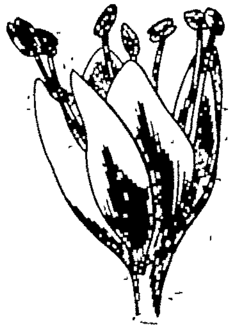

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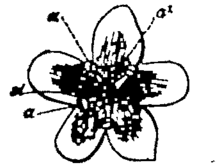
FIG 26—Side view of the long-styled flower of *Polygonum Fagopyrum*, two leaves of the perianth having been removed *n*, nectaries; *a*, anthers; *st*, stigmas. FIG. 27—Side view of the short-styled flower. FIG. 28—Side view of the flower of *Polygonum Bistorta* in its first period. FIG. 29.—Side view in its second period.



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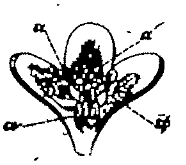

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FIG 30.—Flower of *Polygonum aviculare* viewed from above. *a*, outer anthers; *a*¹, inner anthers; *st*, stigmas. FIG. 31.—The same flower viewed laterally, two leaves of the perianth having been removed.

There is another curious point about the recorded facts. We have seen that more and less attractive flowers adapted to cross- or to self-fertilisation sometimes occur in slightly differing, sometimes in well-marked varieties, sometimes in doubtful, sometimes in good and distinct species.

If we believe the principle of evolution, and view species as originated from varieties, varieties as originated from slight individual differences, we may consider the recorded facts as presenting and explaining one of the many ways in which previously varying forms have been transformed by natural selection into different and diverging species.

HERMANN MULLER

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* Geranium, Stellaria, Cerastium, Rubus, Veronica, Carduus, Hieracium, and others.

*POLARISATION OF LIGHT**

II.

THE experiment described in the previous article, in which the rays reflected from the pile of glass plates are extinguished by the analyser when in one position, while those which have been transmitted are extinguished when the analyser is in a position at right angles to the former, shows that the vibrations of the reflected and refracted rays, so far as they become polarised, are at right angles to one another. And further, if these rays be severally examined with a plate of tourmalin, it will be found that the vibrations of the reflected ray are executed in a direction perpendicular to the plane of incidence, and those of the refracted ray in a direction parallel to that plane.

The same general reasoning as that used in the case of tourmalin plates will serve, if not as actual proof, at all events as illustration in this case. Thus, suppose that a ray whose vibrations are perpendicular to the plane of incidence, that is, parallel to the reflecting surface, fall upon a plate of glass; then there is no apparent reason why a change in the angle of incidence should modify the reflection and refraction, so far as they depend directly upon the direction of the vibrations. The vibrations cannot undergo any change of direction on one side rather than on the other by incidence on a surface to which they are parallel, and will consequently remain parallel to themselves even when the incidence has taken place. And since the reflected and refracted rays both lie in the plane of incidence, the vibrations (which are perpendicular to that plane and consequently to every line in it) will fulfil the optical condition of being perpendicular to the rays in question. But if the vibrations of the incident ray take place in the plane of incidence, it is difficult to conceive that the results of reflection and refraction should be unaffected by a change in the angle of incidence. There are two mathematical and mechanical principles which, when applied to the case of vibrations in the plane of incidence, lead to the conclusion that if the ray be incident at such an angle that the reflected and refracted rays are perpendicular to one another, there can be no reflected ray.

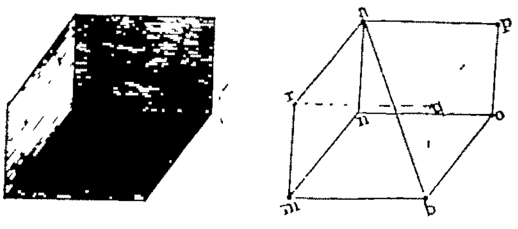

FIG 9.

A general explanation of this very curious result seems difficult; but the following considerations may perhaps tend to elucidate the subject. Reflexion is generally, perhaps always, accompanied by refraction. Bodies are visible in virtue of rays which, after reflexion from their surface, meet the eye. But the natural colours of bodies so seen are due to rays which are not reflected until they have penetrated to some, although inconsiderable, depth below the actual surface. During this penetration the light has been deprived of certain of its component rays, and emerges as a reflected beam covered with the remaining or complementary tint. And although the colourless reflexion from polished surfaces is an apparent exception to the rule, it may still be the fact that this is only a limiting case in which the penetration is a minimum. If this be so, we may fairly conclude that refraction is the ruling feature of the phenomenon, and that it in some sense precedes reflexion. With the change of direction of the ray involved in refraction it is in the highest degree probable that a change of direction of the vibrations (supposed always to be in the plane of incidence) will be also involved. The simplest supposition would be that the vibrations within the medium are perpendicular to the refracted ray; and that the intensity of the reflected light is due to that part of them which can be resolved in a direction perpendicular to that of the reflected ray. If, therefore, the refracted and the reflected rays be perpendicular, so also will be their vibrations, and consequently no part of the vibrations constituting the former can be resolved in the direction requisite for the latter. In other words there will be no reflected ray.

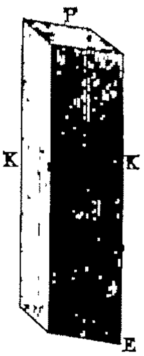

FIG. 10.

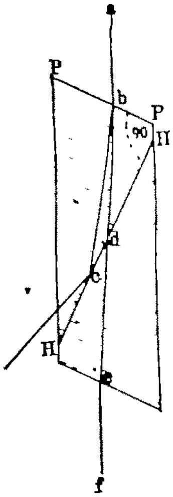

FIG 11.

The above remarks give, it must be admitted, no mechanical theory of reflexions, nor indeed do they pretend to be even a rough explanation of the facts. They merely amount to this: If reflexion depends primarily upon refraction, and the known law of reflexion obtains independently of all questions of polarisation, then when the incident vibrations take place in the plane of incidence no reflected ray, whose direction is perpendicular to that of the refracted ray, can be produced.

We next come to the subject of polarisation by double refraction. There are a large number of crystals which have the property of generally dividing every ray which passes through them into two. But the extent of separation of the two rays varies with the direction of the incident ray in reference to the natural figure of the crystal.

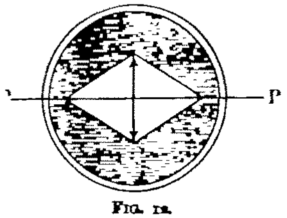

FIG. 12.

In every double refracting crystal there is at least one, and in many there are two, directions in which no such separation takes place. These directions are called optic axes. The relations between the forms of crystals and their optic axes, and optical properties arising therefrom, will be explained later.

Of such crystals Iceland spar is the most notable instance. If we take a block of such spar split into its natural shape, a rhombohedron, Fig. 9, and for convenience cut off the blunt angles by planes perpendicular to the line joining them, a b, it will be seen that a ray of light, transmitted perpendicularly to these planes, that is parallel to the line joining the blunt angles, is not divided In fact, the image either of the aperture of the lantern projected on a screen, or of an object seen by the eye in the direction in question, appears single, as if passed through a block of glass. The direction in question (viz., the line a b itself, and all lines passing through any part of the crystal parallel to a b), is called the optic axis of the crystal. If, however, the crystal be tilted out of this position in any direction, it will be seen by the appearance of two images instead of one, that the rays are divided into two. The angular divergence of the two sets of rays, or what comes to the same thing, the separation of the two images, depends upon the angle through which the crystal has been turned; or, as it may also be expressed, upon the angle between the directions of the incident ray and the optic axis of the crystal. When this angle amounts to a right angle, the separation is at its greatest; and if the crystal be still further turned, the images begin to come together again until, when it has turned through another right angle, they coincide.

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* Continued from p. 129.

This process of separation, or doubling the rays, is called double refraction. And the following experiment will show that one set of rays follows the ordinary law of refraction, while the other follows a different law. The image produced by the first set of rays is, in consequence, called the ordinary, and that produced by the second the extraordinary image. Let us now take a sphere of Iceland spar, which will act upon the rays issuing from the lamp as a powerful lens. In every position in which it is placed it produces two images on the screen; but in that in which I now place it the two images are concentric, differing only in this, that one is larger than the other. The direction in which the light is now passing is that of the optic axis; and it is to be observed that, although there is a difference in the magnifying of the two images, there is still no divergence of rays, or separation of images in the sense used before. In fact, if we suppose the curvature of the lens to be gradually diminished, we should find the difference of the sizes of the two images, as well as the absolute size of both, diminish; until when the surfaces of the lens became flat, the difference would vanish, and the two images would absolutely coincide.

This difference in the size of the images shows, moreover, a very important property of double refracting crystals. The amount of refraction produced by a transparent medium standing in air depends, as is well known, upon the velocity with which a ray of light traverses the medium compared with that with which it traverses air. The smaller the velocity in the medium, the greater the refraction. The greater the refraction, the greater the magnifying power of a lens constructed of that medium. Hence in the two concentric images we can at once point to the system of rays which has traversed the crystal at a lower velocity than the other.

Let us now turn the crystal round into some other position, so that the direction of the optic axis shall no longer coincide with that of the rays from the lamp or from the object. During this process one of the images, the larger, remains stationary, as would be the case with the single image, if we had used a sphere of glass. This, therefore, is the ordinary image. The other shifts about, separating itself from the first, until the crystal has been turned through half a right angle, and then drawing back again until the crystal has swept round through a complete right angle. This is, consequently, the extraordinary image.

It will be noticed that when the sphere has been turned through a right angle, the extraordinary image is no longer circular, but elliptical, and that the major axis of the ellipse lies in the direction in which the motion has taken place, that is, perpendicular to the axis about which the sphere has been turned. This is due to the fact, shown above, that the nearer the direction of the incident rays to that of the optic axis, the less the divergence between the ordinary and the extraordinary rays. The distortion of the image when the sphere has turned through half a right angle is due to the difference of angles between the optic axis and the rays which enter the crystal on one side and on the other of the central ray of the beam coming from the lamp.

That the rays forming each of the images are polarised, and that the direction of their polarisation is different, is easily shown by interposing a plate of tourmalin or other polarising instrument between the lamp and the sphere of spar. But inasmuch as the polarisation in many positions of the sphere is far from uniform, the phenomenon becomes rather complicated; and the character of the polarisation of the two images is better studied by using flat instead of curved surfaces for separating the rays.

For the purpose in question there is, perhaps, no better instrument than the double-image prism. This consists of a combination of two prisms, one of Iceland spar, so cut that the optic axis is parallel to the refracting edge; the other of glass, and usually having a refracting angle equal to that of the spar. The rays passing through the crystal prism being perpendicular to the optic axis, undergo the greatest separation possible. And the chromatic dispersion caused by that prism is corrected or neutralised entirely in the case of the extraordinary, and nearly so in that of the ordinary ray, by the glass prism which is placed in a reversed position. In this arrangement the extraordinary image occupies the centre of the field, and remains fixed while the double-image prism is made to revolve in a plane perpendicular to the incident rays; while the ordinary image is diverted to a distance from the centre, and revolves in a circle about that centre, when the prism revolves.

If the nature of the light in the two images thus formed be examined by any polarising instrument, it will be found to be polarised in both cases; but that the vibrations in the one image are always perpendicular to those in the other. And in particular the vibrations in the extraordinary image are parallel, and those in the ordinary are perpendicular to the optic axis.

On these principles polarising and analysing instruments have been constructed by various combinations of wedges or prisms of Iceland spar, the details of which it is not necessary to describe in full. But the general problem, and object proposed, in all of them has been to cause such a separation of ordinary and extraordinary rays, that one set of rays may, by reflexion or other methods, be further diverted and afterwards thrown altogether out of the field of view. This done, we have a single beam of completely polarised light and a single image produced from it.

One such instrument, however, the Nicol's prism, on account of its great utility and its very extensive use, deserves description. A rhombohedron of Iceland spar double of its natural length is taken (see Fig. 10); and one of its terminal faces P, which naturally makes an angle of 71° with the blunt edges K, is cut off obliquely so as to give the new face, say P' (not given in the figure), an inclination of 68° to the edges K. The whole block is then divided into two by a cut through the angle E in a direction at right angles to the new face P'; the faces of this cut are then carefully polished, and cemented together again in their original position with Canada balsam. Fig. 11 represents a section of such a prism made by a plane passing through the edges K (Fig. 10). A ray entering as a b is divided into two, viz., b c the ordinary, and b d the extraordinary. But the refractive index of the Canada balsam is 1·54, *i.e.* intermediate between that of the spar for the ordinary (1·65) and the extraordinary (1·48) rays respectively; and in virtue of this the ordinary ray undergoes total reflexion at the surface of the balsam, while the extraordinary passes through and emerges ultimately parallel to the incident ray. Fig. 12 shows an end view of a Nicol's prism, the shorter diagonal in the direction of vibration of the emergent polarised ray.

Two such instruments, when used together, are respectively called the "polariser" and the "analyser," on account of the purposes to which they are put. These, when placed in the path of a beam of light, give rise to the following phenomena, which are, in fact, merely a reproduction in a simplified form of what has gone before.

When polariser and analyser are placed in front of one another, with their shorter diagonals parallel, that is, when the vibrations in the image transmitted by the one are parallel to those in the image transmitted by the other, the light will be projected on the screen exactly as if only one instrument existed. If, however, one instrument, say the analyser, be turned round, the light will be seen to fade in the same way as in the case of the tourmalin plates; until, when it has been turned through a right angle, or as it is usually expressed, when the polariser and analyser are crossed, the light is totally extinguished.

In the complete apparatus or polariscope, we may incorporate any system of lenses, so that we may make use of either parallel or convergent light, and finally focus the image produced upon the screen or upon the retina. At present we shall speak only of the phenomena of colour produced by crystal plates in a parallel beam of polarised light—chromatic polarisation, as it is called, with parallel light.

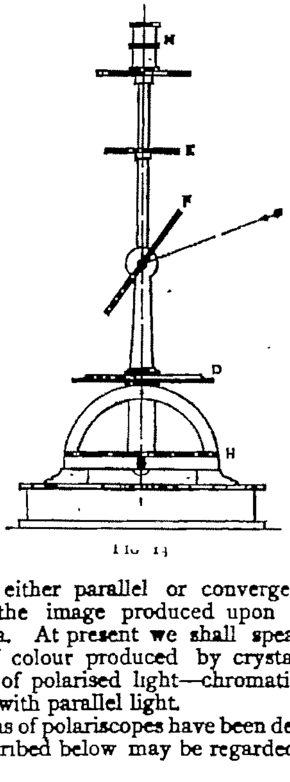

FIG. 13

Various forms of polariscopes have been devised, whereof the three described below may be regarded as the most important.

Fig. 13 is an elevation of one of them. When used in its simplest form, the frame F carries a plate of black glass which is capable of revolving about pivots in the uprights. The positions of the source of light and of the frame must be adjusted so that the plate will receive the incident light at the polarising angle, and reflect it in the direction of the eye-piece which contains a Nicol or other analyser. The objects to be examined are to be placed on the diaphragm E.

This instrument may be converted into another form, due to Norremberg, by placing a silvered mirror horizontally at H. The plate of black glass must be removed from the frame F, and a plate of transparent glass substituted for it, which must be so inclined that the light falling upon it shall be reflected at the polarising angle perpendicularly towards the horizontal mirror. The object may be placed on the diaphragm E as before. But it may also be placed on the diaphragm D below the polarising plate F, and in that case the eye will receive the polarised ray reflected from the mirror; and the polarised ray will have passed, before it reaches the eye, twice through the crystalline plate placed between the mirror and the polariser. The result is the same as if, in the ordinary apparatus, the polarised ray had passed through a plate of double the thickness. If the plate does not fill the entire field of view two images of the plate will be seen, the one larger, as viewed directly, the other smaller, as viewed after reflection from the horizontal mirror; the first will show the tint due to the actual thickness of the crystal, the other that due to a plate of the same crystal, but of double the thickness.

A further modification of this instrument will be described hereafter.

W. SPOTTISWOODE

(*To be continued.*)

GALILEO'S WORK IN ACOUSTICS

IN looking through the "Dialoghi delle Nuove Scienze" of Galileo, I came unexpectedly on a passage* containing two remarkable discoveries in acoustics, which I should have confidently referrred to a much later age. For the sake of such of your readers as may share the same erroneous impression, I hope you will allow me to give, in NATURE, a short account of these results.

The first is a perfectly accurate explanation of the phenomenon called "resonance." Every pendulum has a fixed period of oscillation peculiar to itself. Even when the "bob" is of considerable weight it is possible to set it swinging through a large arc by merely blowing against it with the mouth, provided the successive puffs are properly timed with reference to the pendulum's period of vibration. In the same way a single ringer can, by regular pulling, throw the heaviest bell into oscillations of such extent as to be capable of lifting half-a-dozen men who should hang on to its rope, off the ground all together. When a string of a musical instrument is struck, its vibrations set the air in its vicinity trembling, and the tremors thus set up spread themselves out through space. If they fall on a second wire in unison with the first, and therefore prepared to execute its vibrations in the same period, the effects of the successive impulses are accumulated, and the wire's oscillations can be distinctly seen to go on dilating until they have attained an extent equal to those of the wire originally struck.

Anyone who looks into the chapter on resonance in the "Tonempfindungen" will see that the account of the phenomenon given by the greatest living acoustician is, in principle, identical with that of Galileo.

The second point to which I wish to draw attention is an experiment involving the earliest direct determination of a vibration-ratio for a known musical interval. Galileo relates that he was one day engaged in scraping a brass plate with an iron chisel, in order to remove some spots from it, and noticed that the passage of the chisel across the plate was sometimes accompanied by a shrill whistling sound. On looking closely at the plate, he found that the chisel had left on its surface a long row of indentations parallel to each other and separated by exactly equal intervals. This occurred only when the sound was heard; if the chisel traversed the surface silently, not a trace of the markings remained. It was found that a rapid passage of the chisel gave rise to a more acute, a slower to a less acute, sound, and that, in the former case, the resulting indentations were closer together than they were in the latter. After repeated trials two sets of markings were obtained which corresponded to a pair of notes making an exact fifth with each other; and, on counting the number of indentations contained in a given length of each series, it appeared that for 30 of the lower sound there were 45 of the higher, which numbers are in the exact proportion (2 : 3), which connects the lengths of two equally tense wires, giving that interval. Galileo, who had felt a tremor pass from the chisel to his hand at each experiment, inferred that what really determined a musical interval was the ratio of the numbers of vibrations performed in equal times by its constituent notes, and that that ratio was inversely as that of the lengths of the wires producing them. In order to bring out the crucial nature of his experiment, he goes on to remark, with extreme acuteness, that there was, prior to it, no reason for regarding the relations known to connect musical intervals with the *lengths* of wires as in any exclusive sense *representing* such intervals. With equal propriety might the ratio of the *tensions* under which two wires of equal lengths emitted sounds forming an interval be taken as its representative. In this case we should obtain the inverse square root of the ratio resulting from the former mode of comparison. Thus Galileo's experiment alone supplied decisive ground for concluding that the relations of *length* between similarly circumstanced wires, likewise governed those of *period* between corresponding aerial vibrations.

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* Opere complete di Galileo Galilei. Vol xiii. pp 97–110 (Firenze.)

Prof. Tyndall, in referring to the above experiment, has described it as performed "by passing a knife over the edge of a piastre" ("Sound," 2nd ed., p. 51). This is an obvious mistake caused by incorrect translation. Galileo was scraping "una piastra d'ottone," *i.e.*, not "a piastre," but "a plate of brass." An excellent numismatist assures me that the *material* mentioned is alone decisive of the point, the piastre in Galileo's time being invariably made of *silver*.

SEDLEY TAYLOR

THE HOOSAC TUNNEL

THE following facts respecting the Hoosac tunnel, in which the borings from east and west communicated on Nov. 28, may prove of interest. The mountain penetrated is part of the chain of mountains that skirts, at a distance of two or three hundred miles inland, the Atlantic coast of the United States; of which the Blue Ridge in Virginia, the Alleghanies in Pennsylvania, the Catskills and Adirondacks in New York, the Green Mountains in Vermont, and the White Mountains in New Hampshire, are prominent examples. Hoosac Mountain has two summits, the eastern being 2,210, and the western 2,508 ft. above tide-water.

The enterprise has been the subject of various undertakings by different contractors, and the greater part of the earlier work during the years from 1848 to 1863, in length but one-twelfth of the whole distance, was on a smaller scale than the subsequent plan adopted, and had to be much enlarged and strengthened. The present contract requires a clear width of bore of 24 ft. and a height of 20 ft.; the total length of the tunnel is 25,031 ft. A central shaft pierces it from above, at a distance of 12,837 ft. from the eastern, and 12,194 ft from the western portal. The shaft has a depth of 1,038 ft., and is of elliptical form, its major axis is 27 ft. being coincident with the line of the tunnel; its minor axis is 15 ft. The grade of the tunnel slopes up to the shaft from both ends, with a rise of 26$\frac{1}{10}$ per mile. The shaft is not placed at the lowest point between the two summits of the mountains, as the exigencies of the work at the western extremity, and the presence of a stream of water at the point of lowest depression, made a site half a mile nearer the western portal preferable. The tunnel is 767 ft. above tide-water at its extremities. The temperature within averages 58° F.

The total excavation is about 1,000,000 tons of rock, requiring somewhat over 1,450,000 days' work. The boring was principally through mica schist, similar to that of the surface. The miners found it lying on the edge of the foliations and disposed to hang together after the blast. They compared the operation of working in it to pulling boards endwise from a pile of lumber. Rock of this character was found continuous until a point was reached within about 5,000 feet west of the central shaft. At that point the proportion of mica was diminished and the rock began to lose its foliated structure, becoming more homogeneous or granitic. In fact it might be characterised in general terms as granite with the ingredients differently proportioned at different localities, in some places feldspar, in some mica, and in others quartz predominating. This rock was harder to penetrate with the drills, but broke out more satisfactorily with the blast than the mica schist.

The chief trouble was occasioned by what received the name of "demoralised rock." This was rock saturated with water, which, exposed to air, disintegrated into mere mud, rendering the support of masonry absolutely necessary. The tunnel will not probably be ready for railway traffic before next July, as there is yet much work to be done, the total cost at that date, it is estimated, will not fall short of 12,500,000 dols.

NOTES

ON Monday last the French Academy of Sciences named Mr. J. Norman Lockyer, F.R.S., one of its Correspondents, to fill the place rendered vacant in the Astronomical Section by the death of Encke. We believe that the following is a complete list of the English scientific members of the French Institute at the present time:—Foreign Members—Prof. Owen, Sir C. Wheatstone. Correspondents: Geometry—Prof. Sylvester. Mechanics—Sir Wm. Fairbairn. Astronomy—Sir G. Airy, Mr. Hind, Prof. Adams, Prof. Cayley, Sir Thomas MacLear, Mr. Lockyer. Geography and Navigation—Admiral Richards, Dr. Livingstone. Physics—Dr. Joule. Chemistry—Dr. Frankland, Dr. Williamson. Mineralogy—Sir C. Lyell, Prof. W. H. Miller. Botany—Dr. Hooker. Anatomy and Zoology—Dr. Carpenter.

AT the meeting of the Paris Academy of Sciences, which took place on December 22, the places of Correspondents in the Physical Section, vacant by the death of M. Hansteen, and the election of Sir C. Wheatstone to a foreign associateship, were filled up by the election of MM. Angstrom and Billet.

HER MAJESTY'S Commissioners have resolved to commence, in connection with the series of international exhibitions, permanent collections which shall illustrate the ethnology and geography of the different portions of the British dominions, and ultimately form a great national museum of the empire upon which the sun never sets. They will be arranged for the present in the galleries of the Royal Albert Hall. Many portions of the empire are inhabited by aboriginal races, most of which are undergoing rapid changes, and some of which are disappearing altogether. These races are fast losing their primitive characteristics and distinguishing traits. The collections would embrace life-size and other figures representing the aboriginal inhabitants in their ordinary and gala costumes, models of their dwellings, samples of their domestic utensils, idols, weapons of war, boats and canoes, agricultural, musical, and manufacturing instruments and implements, samples of their industries, and in general all objects tending to show their present ethnological position and state of civilisation. It is proposed to receive for the Exhibition of 1874 any suitable collections, which will be grouped and classified hereafter in their strict ethnological and geographical relations. As, however, there is at present great public interest in the various tribes inhabiting the West Coast o

Africa, including the Ashantees, with whom this country is at war, all objects relating to the Ashantees, Fantees, Dahomeys, Houssas, and the neighbouring tribes are especially desired. The Indian Empire, the Eastern Archipelago, and the islands of the southern hemisphere, are also able to afford abundant and valuable materials for the proposed museum, of which it is believed that the nucleus can be formed at once from materials in private collections. Her Majesty's Commissioners confidently appeal to the civil, military, and naval officers of the British service throughout the Queen's dominions to assist them in these collections. Her Majesty's Commissioners have secured the services of eminent gentlemen to advise them from time to time in giving effect to these intentions. It is requested that offers of gifts and loans of objects should be made known at once to the Secretary of Her Majesty's Commissioners, Upper Kensington Gore, London, S.W.

IN reference to recent communications on the rate of stalagmitic deposit, Mr. Thomas K. Callard writes to say that he thinks the probability is that the rate of deposit in Kent's Cavern was not uniform, "for, when the thick forest (the habitat of the animals whose bones are found in the cave) left an accumulation of decayed vegetation on the soil, we had the natural laboratory where the rain would find the carbonic acid, to act as a solvent upon the calcareous earth, and as this acidulous liquid percolated through the soil and dripped into the cave, we have the origin of the stalagmite; but as, by the axe of man, the forest decreased, in that proportion the chemicals lessened, and as a consequence the deposit diminished. Besides the diminution of the solvent, every year that the operation was going on the material that composed the stalagmite must have been decreasing in the superjacent soil, so that the bicarbonate of lime which now takes two centuries to cover one-eighth of an inch, might have been, in days gone by, the work of much shorter time." Mr. W. Bruce Clarke writes that he visited, about ten years ago, a cavern near Buxton, commonly known as "Poole's Hole," and observed some stalagmite, probably ⅛ in. in the back, had become deposited upon the gas-pipes, which were used to light the cave, and had been laid down six months before. At this rate, granting that the deposit had been six months in acquiring a thickness of ⅛ in., 1 in. would be deposited in four years, a rate of deposit even more rapid than that (viz. ⅛ in. in fifteen years) mentioned by Mr. Curry in the number of NATURE for December 18. It must be remembered, however, that though at one particular spot in "Poole's Hole," 1 in. of stalagmite might be deposited in four years, the same rate would probably not be maintained all over the cave.

THE Sub-Wealden Exploration has proved far more expensive than was at first anticipated, and additional funds will be required to complete the desired depth of 1,000 ft. A third sum of 1,000*l.* has now been promised, and this will form the basis for future operations. This amount includes 200 *l.* from the Duke of Devonshire, 100*l.* from Lord Leconfield, 50*l.* from the Earl of Ashburnham, 50*l.* from the Royal Society, and 25*l.* from the Duke of Norfolk. These sums will be collected as the work proceeds, and additional contributions are solicited. The importance attributed to the enterprise by Professor Phillips in the Geological Section, during the last meeting of the British Association at Bradford, is an additional proof, if any were needed, of the expediency of completing the investigation.

PROF. OWEN, who is suffering from a troublesome bronchial affection, is spending the winter in Egypt.

MR. J. ALLEN, of Clifton College, has been elected to the Natural Science Exhibition at St. John's College, Cambridge (50*l.* per annum tenable for three years). The examiners reported that the merits of Mr. Lodge were very nearly equal to those of the successful candidate. There were ten candidates.

THE Caspian Sea is extremely rich in various species of fish, many of these occurring in prodigious numbers. Indeed, according to Alexander Schultz, the yield is very much greater than that of the Great Bank of Newfoundland. Thus in one single district 15,000 sturgeon are frequently taken in a day, and when the fishing is interrupted for twenty-four hours the waters become almost choked by the abundance of fish, which are so numerous as to press each other out upon the shore. The total yield of the Caspian Sea for one year in fish and fish products has been estimated at 13,000,000 *pouds* (about 469,430,000 pounds avoirdupois), worth about 12,000,000 dols. There are several varieties of sturgeon among the fish taken, including the sterlet, as well as the carp and other cyprinoids, the salmon, the *Coregonus* (similar to the white-fish of the American lakes), several kinds of herring, &c. A peculiar phenomenon observed especially among the sturgeon is that of a kind of winter sleep. At the approach of cold weather they seek the deep portion of the rivers, and remain there in a state of torpor, during which they secrete a viscid matter which forms a coating over the entire body, called by the fishermen a *pelisse*. During this period they appear to eat nothing, their stomachs always being found entirely empty.

MR. DALL, of whose movements as a surveyor and explorer in the Aleutian Islands in behalf of the Coast Survey we have advised our readers from time to time, returned on the 8th Nov. to San Francisco, where he will spend the winter in preparing his report to Prof. Peirce. Part of his labours had special reference to the selection of a suitable locality for an intermediate land station for the proposed Pacific cable between the United States and Japan. Mr. Dall expects to return in the spring to finish his explorations on the islands.

AMONG recent discoveries of valuable minerals in Australia is that of iron in the form of magnetic iron, and brown hematite at Wallerawang, Victoria, in close proximity to limestone, fire-clay, coal, and a railway station.

THE Italian Scientific Commission, appointed to examine, from an anthropological point of view, the remains of the Italian poet Petrarch, and to publish the result of its observations at the centenary of the great poet, proceeded, we learn from *La Nature*, in the beginning of December to open the urn of red granite, amid a large gathering of people. The bones, instead of being contained in a coffin of wood or metal, were spread upon a simple plank, and were of an amber colour, moist, and partly mouldered. The cranium, of medium size, was intact, the frontal bone much developed. The jaws still contained many teeth, among which were a number of molars and incisors very well preserved. The orbits were very large. Nearly all the vertebræ and ribs were found. The bones of the pelvis were in good condition, as also the scapula, the humerus, and the other bones of the arms; the apophyses of the femur were very prominent. There was discovered also a quantity of small bones which probably composed the hands and the feet. The vestments were reduced to a dark powder. From the size and length of the bones, we may conclude that Petrarch was a man of middle height and robust constitution.

AT one of the last sittings of the French Academy of Medicine, says *La Nature*, M. Devergie read a remarkable report on the prize of the Marquis d'Ourches, a prize of 25,000 francs, to be given to the man who should discover an infallible method of recognising certain death. The method must be so simple as to be at the command of the most illiterate and rude. Besides this prize, the testator instituted another of 5,000 francs for the discovery of a scientific method of arriving at the same result. The value of the prize of 25,000 francs has tempted people of all classes and all conditions; thus the Academy has received 102 memoirs, not counting those which arrived after the expiration of the time announced for their reception. Of these 102 memoirs, only 32 were judged worthy of serious examination. But no one has gained the famous prize of 25,000 francs, which therefore reverts to the testator's family. As to the prize of 5,000 francs, it will probably be divided among various competitors who have presented interesting memoirs.

AT the Annual Meeting of the Institution of Civil Engineers, held on December 23, it was stated that on the 30th November last, the number of members and associates was 1,994. On the subject of finance, it was stated that during the last fourteen years the savings had amounted to something like 2,000*l.* per annum, on the average. The receipts are now nearly 9,000*l.* per annum, while the ordinary expenditure was only 6,000*l.* per annum. What with trust funds, investments, and cash balance, the Institution has 30,233*l.* 8*s.* 6*d.* at its disposal. The library numbers 10,443 volumes.

WE would draw the attention of our London readers to the advertisement in this week's NATURE with regard to the Junior Philosophical Society, meeting at 6A, Victoria Street, S.W. We believe we have had occasion to speak of it before, as one whose object and method of work are commendable.

THE January number of Petermann's *Geographischen Mittheilungen*, contains a contribution by Dr. Nachtigal giving valuable details concerning the various Pagan tributaries to the kingdom of Baghirmi. Dr. Meyer gives some statistics of the inhabitants of the Philippine Islands, whose number he estimates at 7,451,352. In the same number is a communication from Dr. Miklucho-Maclay, dated Batavia, October 25, 1873, in which he maintains that the Papuas and Negritos belong to the same race, notwithstanding that the former are dolichocephalic, and the latter brachycephalic.

THE principal article in Guido Cora's excellent Italian Geographical Journal *Cosmos*, is on "Recent Expeditions to New Guinea."

THE "Second Report of the Committee on Boulders appointed by the Royal Society of Edinburgh," contains much interesting information which will no doubt be ultimately of service to geologists.

WE have received the first number of *The Argonaut* (Hodder and Stoughton), a journal started by "a number of young fellows who are just entering on the bolder thoughts or the more active duties of manhood," for the purpose of discussing questions in which all earnest young men take an interest. It professes to be devoted to no party either in religion, politics, or philosophy. It is edited by Mr. George Gladstone, F.C.S., and this first number contains an Introduction by Dr. Gladstone, F.R.S. The contents are varied and mostly interesting.

THE Opening Address to the Geological Association, by the president, Mr. Henry Woodward, F.R.S., has been printed as a supplemental number of the Proceedings. The Address is a survey of what has been done in geology during the past twelve months.

"THE Glaciation of the Northern Part of the Lake District," is the title of a paper by Mr. J. Clifton Ward, reprinted from the *Quarterly Journal of the Geological Society*.

THE *Mémorial Diplomatique* states that the Italian Consul at the Piræus has informed his Government that M. Théodore Tubini, banker, at Athens, has obtained a concession for cutting a canal through the Isthmus of Corinth. The principal clauses of the concession are that the canal shall have a *minimum* depth of 8½ metres (27 ft.), and a width of 12 metres (39 ft.) at the bottom. Half-way through the canal is to be a dock of 30,000 square metres in extent, and of sufficient depth to receive the largest vessels. The canal is to be completed in six years. The concession is for 99 years, and a deposit of 12,000*l.* is to be paid immediately after the Greek Parliament has approved the concession. The estimated cost of the undertaking is 800,000*l.*

THE principal papers in No. 39 of the *Journal of the Scottish Meteorological Society*, are "The Report of the Committee appointed to investigate the Relation of the Herring Fishery to Meteorology," an abstract of which has been given in our report of the Society's meeting, and a valuable paper by the Rev. W. Clement Ley, "On the Mean Inclination of Winds towards the Lower Isobarics." The Journal contains, as usual, the admirably compiled quarterly Meteorological returns from the Society's numerous stations.

WE have received a reprint from the "Proceedings of the Geologists' Association" of Mr. Henry Hick's paper on the "Classification of the Cambrian and Silurian Rocks.

PART III. of vol. xxii. of the "Transactions of the North of England Institute of Mining and Mechanical Engineers" consists entirely of an elaborate and valuable paper on the geology of the Redesdale ironstone district, by Mr. G. A. Lebour, of the Geological Survey. It is accompanied by two useful maps of the district.

AN aerolite, *Iron* says, weighing about twelve pounds fell in the vicinity of Marysville, Cal., on the 24th of August, which was so hot that it could not be handled for some time. It came crashing through the tree tops with a bright flash, and was found buried eight feet in the ground.

THE additions to the Zoological Society's Gardens during the past week include an Asiatic Wild Ass (*Equus onager*) from S.W. Asia, presented by Capt. H. L. Nutt; an Anubis Baboon (*Cynocephalus anubis*) and a Patas Monkey (*Cercopithecus ruber*) from W. Africa, presented by Mr. A. E. Oakes; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. F. E. Bradley; a Hybrid Duck (between ♂ *Aix sponsa* and ♀ *A. galericulata*), presented by Mr. J. C. Parr; a Yarrell's Curassow (*Crax yarrelli*) from S.E. Brazil, and a Coypu (*Myopotamus coypus*) from S. America, purchased.

SCIENCE IN KÖNIGSBERG

WE have before us the *Schriften der Koniglichen Physikalisch-Okonomischen Gesellschaft zu Königsberg*, for 1871-72, in which is to be found a considerable amount of useful scientific observations, both of local and general interest. Dr. Berendt, who, along with some coadjutors, has been engaged in preparing a full geological map of Prussia, and in other geognostic researches, describes a specimen of immature amber brought from the sea-bottom on the Samland coast. Under a wrinked and brittle crust, the resinous substance was soft, transparent, and highly elastic. From some similarity of physical properties (not complete, however), Dr. Berendt inclines to identify it with a fossil resin found by Bergemann in brown coal of Lattorf, and described under the name of kranzit. The sp. gr. of the new substance is 0·934; it is insoluble in alkalies, spirit of wine, oil of turpentine, soluble in sulphuric acid; it begins to melt at 300°; in air it burns with a luminous sooty flame, giving a peculiar smell; it is free of sulphur, but contains a little nitrogen, like amber and some kinds of asphalte.

The same author has given much attention to the formation of amber in Prussia, and in an earlier number of the *Schriften* (1869, first part) will be found a very full investigation, by him, of the subject. In one of the present numbers he gives an account of preparations lately made for subterranean mining of the substance in Samland. Hitherto this method has not been adopted, and on two accounts chiefly; the nature of the superincumbent strata (which are generally sand and clay), and the high value of amber, which sufficiently repaid the other method. The Government, however, has lent some aid, and in July 1872 boring was commenced at the southern base of Carlsberg, where, at a depth of about forty-four metres, blue earth was found containing amber in abundance. This is about 5·7 m., or eighteen feet below the sea. The results were of a highly promising nature, and exceeded expectation.

The coast of Samland affords a good opportunity of studying the Algæ of the Baltic; and this forms the subject of a communication from M Caspary. It is known that the water of the Baltic contains a much smaller proportion of salts than that of the North Sea or Atlantic. According to a recent analysis by Von Behr, the quantity was only 0·6766 per cent. To this fact, chiefly, and also to the fact of a colder climate, M. Caspary attributes the much smaller number of species of Algæ in the Baltic than on the English coast. He enumerates only twenty-five from the Prussian coast, whereas, at Falmouth, in Cornwall, 176 different species have been found. The water of the Atlantic contains about four per cent. of salt, or nearly seven times more than the Baltic water.

We further note, in the department of Botany, a paper in which Dr. von Klingraff describes the species and varieties of Sphagnum found in Prussia. In referring to the colouring of the leaves as a means of characterisation, he points out that the red and yellow colours almost always exclude each other. Red is found in only three species; *S acutifolium, tenellum* and *cymbifolium*; and each of these has a purple-red variety. On the other hand, yellow is wanting in the first two, and the variety *congestum* of the third is the only known example in which the red and yellow co-exist in forms of the same species.

Among the various organic remains found in amber, those of molluscs are peculiarly rare. It might have been expected that the liquid resinous matter would more readily surprise such animals than running or flying insects (which are abundant), while the shell, after death of its tenant, would offer a longer resistance to destruction than an unprotected body. It would be rash to conclude that the amber forest contained as few molluscs as our present exclusively pine forests; and botanists have shown that other trees than those of the pine species must have been present. In these mixed forests there were doubtless numerous molluscs, and we are led to suppose that the resin-producing trees were carefully avoided by them. Such is the view given by M. Kunow, who describes two snail shells found in amber, and probably belonging, he thinks, to the genus *Helix*. Only three previous notices of similar discoveries has he met with; and among the 13,000 organic remains of amber in the Society's collection, there is no piece of the kind in question.

Dr. Buchholz furnishes an account of the Hansa Arctic Expedition, and many interesting particulars as to the forms of life observed in the North Polar regions.

The anatomical collection in the University at Konigsberg contains three bear skulls found in the province. These are described at some length by M. Müller. They differ much in size and form, and it is striking that such different individuals of the same species should have lived so near one another (the places of discovery not having been more than 20 miles apart). A few similar bear skulls have been found in this country and in Ireland, and are described by Owen under the name of fen-bears.

It has been commonly believed that living trees struck by lightning are frequently consumed. In a paper on the effects of lightning on trees and telegraph posts, M. Caspary shows this is a mistake, and that the case is extremely rare. He cites 93 authenticated cases of trees having been struck; the species were as follows (and here also some common notions are disproved):—1 *Populus alba*, 2 *Pirus communis*, 2 *Ulmus*, 3 *Pinus picea* L., 3 *Betula verrucosa*, 3 *Fraxinus excelsior*, 12 *Pinus sylvestris*, 12 *Picea vulgaris* Link., 14 *Populus monilifera*, 15 *Quercus pedunculata*, 20 *Populus italica*. Several valuable experiments and results are detailed in this paper, of which, however, accounts may be found in English serials.

Another important paper in physics treats of the arrangements at a station for measuring ground temperatures in Konigsberg, and the correction of the thermometers there employed. It is by Dr. Ernst Dohrn.

Archæology claims a considerable share of the Society's attention; and there is one paper by Dr. Berendt, which specially deserves our notice. It enters very fully into the question of certain curious "face urns" which have been found in the region about Dantzic, &c. The forms of these articles are calculated to throw a good deal of light on the physiognomical features and the manners of the people that used them.

Konigsberg now numbers over 100,000 inhabitants, and the sewage question becomes urgent. Dr. Muller calls the attention of the Society to what is being done in other cities and countries, by way of improvement in this direction.

WELLINGTON N.Z. PHILOSOPHICAL SOCIETY

THE President, Dr. Hector, delivered his annual address before a meeting of members on Aug. 6, 1873. Dr. Hector in his opening remarks paid a tribute to the memory of Dr. Fred. John Knox, who had during a life-time contributed greatly to the science of comparative anatomy. Dr. Knox was an undoubted authority on all matters relating to the Cetacea, having made it his chief study. As one of the oldest members of the New Zealand Society he contributed largely and valuably to its transactions and the museum, which latter is specially indebted to him for the numerous contributions of anatomical preparations. The society, during its six years' existence, has gone on steadily increasing its members, who now number 142. Referring to vol. v. of the Transactions, Dr. Hector stated it contained forty-eight original papers, some of which possess a value from their originality of research which cannot fail to make the Transactions in future times important for reference.

Mr. T. Locke Travers' paper on the Life and Times of Te Raupara is a valuable page in the history of New Zealand, as the career of a man like Te Raupara is not merely of interest from its association with the early history of the colonisation of these islands, but affords a subject for study in connection with the more general historical question of the rapidity with which changes can be effected in uncivilised races, and the aptitude which they show in acquiring the arts, both peaceful and warlike, from colonists or conquerors as the case may be. Mr. Travers' contribution, valuable though it is, is but a small portion of the material relating to the Maori race which would find a fitting place in the Transactions of the Institute. The Maoris present a peculiarity of a mental type, the reason for which is not yet fully explained; as a race they show evidence of greater mental vigour than might have been expected in a people possessing no written knowledge. The facility with which they acquire our written language, and the delight which they take in exercising it, in reducing to writing their ancient *waiatas* (songs) and traditions is of itself a remarkable evidence of their vigour of mind. It does not appear, however, a reliable course in the collection of these songs to employ the Maori narrators to reduce them to writing, as it must be a process of translation of a most complex kind, and must lead to loss of accuracy both in matters of fact and in form of expression. A most interesting feature in the Maori language is the minute detail with which natural objects have been discriminated and named. He contrasted this with the North American Indians, who have only names for objects of immediate and practical utility in their affairs of every-day life. The Maoris, on the contrary, appear to have possessed a pure love of exercising their discriminating faculty; every tree or shrub, useful or useless, nearly every fish of large size or insignificant, and even many insects and lower forms of life that would remain unnoticed by most Europeans unless specially trained to the observation of such objects, have all their special names to the Maoris. The frequent reference made in their songs and traditions to these natural objects, invests them with a richness of imagery that adapts them for the poetical expression of sentiments and emotions that could only have been feebly if at all developed to the minds of the originators and narrators of those legends.

One of the most important events connected with this subject is the publication of the poem "Ranolf and Amohia," to the talented author of which all who love natural history must feel grateful for the abundant allusions which he has made to the characteristic features of the fauna and flora of the country, and the care which he has exercised in making his descriptions accurate. When a poet qualifies himself to appreciate the precise relations of the objects that enter into the scenes he depicts he will find that it is not necessary to sacrifice either facility or grace of expression in order to obtain the impressiveness which arises from strict accuracy. From this point of view Mr. Domett's poetical descriptions of the natural history of the new country cannot fail to aid in linking the sympathy of literature and fancy with the study of Science, and do good service to those objects which the society has most in view. The president also eulogised the efforts of a member of the Institute, Mr. G. H. Wilson, whose graceful and vigorous pen has been devoted to the rendering of those legends which relate to events that occurred in past time in the immediate neighbourhood of Wellington. The President referred to the papers of Messrs. Mantell and Taylor as bearing out his (Dr. Hector's) view of the recent date of the extinction of the moa. The discovery of a large bird of the *Anser* family, but which could not fly adds another remarkable feature to New Zealand's extinct ornithology. Chief among the additions which have been made to the zoological literature of the colony during the past year is Dr. Buller's great work on the Birds of New Zealand, which is to be rendered more complete by the publication of additional plates. The President expressed a hope that a second edition might be called for in order to give Dr. Buller an opportunity of bringing up the information to a still later date. The enumeration of our whales and dolphins communicated to the Society by Dr. Hector has already called forth critical remarks from the veteran zoologist, Dr. J. E. Gray, of the British Museum. The President expressed his opinion that the fur seals frequenting the South Island all belong to one species, *Arctocephalus cinereus*, although skulls of a second species (*A. lobatus*) are found in caves and Maori ovens. Captain Hutton's valuable addition to the list of fishes was also referred to, as also the successful introduction of salmon during the past year. Dr. Hector expressed an opinion in favour of introducing ova not only of salmon but of trout, white fish, and other species, that inhabit the inland waters of British Columbia. The catalogues of the Marine Mollusca, and the Star Fish of our coasts, prepared by Captain Hutton, will be found invaluable by collectors, but the most interesting contribution to the Zoology of New Zealand is Captain H.'s essay on the Geographical Relations of the Fauna, which to a great extent bears out the hypothesis advanced by Dr. Hector in a previous address, that the peculiar insular characters of the forms of life in New Zealand have been present from a very remote period.

The President referred to the expected visit of the *Challenger* on a scientific exploration of the Southern Seas, and expressed a hope that it would add largely to our knowledge on this interesting subject. Referring to the great Southern Continent, which is full of interest with its active volcanoes amidst perpetual snows, he stated it was likely that the ensuing year will add greatly to our knowledge of that land, which is only 1,200 miles distant from New Zealand, on whose climate it probably exerts a marked influence. This little known land possesses large supplies of guano, and according to Sir James Ross, has a large and undisturbed whaling ground near it, in which whales of several different species abound. The President criticised Captain Hutton's paper on the Glacial period of New Zealand, and confirmed his dissent from the theory of a submergence of the New Zealand area on a grand scale during the post-pliocene or post-glacial period, and stated that unless palæontological evidence of recent date can be obtained from strata occupying valleys that were eroded during the last extension of the glaciers he must still adhere to his formerly expressed opinion, that the geological period previous to that which may be termed the recent period in New Zealand was characterised by a prolonged though perhaps not excessive elevation; and that especially in the South Island there is in consequence a marked absence of marine drifts and tills. The President commended the study of the subject of our soils, surface drifts, and beach rocks to the members of the Society. He also differed from Capt. Hutton, who underrates, he thought, the erosive power of existing glaciers, and referred to the recent changes reported to have taken place in the outline of the summit of Mount Cook, owing to a great avalanche having slipped from the ridge, leaving a conspicuous gap in the formerly even tent-like form of the apex.

After referring to the Geological reports for the progress made during the past year in the survey of the country, the President stated that descriptive catalogues of fossils from the tertiary formations, as also an illustrated work on the fossil plants from the different coal-bearing formations are nearly ready for publication. The development of the wonderful reptilian fauna in the upper secondary rocks will afford subject for several communications at the meetings of the Society during the present session. Already at least seven distinct forms belonging to the genera Plesiosaurus, &c., have been worked out from the blocks of matrix collected at the Amuri Bluff (Marlborough) and at the Waipara, and the description of these gigantic Saurians will be sure to excite great interest in the study of geological structure by exciting discussion at home, and indirectly to attract attention to the mineral and other resources of the colony. Mr. Skey's contributions were also favourably reviewed by the President, who concluded by thanking the members for the courtesy and support which he had received. He then vacated the Chair in favour of Dr. Knight, the President for the present year.

SCIENTIFIC SERIALS

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, November 1873.—In order to give warning of approaching storms, an important determination is that of "barometric gradient" between two places, found by ascertaining the difference of atmospheric pressure, and dividing by the distance. But as the places may be at unequal heights above the sea level, the influence of this inequality on the barometric state must first be eliminated. This may be done by either of two methods; reduction to the sea level (the more common way), or determining the divergence of the observed barometric state from the average for several years. At the recent meeting of the International Congress of Meteorology at Vienna, the question came up, which method was preferable; and it was decided, that for stations not more than 300 metres above the sea, the method of reduction had advantages over the other. Dr. Hann here compares the two methods, and presents the grounds of the Congress's decision. Austria adopts, this year, the method recommended, in place of the other.—The paper is followed by one giving a sketch of the organisation for meteorological observations in France, under the direction of M. Le Verrier.—We further note some observations by M. Caloria, of Milan (communicated to the Istituto Lombardo), comparing the number of sun-spots with the temperature and rainfall during the period 1763–1872. The tables indicate pretty clearly an increase of heat with decrease of spots; though anomalies occur. In rainfall the connection is less marked. Among the other notes will be found information as to the climate of the Philippines, statistics of earthquakes in Austria, meteorites, &c.

Bibliothèque Universelle et Revue Suisse for November 1873, commences with a paper by M. Rahn on the Origin of the Renaissance in Italy. He considers the essential character of the art of that period to have been, that the works produced were no longer the product of a collective activity, but the creation of such and such a master. He also shows from the Palais Pitti at Florence, and other edifices (as compared with the Gothic style), how the sense of harmony in proportion was developed.—M. L. Piccard communicates the second part of a paper entitled "Poisons and Counter Poisons," giving here a clear popular account of the three different methods of remedy in cases of poisoning—mechanical elimination of the unabsorbed poison, neutralising of the poison by substances forming with it a harmless compound, and symptomatic treatment, dealing with the effects produced.—Mdlle. Anneville, criticising the public instruction in the United States, thinks history and literature, and æsthetical studies, are too much ignored.—M. Giardon has a review of some English works on Patagonia. The remaining papers do not specially call for notice here.

Bulletin de L'Académie Royale de Belgique, Nos. 9 and 10. This issue contains some interesting observations by Dr. Nuel, of Utrecht, on the electrical phenomena of the heart. Electrodes being applied, one to the apex, the other to the lateral face of an intact, fresh heart, beating regularly, there is, *in diastole*, a small current from the former to the latter, increasing with distance between the points. If the heart is exposed to air, however, the current is soon reversed. Any wounded part is negative to every other point on the surface. The circuit being closed between an intact surface and a transverse section, there is a considerable current from the former to the latter (greater than in ordinary muscles); this diminishes rapidly, but increases again somewhat, when a contraction has been excited. As to the phenomena *during contraction*, the weak current at the surface of the intact and fresh heart does not change; but if a strong current is obtained from lesion, this is weakened or reversed during systole. The negative variation precedes contraction; it reaches a maximum at commencement of systole, and lasts to the end of the contraction. The same author has experimented on the influence of the vagus nerve on the heart; and finds that stimulation affects the auricular contraction differently from the ventricular, implying the presence of different nervous elements. The nerve also contains some fibres which excite, instead of retarding, the heart's movements. In an investigation of the orbits of comets, M. Houzeau shows that the greater axes have a decided tendency to place themselves parallel to the double heliocentric meridian 104° 20′ and 282° 20′, and this longitude differs little from that of the point in space, towards which the solar system is found to be moving. —M. Plateau describes a parasite of the Belgian Cheiroptera. There is a short account, by M. Quetelet, of the proceedings of the recent international Congress of Meteorology held at Vienna; and

M. Van Rysselberghe's "universal meteorographic system" (which we lately noticed), is here described in full, with illustrations, and deserves the attention of meteorologists.

SOCIETIES AND ACADEMIES

Edinburgh

Royal Society, Dec. 22, 1873.—Sir W. Thomson, president, in the chair. At the request of the Council, Dr. Andrews gave an address on ozone. After giving a full *résumé* of the history of the discovery of the more important properties and relations of ozone, Dr. Andrews showed a number of beautiful experiments. Especially remarkable among these was a class-illustration of the contraction of oxygen by the silent electrical discharge. By the use of a new form of apparatus a diminution of volume was obtained, exceeding any hitherto recorded. Among the more remarkable of the new experiments shown was one quite recently made by the lecturer, proving that coarsely pounded glass, shaken in a vessel containing electrolytic oxygen, rapidly destroys the ozone reactions. This experiment forms a new link between a purely mechanical action and a chemical change, closer than any hitherto observed. The chairman, thanking the lecturer in the name of the Society, pointed out how very large a portion of all that we know about ozone is due entirely to the exquisite researches of Dr. Andrews.

Royal Physical Society, Dec. 17, 1873.—Dr. James M'Bain, R.N., president, in the chair.—The communications read were the following :—On a deposit of magnetic iron ore on the shores of Bute, by James Middleton, M.B. (with exhibition of specimens.) It seems that some time ago Mr. Cameron, of Rothesay, had noticed some remarkable kind of black sand on the beach at Bogany Point, at the entrance to Rothesay Bay. Being interested in it, he carried home a specimen, dried it, and made an examination of it, the result being that he found the sand to consist of almost pure magnetic iron-ore. Bogany Point is not the only part of Bute where it has been found, as it occurs at Kilmichael in the Kyles of Bute. An interesting circumstance, probably connected with this deposit, is that captains of small coasters in the neighbourhood say that they have noticed a divergence of the compass near the point where the principal deposit lies.—Experiments regarding the rate of deposition of sediment from fresh and salt water, by David Robertson, F.G.S. A simple way to illustrate the experiment of the precipitation in fresh and sea water is to take two small glass jars of equal size. Fill the two about four-fifths full, the one with sea and the other with fresh water; then fill both up with clay dissolved in fresh water—say about the consistence of cream—and stir both well up. Set the jars side by side to settle, and in a very short time the precipitation in the jar containing the sea-water will be seen to be going on rapidly, while in the jar with the fresh water little or no change will be observable. From these results, we can easily understand that whatever changes may have taken place relatively to land and sea from other causes, it does not appear that deposits from fresh water currents can be carried far seaward.—Note on the deposition of mud from various solutions, by Joseph Sommerville.

Manchester

Literary and Philosophical Society, Dec. 16, 1873.—E. W. Binney, F.R.S., vice-president, in the chair.—"Method of Construction of a New Barometer," by Dr. J. P. Joule, F.R.S., president. The condition of the instrument placed on March 18 in the Society's Hall proves that it is possible to use sulphuric acid on the top of the mercurial column without chemical action taking place. I have therefore proceeded to prepare other tubes with a view to test, by practical work, the merits of the new contrivance. A tube of about $\frac{1}{8}$ inch bore is selected. It is first cleaned by drawing a knotted string through it. It is then bent to the siphon shape; and near the longer end it is drawn to a capillary tube. It is then washed with nitric acid; afterwards with sulphuric acid. The sulphuric acid is then drained off. Mercury is then poured into the short limb. The end of the longer limb is then attached to my mercurial exhauster. On working this the mercury rises in the tube, and, being replenished by pouring it into the short limb, soon arrives at the height due to the atmospheric pressure. It carries with it the acid left adhering to its sides, so that after a few hours half, or, what is better, one third of an inch of acid stands above the mercury. Small bubbles of air are seen to arise; but by leaving the tube in connection with the exhauster for a day or two these finally cease. Mercury is then poured into the short limb until that in the longer rises nearly to the capillary part of the tube. This is then sealed and detached from the exhauster. Mercury is then removed from the shorter limb until it stands in the long one at a convenient height. Sulphuric acid is then introduced into the short limb until it forms a column equal to that in the longer limb. A small tube is finally attached to the short limb, and dipping a little way into a small bottle containing a small quantity of sulphuric acid, prevents the access of moist air into the short limb. The tube thus completed possesses the following advantages :—1st. There is the utmost facility in the movement of the column, so that the most minute changes of pressure are at once registered without any dragging. 2nd. The depression produced by capillary action is reduced to one *half*, so that the siphon arrangement can be satisfactorily used as affording an accurate neutralisation of capillary action.—Mr. Baxendell read a letter from Prof. C. Piazzi Smyth, F.R.S., Astronomer Royal of Scotland, referring to Prof. Reynolds's experiments on exploding glass tubes, and confirmatory of the conclusions of the immense force exerted by water when suddenly converted into steam, as when lightning rends a tree.

Vienna

Imperial Academy of Sciences, Oct. 9, 1873.—Prof. Krasan made two contributions in plant physiology; one of them as to what degree of heat wheat-seeds can bear without losing the power of germination. It is much higher than had been thought. They could bear a boiling heat for some hours, desiccation being effected by very gradual rise of temperature, and the use of chloride of calcium (65° for one hour was the limit previously supposed).—A second paper treated of the germination of tubers and bulbs of some early-spring plants.—Prof. Lindemann communicated a paper on the behaviour of acrylic acid towards hydrogen liberated from acid solution, and towards agents of oxidation. He finds that acrylic acid at 100° C., with zinc and sulphuric acid, readily passes into ordinary propionic acid; and that, in oxidation, it furnishes no acetic acid. He thinks acrolein and acrylic acid cannot be constituted similarly to true aldehydes and fatty acids.

October 16.—Prof. Heller, who had been requested to study the Tunicata of the Adriatic, gave a paper on the vascular system of these animals, and especially Ascidians. The walls of the heart (which is a long cylindrical bag, with a thin pericardium), show fine striated muscular fibres, not parallel, but forming a network. The two vascular trunks immediately proceeding from the heart have a similar wall-structure, and contract along with it. The vessels supplying the outer coat in Ascidians always appear as double vessels, joined together only at the end of the last ramification; in one vessel the blood flows outwards, in the other inwards. The blood in Ascidians is often coloured; sometimes greenish yellow, sometimes brownish; while in some species (as *A. intestinalis*), it is quite colourless.—Dr. Von Reuss communicated the first part of a monograph of the fossil bryozoa of miocene Tertiary strata in Austro-Hungary.—Prof. Ritter read a paper on the path of Winnecke's Comet (III. 1819).—Prof. Böhm described experiments which proved the injurious action of ordinary gas on plants. For example, of ten plants (Fuchsia and Salvia), in pots, in which gas was constantly being conducted to the roots, seven died in four months. It was also shown that the gas does not in the first instance kill plants, but that it poisons the ground. Dr. Bohm recommends Von Jurgen's method of preserving plants from gas in the ground, which is, to place the pipes in wider pipes communicating with the outside air, and in which a draught is produced.

October 23.—M. Stefan gave the result of experiments on evaporation, made chiefly with ether. The rapidity of evaporation of a liquid in a tube is inversely proportional to the distance of the liquid surface from the open end of the tube; it is independent of the diameter, and increases with the temperature. If a pipe, closed at one end, open at the other, is dipped with the latter in ether, bubbles are developed, and the times in which successive equal numbers of bubbles appear, are (initially) in the proportion of the odd numbers. If the tube contains hydrogen instead of air, the same number of bubbles appears in a four times shorter period. Thus evaporation in hydrogen is four times quicker than in air. If a pipe, with open stop-cock, is dipped in ether, and the cock then closed, the surface of liquid within the tube sinks under that without, and the depths to which it sinks in given times are as the fourth roots of the times.—Dr. Peyritsch communicated a memoir on Laboulbenia, describing a new species of the parasitic fungus, also the mode of development.

I. R. Geological Institute, Oct. 30, 1873.—Prof. Dr. A. Alth sent the first part of a monograph on the palæozoic rocks of Fadolia and "its organic remains," which will be published in the transactions of the Institute. This first part of Dr. Alth's memoir contains the geological description of the oldest formations of Padolia which, covered by large masses of cretaceous and tertiary deposits, and nearly horizontally stratified, appear only in the deep creeks along the beds of the rivers. The lowest rudimentary beds, resting immediately upon granite are sandstones which alternate with violet Argyle-slates, and are almost deprived of fossils. They contain the known concretionary globes of phosphorite. The next layer consists out of bituminous limestone with many fossils which belong to the Wenlock series; it is covered by grey marly slates which contain Brachiopods and Crinoids, and rarer Trilobites and Orthoceratites. The highest Silurian beds are green or grey shists alternating with crystalline limestones, which correspond to the Ludlow-series and contain, besides other fossils, very interesting remains of fishes. The Silurian strata are covered by red sandstone of Devonian age, and these immediately by the cretaceous strata. Of the fossil remains are described in the first part of the memoir, the fishes, chiefly Cephalaspidæ but partly also Placadermata (M. Cay) and the Crustaceæ, as Trilobites and Ostracadæ. They are figured on five plates.—Dr. O. Leny describes a fossiliferous bed belonging to the upper Neocomian limestone (Spatangenkalk) near Klein, in Vorarlberg. It consists chiefly of well-preserved oyster-shells and contains, besides, many different forms of Brachiopods, which certainly lived here in company with the oysters. This observation furnishes a new proof that the ancient Brachiopods were not confined to the deep sea like the modern representatives of this class, but inhabited the shores, also, together with the oysters; analogous observations had been made formerly by Th. Fuchs in the tertiary deposits of the Vienna basin, and by Dr. Majsisovics in the Muschelkalk of the Rhaetic.—Dr. C. Doelter examined last summer the environs of the Gurgl-valley in the Oetzthal Alps. He sends a notice about the different crystalline rocks which form this region.

Göttingen

Royal Society of Sciences, Sept. 3, 1873.—Chemical papers were communicated, on a base from nitrobenzanilid (Hübner and Retschy), on the xylidine from coal tar (Hubner and Struck), on the combination of nitrile with aldehydes (Hübner and Jacobsen).

November 12.—Dr. Hermann Ethé made a lengthy communication on the oldest period in new Persian poetry, criticising works of the poet Rûdagê, some of whose songs he translates.

Boston, U.S.

Natural History Society, Oct. 15, 1873.—Mr. S. H. Scudder described some kittens which he had seen at Plymouth, N.H., supposed to be a cross between the rabbit and the cat. The animals had a short rabbit-like tail, long haunches, and the gait of a rabbit, but in other respects were cat-like. Mr. Scudder could not believe the possibility of a cross between animals so far apart in the natural system, and asked for information from those present.—Dr. T. M. Brewer read a paper on the specific characters of the hermit thrushes, and also read extracts on their habits from the forthcoming work on "Birds of North America," by Prof. Baird, Mr. Ridgeway, and himself.—Dr. T. Sterry Hunt gave some account of the crystalline rocks of the Blue Ridge and their decomposed condition, as seen by him at various points in the region to the south-west of Lynchburg, Va. They are principally gneisses with hornblendic and micaceous schists, like those of the Montalban or White Mountain series, and are completely decomposed to a depth of 50 ft. or more from the surface, being changed into an unctuous reddish brick-clay, in the midst of which the interbedded layers of quartz are seen retaining their original positions, and showing the highly-inclined attitude of the strata. The nature of these chemical changes of the gneissic and hornblendic rocks consisted essentially in the removal, in the form of soluble carbonates, of the alkalies, lime, and magnesia of the silicated minerals and the hydration of the residues. The great antiquity of this chemical decomposition of the rocks was next alluded to. It was, in his opinion, effected at a time when a highly carbonated atmosphere and a climate very different from our own prevailed.

Paris

Academy of Sciences, Dec. 22, 1873.—M. de Quatrefages, president, in the chair.—The following papers were read:—Note on the report of the last meeting by M. Pasteur. The author called attention to the tone of M. Trecul's paper, which he considered was too personal; he briefly re-asserted his statements with regard to the origin of Mycoderma, &c.—M. Trecul replied and adduced in support of his own views as opposed to those of M. Pasteur, the experiments of Wyman, H. Hoffman, and Bastian. After a brief reply from M. Pasteur the subject was dropped.—On loss of magnetism, by M. J. Jamin. The paper dealt with the loss of increasing magnetism, as exhibited on cooling, suffered by a steel bar subjected to increasing temperatures.—Researches on the stability and reciprocal transformations of the oxidised compounds of nitrogen, by M. Berthelot.—On the results of the experiments made by the commission on vine sickness of the department of Herault, by M. H. Marès.—On a skeleton of *Palæotherium Magnum*, found in the Vitry-sur-Seine gypsum quarries.—On the anharmonic relation of four points of a plane.—Note on magnetism (6th part), by J. M. Gaugain.—On the phenomena of gaseous thermo-diffusion in leaves and on the circulatory movements which result from the chlorophyllian respiration, by M. A. Merget.—On the action of incandescent bodies in the transmission of electricity, by M. E. Douliot.—On an eruption of mud from the volcano of Nisyros, by M. Gorceix.—On the limit of the ice in the Arctic Ocean, by M. Ch. Grad.—On the form of the *Phylloxera*, a comparative study of the young, from leaves and branches, of hybernating and of sexed insects, by M. Max-Cornu.—An essay on the geographical distribution of the primitive populations of the departments of the Seine-et-Marne and Moselle.—On bilinear polynomes, by M. C. Jordan.—On the physical constitution of the sun, an answer to M. Faye's criticisms, by M. E. Vicaire.—Note on a process for the measurement of the relative intensity of the constituent elements of different luminous sources, by M. H. Trannin.—On the chemical composition of certain vegetable parenchyma, by M. Mandet.—New researches on the preparation of Kermes mineral, and on the action of alkaline carbonates and alkaline earthy bases on sulphide of antimony, by M. A. Terreil.

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Erratum.—Vol. ix. p. 124. 1st col. l. 6, for "South Villa," read "Campden Hill, Kensington."

THURSDAY, JANUARY 8, 1874

VIVISECTION

THE question of the propriety of vivisection has ever and anon cropped up for the last two centuries, and learned and unlearned persons have not been found wanting to condemn the practice. Amongst the latter the term vivisection has been taken to mean the dissecting of animals alive, with no other motive than curiosity or a malignant desire to be cruel to animals.

This arises from the utter and entire ignorance, on the part of the great mass of the public, of the scope and nature of physiology or the laws of life. If the elements of this noble and most useful science were taught in our schools as they should be, the unmeaning outcry against the practice of "dissecting live animals," as it is called, would not be heard. People would then know that the wonderful knowledge now possessed by man of the functions of his body has mainly been acquired by experiments on living animals, and that by the practice of vivisection is not meant the dissection of living animals, but the performance of experiments by which the nature of the functions of living beings may be ascertained.

Whatever excuse may be made for the public on account of their ignorance, there ought not to be any for men belonging to the medical profession, who should know the history of the science of physiology and the dependence of all true practice of medicine and surgery on the laws of life, mainly gained by humane and careful experiments upon living animals. These men would be answerable for much human suffering and premature death if they compelled men of science to give up the practice of studying the laws of human life and arrest the hand of Science in investigating the functions of living animals by inspection and experiments.

We feel almost ashamed in the present age to have to speak of the grand results which have been reaped by mankind from the observations of our great physiological discoverers in experiments on living animals. To begin with Harvey, whose name is a household word amongst us, and one of the grandest on the long page of England's discoverers; it is no perversion of words to say that he could not have discovered or demonstrated the circulation of the blood without the aid of vivisection.

In his great work, "An Anatomical Disquisition on the Motion of the Heart and Blood in Animals," he heads the second chapter "Of the motions of the heart as seen in the dissection of living animals." In this work he gives detailed accounts of his experiments, and also of those performed before the noblest and most learned in the land, who did not object to Harvey's experiments, but felt they were witnessing the demonstration of a truth that would for ever be a benefit to mankind. Had public opinion, had the Government of the day, instead of encouraging Harvey proceeded to prosecute him for cruelty to animals, then mankind would have lost a discovery that has saved myriads of human lives from torture and premature death by disease.

The discovery of the circulation of the blood produced an immense revolution in the practice of medicine and surgery. Counting the pulse became an intelligent aid to the diagnosis of nearly all diseases. Operations for the relief of disease were undertaken with fearlessness and the greatest success. The nature of aneurism and its means of cure were now understood. This last disease was studied and the surgical operation for its cure almost perfected by experiments on living animals by John Hunter. This great anatomist also made most important contributions to our knowledge of the nature of venous absorption, by his operations on animals. Nearly all the advances that have taken place in the treatment of aneurism since the time of Hunter have been made by experiments on living animals, amongst others we may name those of Spence, of Edinburgh.

Only to mention names rising to the surface from the greatness of their discoveries, we refer to Sir Charles Bell, to whom we are indebted for a knowledge of the nature of sensationary and voluntary nerves and their double origin in the spinal cord. These discoveries were made by experiments on living animals, and belong to a series which cannot be performed by the aid of anæsthetics, as the very essence of them consists in demonstrating that whilst one set of nerves is devoted to the feeling of pain, the other is the means of producing locomotion.

Another almost equally important discovery, the nature of the excito-motory action of the nervous system, was demonstrated by experiments on living animals by Marshall Hall. To say that these discoveries of Bell and Hall have had no influence on pathology and therapeutics, is to deny the experience of every medical practitioner in the kingdom—is to proclaim that the science of medicine is now practised on the system pursued by physicians and surgeons previous to the time of the discovery of the circulation of the blood. Numerous are the discoverers who have made great advances in our knowledge of the functions of the nervous system, by observations on living animals, who still live to be honoured for the advances they have made in that science which leads to the amelioration of human suffering. We need but mention here the names of Brown-Sequard and Ferrier. No human mind could have guessed at the conclusions at which they have arrived, but they have done so by the sure and certain method of observing facts in the living organism.

We might go on and fill our pages with the memories of great men who have not hesitated, for the benefit of mankind and the advancement of Science, to sacrifice the life of the lower animals. Majendie was accused in Paris of cruelty to animals, but his experiments led to a more accurate knowledge of the influence of medicines on the animal frame, and the introduction of a number of new remedies, which are still in common use. Blake, by the introduction of saline substances into the blood of living animals, showed what was the action of these matters on the blood, and he produced a sensible effect on the practice of medicine.

To the instructed this will seem a meagre list; but we hope enough has been said to show that to deny the utility of experiments on living animals is to deny that medicine has advanced at all during the last two centuries and a half, and to admit that the guesses of uninstructed practitioners are as good as the practice of the most cultivated practitioners of medicine and surgery.

Against this proof of the benefits of vivisection it has been urged that man has no right to inflict pain on animals. The same argument has been urged against the destruction of the life of animals at all, and the adoption of a vegetarian diet has been the result. It is surely not needful to answer the last argument here, but in a degree the answer is the same against giving pain to animals; if we take animal life for the purpose of food, it is only taking the life we have given us for the purpose of our existence; and in giving a minimum of pain to animals we give it for the higher purposes of securing human life and freedom from pain. It is curious to see those who defend the cruel sports of fox-hunting, hare-hunting, and partridge and pheasant shooting exclaim against the cruelty of vivisection. Yet it could be clearly shown, we believe, that those physiologists who are in the habit of practising vivisection would not be found at Hurlingham taking part in pigeon-shooting, or meeting with the hounds in any part of the country. In fact, so far from producing a hardening effect on the mind, these experiments seem to engender in the mind of the observer a love and a care for the brute creation, that does not exist in the mind of an ordinary person. A celebrated entomologist, in answer to the objection made to the pursuit of his science, the destruction of the life of insects, made answer that his habit of observing insects had induced him at various times to save more lives of insects—as flies from the cream-jug and tea-cup—than he had ever destroyed to make his entomological collection.

The question still arises whether the experiments that resulted in the discoveries to which we have referred should be repeated for the instruction of a class, or be regarded as final? Many physiologists think that the renewal of the experiments in the form of a demonstration before a class is not necessary. This position, however, cannot be maintained, if regard is had to the good of mankind. He would be a poor chemist who did not re-perform the experiments of those who had gone before him; and the natural philosopher could not make progress in his science if forbidden to repeat the observations of his predecessors. It is not only necessary to make good practitioners of medicine, and surgery that these experiments should be repeated but it is necessary for the advancement of the science of physiology.

Of course all these experiments should be performed with the greatest attention to diminishing pain to the utmost extent. Happily, by the use of anæsthetics, we can now do this so that an animal does not suffer more than it would in passing out of existence in any other way. And we are glad to find whilst writing this, that Prof. Schiff, of Florence, who has been so unrighteously assailed for these experiments, in a letter to the *Times* completely refutes all the charges brought against him, never failing to administer anæsthetics in the performance of these operations.

THE RELATION OF MIND AND BODY

Mind and Body. The Theories of their Relation. By Alexander Bain, LL.D., Professor of Logic in the University of Aberdeen. (Henry S. King and Co., 1873.)

IN this volume, which forms one of the international scientific series, the thoughtful reader is once more called on to consider those leading positions in psychology for which Prof. Bain has so long and so ably contended. He has here succeeded in presenting his views in language as concise, clear, and popular as the nature of his subject will permit. Whoever attaches importance to the application of scientific method to mental phenomena must welcome this popular statement of doctrines, which, if not the whole truth, are immeasurably nearer the truth than are the superstitions to which not only the uneducated, but also the great mass of the learned, are subject.

It is already known that Prof. Bain has given his adhesion, more or less fully, to the doctrine of inheritance in the region both of intellect and emotion—a doctrine without which the "experience" philosophy was utterly inadequate to explain the known facts. We may therefore be allowed to regret that he has not in this volume given more prominence to a conception without which his own system is but a half truth plus something of positive error. We are disappointed, for we certainly expected more than grudging references to "the new theory."

We have before now indicated our opinion that there is something wrong about Prof. Bain's celebrated theory of the Will; and we cannot now refrain from observing that in the present volume he seems to us to make the weakness of his position more manifest by placing alongside of his old theory some of the clearer and more thorough conceptions of recent development. "The distinguishing peculiarity of our voluntary movements," says Prof. Bain, "is that they take their rise in Feeling, and are guided by Intellect." Now our contention is, that there is no fact in nature corresponding to this description. Taking it for granted that "feeling" and "intellect" here mean facts of consciousness, and not physical facts—the objective activity of nerve cells and nerve fibres—we assert (1) that taken in the lump it is an expression of the popular notion, which Prof. Bain rejects, that the body is governed by the mind somewhat in the same way that the horse is governed by his rider; (2) that looked at closely it is a string of words making up a proposition that cannot be represented in thought. In support of the first point in our criticism it must suffice to show that Prof. Bain's teaching with regard to the will is relied on by the most thoughtful advocates of the doctrine of the soul—a belief against which Prof. Bain has been fighting all his life. A perfect example of the way in which Prof. Bain's theory is interpreted in favour of the hypothesis of a soul will be found in Mr. Lowne's "Philosophy of Evolution." We had recently occasion to make a few remarks on this essay, and we cannot now do better than quote part of what we then wrote:—"It is in studying the phenomena of volition (as understood by Prof. Bain) that Mr. Lowne finds the unmistakable evidence of a spiritual clerk employed in working the nervous apparatus. . . . Comparing the nervous system to a complex telegraphic system, he says:—'If the electric fluid became periodically liberated and affected all the instruments at once, or in a given succession, mechanism alone would account for the phenomena (reflex action); but if the electric current were always utilised according to ever-varying conditions which do not bear any direct relation to the manner in which the effect is produced—that is, which are themselves unable to alter the arrangement of the apparatus by which the effects are brought about—a guiding intelligence is needed (voluntary action). Such appears to be the condition of the nervous system in the higher forms of life; and we recognise such a guiding power, although we know of its existence only by its effects on the organic mechanism; and we speak of it as the mind or soul.'" It is for those who, holding Prof. Bain's theory of volition, reject the popular hypothesis that the body is endowed with a soul, to show the flaw in Prof. Lowne's argument. In saying this, however, we by no means wish to imply that there is not much in the writings of Prof. Bain quite inconsistent with this interpretation of his doctrine. Indeed we find set out with remarkable clearness in the volume before us some of the considerations which we urged, not against Mr. Lowne's argument, but against the theory of volition on which it is founded. "There is no warrant for the assumption (we said) that any movement of the kind called voluntary is not as completely and necessarily the result of purely physical antecedents, as are the movements of the planets or the spelling out of a telegraphic message . . . Whatever may be the link of connection between consciousness and nervous action, it seems both unnecessary and irrational to assert that either the amount or the direction of any vervous discharge depends in the slightest degree on the state of consciousness that preceded or accompanies it.

Sitting in his easy chair, Mr. Brown debates with himself how much he will give to the Mill Memorial Fund. Greed, small vanity, respect for Mr. Mill, the fear of being thought shabby, and perhaps a score of other mental states come and go, and at last he writes a cheque for 5*l.* Mr. Brown was aware of the mental side of his deliberations, while the corresponding physical changes in his nervous system were hidden from his observation. Hence the easy mistake of supposing that in writing out the cheque the fingers moved in obedience to spiritual direction.* This view seemed, and still seems to us, to forbid every conceivable interpretation of the proposition that movements "take their rise in feeling and are guided by intellect." It would appear, however, that what we feel to be an incongruity, does not strike Prof. Bain as such. For he also, if we understand him aright, believes the physical chain to be at all points complete and sufficient within itself. At least we find it difficult to understand the following extract from the chapter "How are Mind and Body united?" in any other sense. "From the ingress of a sensation, to the outgoing response in action, the mental succession is not for an instant dissevered from a physical succession. A new prospect bursts upon the view; there is a mental result of sensation, motion, thought, terminating in outward displays of speech or gesture. Parallel to this series is the physical series of facts, the successive agitation of the physical organs, called the eye, the retina, the optic nerve, optic centres, cerebral hemispheres, outgoing nerves, muscles, &c. While we go the round of the mental circle of sensation, emotion, and thought there is an unbroken physical circle of effects. It would be incompatible with everything we know of the cerebral actions to suppose that the physical chain ends abruptly in a physical void occupied by an immaterial substance; which immaterial substance, after working alone, imparts its results to the other edge of the physical break, and determines the active response—two shores of the material with an intervening ocean of the immaterial." Now remembering that movements of all kinds are physical facts, have their place in the "unbroken material succession," we once more put the question—In what sense can a particular class of movements be said to take their rise in the mental series which runs parallel to, without forming part of, the physical series?

The truth or meaning of our assertion that the proposition, "movements take their rise in feeling," cannot be rendered into thought, may now be perceived by anyone who will attempt to picture to themselves a state of consciousness turning on, or in any way determining the direction of, a nervous discharge. But as some of our philosophers, strong in logic, can surmount psychological impossibilities with the same ease that our divines can rise above them on the wings of faith, the disciples of Mr. Mill and Prof. Bain may demur that the question is not one of conceivableness or inconceivableness, but of proof. Well, then, let them show, if they can, that they have any better ground for the opinion that voluntary movements take their rise in feeling and are guided by intellect, than a superficial observer ignorant of the construction of the steam-engine might have for a belief that the movements of a locomotive take their rise in noise and are guided by smoke. Should it be attempted to turn the point of the foregoing argument by aid of the curious description of a mental fact, that it is a "two-sided fact"—both body and mind—our difficulty only requires to be restated. In what sense can a movement called voluntary—the objective side of a "mental fact"—take its rise in feeling the subjective side of the same "two-sided fact"? Using Prof. Bain's own words, "it is, after all, body acting upon body."

In this work Prof. Bain does not advance his idealism; probably he may have concluded, and justly, that it would prove too metaphysical for the readers of the International Scientific Series. Throughout his language is that of a realist. Mind and Matter seem to be accepted as ultimate facts; and "the institution of two distinct entities" is spoken of as "not in itself a crushing dispensation." Not only so, in such expressions as "undivided twins," "one substance, with two sets of properties, two sides, the physical and the mental—a double-faced unity," we have, to say the least, very much of the ring of Mr. Spencer's hypothesis that nervous action and consciousness are the objective and the subjective faces of his Unknowable—the one Ultimate Reality. We do not say that Prof. Bain is attempting the dangerous experiment of trying to put new wine into old bottles, but we fear until he has explained more fully the modifications which, by changes or additions, he means to make in his system, his present deliverance will be apt to suggest this.

DOUGLAS A. SPALDING

THE ELEMENTS OF LOGARITHMS

The Elements of Logarithms. By J. M. Peirce. (Boston, U.S.A.: Ginn Brothers, 1873.)

IN the preface Prof. Peirce writes:—"Logarithms ought not to be comprised, as they often are, in the midst of a treatise on algebra. For, in the first place, they are not algebraic functions; and, besides this, the student is unlikely to form an adequate comprehension of their purpose, or to appreciate the importance of acquiring skill in the use of his tables if he takes them up in the course of a study to which they have no application. If logarithms must needs be combined with any other branch of mathematics, their true alliance, on grounds both theoretical and practical, is not with algebra but with trigonometry." In point of fact, logarithms are usually included in works on trigonometry; and we can see no reason why their principle should not also be explained, as at present, in treatises on algebra, to which the theory does really belong. Ordinary students of mathematics never learn to use logarithms properly, not so much owing to deficiency of explanation in the existing works as to the fact that they never meet in the course of their reading with anything requiring such a knowledge. Prof. Peirce's work contains 82 small octavo pages, and is intended for readers possessed of only a very trifling knowledge of algebra. It is simply what a chapter on logarithms in an ordinary algebra would become if printed separately, with the addition of copious examples and an appendix on their use in trigonometry. To show how limited is the range of the book, it is only necessary to state that all the rules have reference merely to three and four figure tables, and that the natural base *e* is not even alluded to, though it is stated that a chapter on the Napierian system will be added in another edition. There is little either to commend or blame in the book. It is partly intended for the entrance examination at Harvard, but it seems to us it would be most useful to computers who wished to obtain some notion of the reason for the rules they were in the habit of employing. In one respect the book is in advance of the time, viz., some paragraphs are devoted to the history of the subject. We believe the day will come when no scientific treatise will be considered complete that does not contain short historical notices relative to the discovery of the principal results.

Prof. Peirce defines the arithmetical complement as the complement from 10; we should much prefer to see it defined as the complement from zero, so that the arithmetical complement of a logarithm of a number should be the logarithm of the reciprocal of the number, viz. its cologarithm. We also hope the day will come when the addition of 10 to the mantissæ in our logarithmic trigonometrical canons will be abandoned, and the true negative characteristics printed and used. A complete seven-figure table with negative mantissæ was published at Paris by M. J. Dupuis in 1868, which was a step in this direction.

PEDIGREE AND RELATIONSHIP OF MAN

The Story of the Earth and Man. By J. W. Dawson, LL.D., F.R.S. (Hodder and Stoughton.)

Man and Apes. By St. George Mivart. (Hardwicke.)

THESE two works possess some points in common. Neither of their authors accept Darwinism in its entirety, the former absolutely rejecting it. They both treat of the relations of man to the lower animals, and both find the chasm of the human mental and moral phenomena the great drawback against bringing man into the same category with the apes. The manner in which the subject is treated, and the facts employed, are however not the same, while the results arrived at are very different, as will be seen from the following remarks.

Dr. Dawson is very much irritated by the manner in which many of the biologists of the present day, without feeling any necessity for giving the reasons for their belief, are in the habit of writing and talking as if the evolution hypothesis were fully proved, and established as a fundamental principle of nature. "That in our day a system destitute of any shadow of proof, and supported merely by vague analogies and figures of speech, and by the arbitrary and artificial coherence of its own parts, should be accepted as a philosophy, and should find able adherents to string upon its thread of hypotheses our vast and weighty stores of knowledge, is surpassingly strange,' remarks our author in a spirit which we are surprised to meet in one who thinks that "in the present state of natural science in Britain this evil (of regarding geologic facts from an evolutionary point of view) is to be remedied only by providing a wider and deeper culture for our young men." In the same dogmatic and unscientific spirit all the theoretical questions which are discussed, are written for the perusal of the readers of a popular journal, and such being the case, it is hardly surprising that false notions are so common as to the direct bearing and tendency of the greatest theory of modern times.

"We need not stop to mention the usual inaccuracies as to facts" is the way in which a criticism of a paragraph in one of Mr. Herbert Spencer's works is commenced, and as might be almost predicted fiom so self-satisfied an author, it is in the criticism only in which the inaccuracy is to be found. On the following leaf we are astonished to learn with reference to the Ascidian, that its "resemblance to a vertebrate animal is merely analogical, altogether temporary and belonging to the young state of the creature, without affecting its adult state or its real affinities with the mollusks." The author can hardly have studied Kowalevsky's memoir on the subject, in detail.

In his anatomical structure, man, according to Mr. Dawson's distorted view, presents differences from all the apes which are at least of ordinal importance, distinctions "mainly dependent on grade or rank, and not to be broken down by obscure resemblances of internal anatomy having no relation to this point, but to physiological features of very secondary importance." When, in association with this, we are told that it is merely begging the question to say that "the fact that the human skeleton is constructed on the same principles as that of an ape or a dog, must have some connection with a common ancestry of these animals," we think it hardly necessary to make further comment on the work in question, except to hope that it will not fall into the hands of commencing biological students, who would find it difficult to shake off the false associations that, in it, surround the facts which are discussed.

Mr. Mivart treats his subject in a very different manner. His object is "to investigate by the unimpassioned process of enumeration and weighing facts of structure, what is the teaching of Nature as to the affinities of various apes to man." In doing this, after a rapid review of the clasification of Mammalia generally, and the geographical distribution of the apes and lemurs, or half apes, as they are termed, the peculiarities of the osteology of the Primates, and their soft-part anatomy, are entered into in detail. From the facts thus obtained, especially from the peculiarities of the liver and brain, it is shown that the, at present accepted, notion that the Gorilla is man's nearest ally, is not borne out by anatomical investigation, and that the Chimpanzee, the Orang, or the Gibbon can either of them claim a closer relationship. The recapitulation of the many different points in which man in some one or other point resembles the various higher and lower apes, leads the author to think that the laws of affinity form a "network" or "tangled web" rather than a "ladder," from which it is only possible to infer that in the course of development there has been blood relationship established between the different species of apes, after their differentiation into distinct species, which is hardly compatible with our notions of the definition of a species. No decided opinion is given as to which ape does stand nearest to man, the various points of similarity in each being considered as fairly balanced. However, there are two structural features at least that are not mentioned, which, when added to those noted, go strongly to support the placing of the Orang-Utang, as the nearest ally to the human race. The first of these is an osteological one; in man and the orang the postero-internal angles of the orbital plates of the frontal bone do not meet and blend behind the cribriform plate of the ethmoid bone, as they do in the gorilla and chimpanzee. The second is in the soft part, the penis of the orang being very similar in general proportions to that of man, whilst in the chimpanzee at least, it is decidedly different, being smaller proportionately, and with a button-shaped glans.

Mr. Mivart adds the weight of Gratiolet's bold attempt to classify the monkeys by their cerebral convolutions, to show more demonstrably that the gorilla is anything but as high as he has been placed in the scale. Though Gratiolet may have been correct in so displacing the gorilla, nevertheless it is difficult to believe from their general appearance, structure, and geographical range, that, as he thought, the baboons and cercopitheci are far separate from one another, that the Asiatic true macaques and the African chimpanzee, are most closely allied; and that the affinities between the Entellus monkey and the orang are very intimate. These somewhat shake our faith in the results that have as yet been arrived at from the study of the cerebral convolutions.

No animal seems more difficult to depict correctly than the monkey, man alone excepted, which may in itself be considered to indicate a point of affinity; the illustrations accompanying Mr. Mivart's work, are, however, of the poorest description, many not being the least worthy of their author; old, and imperfect, inaccurate in not in any way giving the expressions or correct attitudes of the originals, we should have preferred to see them omitted.

OUR BOOK SHELF

Geology. By Prof. Geikie. Science Primers. (Macmillan and Co. 1873.)

THIS is a charming little book of 128 pages. It is well arranged, well written, and well illustrated, and is thoroughly well adapted for its purpose. Of course the geology in it is unexceptionable, and therefore it follows that the only thing with which a reviewer can quarrel at all is the selection of subjects for omission. Among small omissions he might mention that of Darwin's theory of coral islands as a "proof that a part of the crust of the earth has sunk down." This is so beautiful an instance of an explanation of the curious phenomena of coral islands, that it never fails to interest boys. To lead them up to this theory, and then test it as Darwin tested it, is an excellent exercise in that peculiar kind of reasoning about past causation which is of the essence of geology. A greater omission is perhaps that of the history of geological science. A sketch of this in half a dozen pages would greatly interest boys; it would show them how science grows; and they would infer that geology is not yet completely mastered, but that there is something left for them to do. It strikes one also as an omission, of a very grave kind, to say nothing at all about stratigraphical geology, a few pages of it with a general description of the stratigraphical structure of England would increase the value of the book, and what is more, inspire the reader with a desire to learn more. And lastly, one cannot but desiderate some sketch of the sequence of life on the earth as the result of palæontology, for the same reasons. If all these things were put in, the book would still be small, and would really introduce the reader to the whole of geology, and excite his curiosity. J. M. W.

Jahrbuch der kais. kön. geologischen Reichsanstalt. Band xxii. Nos. 3 and 4.

IN the first of these numbers, perhaps the most interesting paper to an English geologist is one by von Theodor Fuchs, "On Peculiar disturbances in the Tertiary Formations of the Vienna Basin, and on a self-evident Movement of Unconsolidated Earth-masses," which is accompanied by a number of illustrations and sketch sections, taken chiefly from the cuttings of the railway at Marchegg. The writer thinks that the contortions and displacements witnessed in superficial deposits, and which have been variously accounted for—some geologists supposing them to be due to subterranean action, others to glacial action, and so forth—have been induced by causes, which have hitherto been either overlooked or treated as insufficient. His studies have led him to conclude that these superficial confusions and displacements are brought about by a movement amongst the earth-masses themselves, which, as a rule, beginning with some local slip of the beds, becomes eventually converted into a movement of the whole. The motion of the earth-masses, now rolling, now gliding, can only be compared to the flow of a mud-stream or that of a glacier. After the author's paper was written, he became aware that he had been preceded in his general conclusions by Mr. R Mallet, whose paper in the Journal of the Dublin Geological Society ("Some Remarks on the Movements of Post-tertiary and other discontinuous Masses," vol. v. p. 121) will no doubt be known to many of our readers. It is not likely, however, that glacialists will ever be got to believe that their boulder-clays, &c., and scratched rock-surfaces have been produced by the continuous or intermittent slipping of loose material which is in daily progress around all the existing coasts. The other papers in this number are, "The Mountain-land of South Glina in Croatia," by Dr. Emil Tietze, and "On the so-called gas-shales of Nyran and their flora," by von Ottokar Feistmantel. Number 4 opens with the second part of Professor Hochstetter's interesting paper "On the Geology of the eastern parts of European Turkey." This part is accompanied by a geological map of Central Turkey, which shows the distribution of the rock-masses, while several diagram sections scattered through the paper enable us to understand more clearly their succession and relative position. Amongst primary rocks the author enumerates gneiss, amphibolgneiss, mica-schist, talc-schist, phyllite, granite, syenite, amphibolite, serpentine, and crystalline limestone. Under the mesozoic division, he gives red sandstone, quartzite, and conglomerate, which he considers to be of Triassic age, and compact limestone and dolomite, which may be either of Triassic or Jurassic age, or both. Above these come deposits of chalk and marl of middle cretaceous age. The tertiary and quaternary deposits consist of miocene lacustrine beds with lignites, post-miocene diluvium, or fluviatile gravels, and alluvium. Amongst eruptive rocks he enumerates quartz-porphyry, augite-porphyry, pyroxenic tuff and conglomerate, trachyte, trachyte-conglomerate, pumaceous tuff, &c. The only other geological paper in this number is an explanation of Sheet iv. (East Carpathia) of the Geological Survey's map of the Austro-Hungarian empire. Both numbers of the *Jahrbuch* are accompanied by the usual mineralogical communications, which contain a number of papers, amongst them one by von Johann Rumpf upon "Kaluszite," a new mineral, the chemical formula of which is given as $CaK_2 (SO_4)_2 + aq.$ or in another way as $CaO, SO_3 + KO, SO_3 + aq.$ An illustrative plate accompanies the description.—Prof. Tschermak furnishes some account of the meteorites in the imperial mineralogical collection up to October 1872, giving a table that shows in a condensed form the names of the places where the meteorites were found, the hour of the day, the day of the month, and the year in which the stone fell, &c.—Special mention must also be made of a paper by Fuchs on the Island of Ischia, which is geological and historical, as well as petrological—a paper which will well repay perusal by those who are engaged in the study of igneous geology.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Largest Amphipod.—Willemoesia (Deidamia).

IN a paper which was read at the Royal Society this year, I described the anatomy of a female amphipod caught in the Atlantic, and remarkable for its large size and the absence of the second pair of antennæ. This female had a length of 84 mm., not of 14 mm., as has been stated in NATURE and in other periodicals which have reprinted my abstract from the Proc. Roy. Soc. We have since also caught males of this interesting amphipod, which were still larger, more than 3 in. long. A description of these has been added to the above-mentioned paper, so that now the anatomy of both sexes will be known. This amphipod, which, as we have discovered, lives on the surface, is, thus, by far the largest one known. Some figures representing the male and parts of the mouth, which at first could not be dissected, and therefore not well be seen, will appear in a larger paper on some of the remarkable deep-sea and other crustacea caught during the *Challenger's* cruise in the Atlantic.

This amphipod, however, which was supposed to be new, and to which I gave the name of *Thaumops pellucida*, has been already described by Guérin-Méneville under the name of *Cystosoma neptuni*. The distinguished French naturalist has described this species from a single specimen caught in the Indian Ocean. This I found out only when I got Mr. Spence Bates's catalogue of amphipods sent out to me, in which the original figure has been reproduced. The first description of this species seems, however, to be so incomplete, that some additional knowledge about its structure will be welcome, I hope, to zoologists.

The geographical distribution of certain amphipods seems to be a very wide one, for we have not only caught several specimens of Cystosoma in the Atlantic, but also a species of the genus Oxycephalus, which hitherto seems to have been found in no other but the Indian Ocean.

With regard to *Willemoesia* (*Deidamia*), Mr. Grote has been kind enough to point out in vol. viii. p. 485 of NATURE, that the name Deidamia has been used already for a genus of Sphingidæ, by Dr. Clemens, and honoured me by calling the two blind crustacea, which are so closely allied to the fossil Engonidæ, by our family name; I am very much obliged to the curator of the Buffalo Museum for this information, and will always be glad, during the time of our cruise, to receive communications of this kind. For though we have a good library on board, mistakes like these cannot always be avoided, when it is necessary to give a name to those animals which I describe, not because they are new, but because they furnish interesting additions to our knowledge of the morphology of lower animals.

R. v. WILLEMOES-SUHM

H.M.S. *Challenger*, Simons Bay, Cape of Good Hope,
Nov. 24, 1873

Physiological Effects of Ozone

LONG before Schonbein discovered ozone, electricians who had been in the habit of employing Franklinic or statical electricity as a therapeutic agent, had discovered that the electrical aura, as they termed it, or the current of air proceeding from an electrified point, possessed decided physiological properties, and the effect appeared to be the same whether, on the single fluid hypothesis, the electrical current or breeze passed from the point to the animal surface, or *vice versâ*. The physiological effects principally noticed were the power of this breeze to allay chronic inflammatory actions in delicate organs, such as inflamed eyes, or to relieve pain arising from a decayed tooth; but they were most remarkable in the curative effect produced on obstinate ulcers, when the electrified current or aura was daily thrown upon their surfaces for some minutes. The *rationale* of this process was not understood, and electricians were contented to accept the facts without being able to explain them.

About 45 years ago I employed Franklinic electricity as a collateral branch of my electrical pursuits pretty extensively as a therapeutic agent, and had abundant opportunities of noticing these physiological actions. In addition to these a fact gradually developed itself during the course of my electrical investigations, namely the effect which an electrified atmosphere had upon the mucous lining of the throat and bronchial tubes. It was no uncommon thing, after a day's continuous use of an electrical machine in a close room, to feel a considerable amount of irritation over the respiratory tract very similar to that experienced when recovering from an attack of influenza, and I found that I could often produce the same irritation by removing the prime conductor from the cylinder of an electrical machine and holding my face in such a position as to breathe the copious ramifications of electricity that were thrown off from it.

These effects are all now referable to the development of ozone, and the interesting experiments of Mr. Dewar and Dr. M'Kendrick, recorded in NATURE (vol. ix. p. 104) open up a field of inquiry, the extent and importance of which can scarcely be estimated. Of late years ozone has, by a kind of *post hoc propter hoc* reasoning, been designated the scavenger of the atmosphere, since raging epidemics have been suddenly checked in their course after the occurrence of a good rattling thunderstorm, and hence the old notion, not without good foundation, that lightning clears the air. So much importance has been attached to the supposed value of this antiseptic agent, that not a few, and myself amongst the number, have recommended various forms of apparatus for the development of ozone within the precincts of fever hospitals, but the experiments of Mr. Dewar and Dr. M'Kendrick seem to show that there is a limit beyond which it would not be prudent to ozonise an atmosphere destined for respiratory processes.

The further investigation of its physiological effects will therefore be looked forward to with no small interest. The examination of the subject, however, must not end with its effects upon animal physiology.

From experiments which I have made on the extraordinary electrical conditions which are suddenly induced in an atmosphere forming the extended di-electric of a thunderstorm, I can trace an intimate relation between the copious development of ozone and a corresponding effect upon delicate vegetable organisms, which may lead to the discovery of the proximate causes of blight so frequently the accompaniment of thunderstorms. Some years ago I extended a small atmospheric exploring wire between my own house and the cupola of a chapel 400 ft. off. One end of this wire was brought into my study, and connected with an electrical battery containing about 12 square feet of internal surface; a discharging apparatus, which also served the purpose of a lightning-conductor to the ground, was connected with this wire. The changes which this wire indicated as occurring in the stratum of atmosphere with which it was in contact, were of a most extraordinary character.

Simultaneously with the occurrence of a flash of lightning, even at a mile distance (the battery being disconnected), a torrent of sparks rushed between the exploding balls, presenting the appearance of a thick bundle of brilliant sparks, with a noise similar to that of suddenly breaking a hard fibrous stick. When the battery was in connection with the atmospheric wire, the quantity of electricity brought in by it was sufficient to charge and discharge the battery, over an interval of a quarter of an inch several times so rapidly, that it was impossible to count the discharges, the cracking noise being correspondingly loud. This effect is now accurately imitated by the bundle of sparks passing between the terminals of a powerful induction coil, having an electrical battery connected with it. When it is considered that the earth's surface in immediate connection with this electrified stratum is acted upon electrically by it, one can easily see the influences which such actions are likely to produce upon the delicate vegetable organisms which have not only to act as conductors in these electrical changes, but are exposed to the actions of a sudden development of an enormous amount of ozone. It will therefore be an interesting matter to know how vegetable life will be influenced by an ozonised atmosphere, especially as the conditions necessary for artificial experiments will not be difficult to obtain.

Plymouth, Dec. 29, 1873 J. N. HEARDER

Photographing the Transit of Venus

THE following is the result of some experiments recently made on photographic irradiation:—

If, as is generally supposed, photographic irradiation is caused by the reflection of light from the back surface of the plate, then photographs taken on non-actinic coloured glass ought to be free from irradiation, because the light would be quenched in the glass, and therefore no reflection could take place. Photographs of a model transit were taken on yellow, orange, and red glasses; but in all cases the irradiation was nearly as bad on the coloured glasses as on the clear glass.

Photographic irradiation may possibly be caused, either by the bright light producing an intense state of chemical activity, which has the power of spreading itself, or what seems more probable, the parts of the collodion on which the bright light is falling become luminous, and reflect light to the surrounding parts of the sensitive film, and thus extend the chemical change in each side of the true optical boundary line. If this is the explanation, then we can correct photographic irradiation by allowing only sufficient light to fall on the plate to produce the necessary chemical change, so that there shall be no surplus to be reflected; or we may make the sensitive film of such a nature that it cannot reflect the actinic ray. There are two ways of carrying out the first of these plans. We may either "stop" down the lens by means of a diaphragm, or we may pass the light through a non-actinic coloured screen. The first should be the best plan, but was not found practicable with the Dallmeyer "triplet" lens used in the experiments. Screens of glass and coloured solutions were then tried, and photographs of the model transit taken perfectly free from irradiation, and not to be distinguished from photographs of the model taken against a dull sky, which required 15 seconds' exposure. Experiments were then made to make the sensitive film incapable of reflecting actinic rays. This was done by adding red aniline to the collodion, till the colour was found by experiment to be deep enough. Photographs taken in this way were also quite free from irradiation. After the photographs were developed and fixed in the usual way, they were treated with chlorine gas, which destroyed the red colour and left the photographs on a clear film.

Ocular irradiation is also, in all probability, in part caused by the reflection of light in the eye. But in addition to this cause there is another of considerable importance—namely the "persistence of the image" combined with the unconscious motion of the eye—as the impression received by the brain is not only that of the light on the part of the retina where the image at the time is, but also that of where it was a short time before, the mental impression must therefore be larger than the image on the retina. Ocular irradiation can also in all probability be corrected, by reducing the amount of light falling on the eye, to the minimum necessary to give a distinct impression. The reflection in the eye will then be less. The image not being so bright will not "persist" so long—and the light not being so brilliant, the stimulus to the unconscious motion of the eye will not be so great. Diaphragms will of course be preferred for this purpose. When screens are used it is probable that neutral tinted ones will be found to suit best.

JOHN AITKEN

The New Marine Animal

IN NATURE, vol. viii. p. 488, under the heading "New Marine Animal from Washington Territory," Mr. P. L. Sclater announces the description by Mr. Stearns of the *Verrillia blakei*, the long-sought-for owner of the wand-like rod named by Gray, *Osteocella septentrionale*.

I write to say that the nationality of the Polyp is altogether British; Burrard's Inlet—the only place it has yet been found—is in British Columbia, close to the north mouth of the Fraser, and the first description of it would have been British too, but for unavoidable postal delays in the transmission of my paper, the receipt of which by the Zoological Society Mr. Sclater mentions. EDWARD L. MOSS

Royal Naval Hospital, Esquimalt, B.C., Nov. 26, 1873

The Potato Disease

IN NATURE, vol. ix. p. 161, it is stated by Mr. W. G. Smith that the bodies referred by Dr. Montagne to Artotrogus are possibly no other than *Volutella ciliata*. Nothing can be more common on decaying potatoes than *V. ciliata*, but I can state most positively that Montagne's fungus, whatever its nature may really be, had nothing to do with *V. ciliata*. It is very important that attention should not be drawn off from Dr. Montagne's, or rather Dr. Rayer's curious observation by a supposition which is entirely without foundation. A reference to the figures in the *Journal of the Horticultural Society* (vol. i. tab. 4, figs. 27, 28, 29), and the characters of Artotrogus, apart from the specimens submitted to myself, and the occurrence within the cellular tissue, ought to be quite sufficient.

Jan. 3 M. J. BERKELEY

Specific Gravity of Sea-water

IN Prof. Wyville Thomson's work "The Depths of the Sea" there appears to me a curious discrepancy between two statements of the specific gravity of the sea, to which it may be useful to direct general attention. At p. 505, Mr. W. L. Carpenter states that the average specific gravity of surface-water, at a sufficient distance from land to be unaffected by local disturbances, was 1·02779. At p. 513, Dr Frankland gives the specific gravity of four samples of surface-water, the mean of which is only 1·0267, even less than the minimum value as given by Mr. Carpenter. Both results are said to be for temperature 60° F. I should have expected Dr Frankland's determination to have been the higher, from possible loss by evaporation. The difference may probably be due to want of identity of indication between the instruments used From whatever cause it may arise, the difference is so considerable, as to leave no doubt whatever that it ought to be accounted for in some way; and the error wherever it lies fully exposed. R. STRACHAN

Meteorological Office

Optical Phenomenon

A SHORT time ago I was lying, during the heat of the day, in a darkened room in a house at one of the hottest stations in India. There was a great glare of sunlight outside All at once I became aware of figures moving about on the opposite wall. On examination they proved to be the inverted images of the servants of the establishment who were walking about in the performance of their several duties in the gravelled courtyard outside the house. The white colour of their clothes, the dark colour of their skin, and the red colour of their sashes or turbans, were distinctly reproduced, and every servant was recognisable without difficulty. The images were produced by rays passing through three or four holes in the Venetian shutters; and while they all remained open there was a large penumbra round the images, but on closing all but one hole, this was very much reduced. The holes were of the size of a shilling or half-crown, and made in an outer door as well as the shutter, having been constructed to admit of a punkah rope passing through. The explanation appears to be this:—The sun was above and slightly behind the house. The solar rays falling on the objects in the court-yard were transmitted through the shutter holes. There being no other light in the room, and the rays being strongly scattered by the rough whitewashed wall, the rays were sufficiently powerful to produce an image on the retina of an observer in whatever part of the room he might be; the room became, as it were, the box of a large camera.

On intercepting the rays with a smooth oval looking-glass, they were not, of course, scattered, and no image was visible on the glass, but the image could be reflected from the looking-glass to any part of the wall which contained the shutters through which the rays passed. The appearance produced when a servant was made to stand in the required position, was singular. A full-length (inverted) coloured figure appeared in an oval frame of bright white light, much larger, of course, than the looking-glass. The white light was produced by the glare from the gravelled yard, shadows on which were reproduced.

A dog-cart and horse were imaged on the wall most clearly, the chesnut colour of the horse being very distinct. The whole phenomenon was always producible at any time when the sun was in the proper position above the house.

Are not mirages of one class, *i.e.* the appearance of inverted images in clouds, produced in a similar way? The rays from a figure might pass through an opening in one cloud to the face of another otherwise unilluminated, and be thence scattered. I believe I have seen this explanation given somewhere, but I cannot remember where.

E. C. BUCK

N. W. P., India

ON TEMPERATURE CYCLES *

SINCE the discovery of an eleven years' period in the phenomena of solar spots, several corresponding periods (it is now well known) have been demonstrated in terrestrial phenomena, more especially in those of magnetism, auroras, cyclones, and rainfall. With regard to weather changes, it has been thought by Dove, that the tracking of a cycle in these could not, theoretically, be made an object of research; and that while some indications of a periodicity might appear, a great part of the complicated changes named must be, from the nature of the case, quite unperiodical. The series of observations by Dove on the subject led him to the conclusion (1) that divergences from the normal, especially those of temperature, are not local, but spread over large surfaces; but (2) that negative divergences, in one region of the earth, are compensated by positive in another; and conversely. That the compensation is perfect, and that the quantity of heat annually given by the sun is constant, has been affirmed also by Maury and others.

The data on which this conclusion is based are limited. They appeared quite insufficient to a German physicist, Dr. W. Köppen, who has recently been led to undertake a wider investigation of the subject. He has communicated to the Austrian Society for Meteorology a preliminary notice of his inquiries and results (*Zeitschrift*, Aug. and Sept. 1873), which will be found of considerable value.

We may first note here his materials and method. He furnishes a long list of places from which observations (more or less extensive) have been had; and in his first table he gives the divergences of temperature of individual years (1820—71) from the average temperature, and for the following regions: India, Tropical America, Temperate South America, South Africa, Australia, China, and Japan, Mediterranean region, Southern United States, Western U.S., Western Central Europe, Austria, South Russia, South-West Siberia, East Siberia, Central part of U.S., Atlantic States, British Islands, North Germany and Netherlands, North-West Russia, North-East Russia and Ural, North-West America, North East America, Iceland, Northern part of Europe. [The particular towns, &c., are given, and the author's purpose partly is, that the list may be supplemented by other series of observations (which he has not been able to see), being sent to the Central Physical Observatory at St. Petersburg, where he has chiefly been prosecuting this research.] The periods of observation ranged from three to thirty years; the average was taken from several years' observations. In many observation-series, the yearly average had to be calculated for the first time. Series of six years' length were the shortest admitted, and such short series only by way of completing the longer. The original sources of Prof. Dove's material were consulted.

A second table shows the divergences of temperature in various regions for the years 1768—1819. By way of condensing, a third table is given, in which the material from 1820—71 is arranged in five series, one of which represents the tropics, and the four others four successive ex-tropical zones. The zones are not bounded by determinate parallels of latitude, but it was sought to combine approximately equal material of observation and earth surface.

On comparison of the curves of Table III. with the sun-spot curves (according to Wolf), a striking correspondence at once appears, as far as the year 1854. In the tropics, the maximum of heat occurs ½—1½ years *before* the spot-minimum; in the ex-tropical zones, on the other hand, it occurs *after* the minimum; in some cases (in the forties, *e.g.*) as much as three years after. The regularity and extent of the variations diminish from the tropics to the poles.

It is further noticeable that as the interval from maximum to minimum of the spots is always greater than that from minimum to maximum, a corresponding inequality occurs in the temperature changes.

On these results Dr. Köppen remarks that, while there is evidently some connection between the two kinds of phenomena, the sun-spots do not act directly by darkening a part of the solar disc; for, as the temperature of the earth's surface is a function of the solar radiation, the change in the former must follow that in the latter; but the opposite occurs, as we have seen, in the tropics. It is probable that the temperature of the sun's surface is (from some unknown cause), at its highest one or two years before the minimum of the sun-spots. That the spots (if we suppose them to be solid bodies) take so long to melt that their minimum only occurs after the maximum temperature of the earth's surface, is not remarkable, considering their size.

If we consider the period 1800—71, we find a section of about 40 years, with marked periodic variation, 1815—54, and two periods, before and after, showing great disturbances, (say) 1792—1815, and 1854—66. Whether in 1865 we have again entered (as the curve would seem to indicate) on a time of distinct periodic variation, will doubtless appear in the next ten years.

The observations before 1800, again, show such anomalies in the temperature, that we should almost doubt the existence of connection with the sun-spots were it not for the convincing evidence of the years 1815—54. We find all possible cases, from complete indifference of the temperature in contemporaneous change of the sun-spots (1750—71), and a short correspondence of both (1772—77), to a well-marked and regular variation of temperature (1777—90), which stands to the sun-spot curve, in exactly the opposite relation to that found in 1816—54. True, the observations here are only from a small fraction of the earth (West Europe and the New England States); but the continuance of the same curve shows the normal variation in 1816—54 quite distinctly. The estimation of the spots previous to 1826 is somewhat arbitrary, but an error such as that the maximum is put in the place of the minimum cannot be supposed. And lastly, if it be urged that the turning points of the temperature curve (1779 maximum and 1785 minimum) are precisely where, according to the mean length of the sun-spot period of 11·1 years, they must be; that there may, perhaps, be an 11 years period in the temperature independent of the sun-spot period, and that, in the present case, a displacement which the spot period has experienced is not shared by the temperature period; we have to remember that the correspondence of the temperature changes in 1815—54, does not merely extend to the average length of the periods, but that all peculiarities and disturbances in the sun-spot curve are, in these 30 or 40 years, reflected in the temperature curve. Further observation is needed to explain this phenomenon. Possibly (the author suggests), we have here the interference of a number of quite independent periodical actions; and (without laying stress on the fact, in default of causal evidence), he notices that the greatest negative anomalies occur, for a considerable time, in a series which progresses by multiples of 9, and in such a manner that an interval of 27 alternates with one of 18 years. Thus—

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* Abstract of paper by Dr. W. Koppen in the Austrian *Zeitschrift fur Meteorologie*.

$$\begin{array}{ccccccccccc} 1740 & = & 1767 & = & 1785 & = & 1812 & = & 1830 & = & 1857 \\ & +27 & & +18 & & +27 & & +18 & & +27 & \end{array}$$

The first four agree; there is merely the quite isolated cold year 1794 intermediate. Going further, we find divergence; for the table shows a strong negative anomaly about 1836; but we have, again, the well-authenticated negative anomaly of 1856—57 conforming to the rule. Renou has assigned, for the return of the cold winter of south-western Europe, a period of 41 years; the author asks whether the time 27 + 18 = 45 years does not better agree with the phenomenon. On this view, the first winter, reckoning back from 1740 is 1695, and this is recorded as having been one of excessive cold. Between these two occurs one winter of extraordinary cold, 1709, but it is quite isolated, the neighbouring years having been warm. If we go still further back, the periodicity cannot be ascertained with any certainty. If the rule is correct, and its validity between 1740 and 1857 not a mere accident, *i.e.* the expression of quite other laws, we have to look for a very cold year in 1875 (being 1857 + 18).

Dr. Koppen proposes, in a future communication, to treat of hydro-meteors, and to examine the influence of periodic weather changes (at several years' interval) on some phenomena of organic nature.

LAVOISIER'S WORK IN THE FOUNDATION OF THE METRIC SYSTEM

SINCE the publication of the article on the Metric System, in NATURE, vol. viii. p. 386, my attention has been drawn to some recent information showing the important part taken by the celebrated Lavoisier in the scientific operations for establishing the basis of the metric system of weights and measures in France. Lavoisier's name has hitherto been little noticed amongst those of the men of science who were prominently engaged in this work; but it is now clearly proved that up to the period of his being guillotined on May 8, 1794, when he fell a victim to the revolutionary fury during the reign of terror, no one took a more active or serviceable part in the scientific labours for founding the Metric System than Lavoisier.

This information is contained in a "Notice historique sur le Système Metrique," by General Morin, lately published in the "Annales du Conservatoire des Arts et Métiers." It is derived from original documents left by Lavoisier, and now in the possession of the Académie des Sciences. These documents have since been submitted to my inspection by M. Dumas, and full details of them will soon be given to the world in the fifth volume of the works of Lavoisier, which M. Dumas is now completing.

Although Lavoisier's name does not appear in the list of the original Committee of Weights and Measures in France, yet it is shown that he was very actively engaged in making the arrangements for their meetings and in preparing the minutes of their proceedings, as appears from papers and letters in his own handwriting. It was through his personal agency that funds were provided at Paris for continuing the measurement of the arc of the meridian in Spain by Méchain. And more particularly, all the actual comparisons for determining the length and dilatation of the standard measures used by Méchain and Delambre for measuring the basis, and known as the *Règles de Borda*, were made, not by Borda, but by Lavoisier. The subsequent computations only were made by Borda. Lalande has expressly stated that the work of preparing them was executed by Lavoisier and Borda, but that the construction of the measures of platinum and brass, forming metallic thermometers, and of the comparing apparatus used, was carried out under Lavoisier's directions. The published report upon the construction and verification of these measures in 1792 is contained in the "Base du Système Metrique," vol. iii. p. 313. It was drawn up by Borda, but Lavoisier's name is not mentioned in it.

Another very important part of the work, the determination of the weight of a cubic decimetre of water, was carried out, in the first instance, chiefly by Lavoisier. This branch of the operation had been specially entrusted by the Committee to Lavoisier and Hauy. The necessary apparatus was constructed under Lavoisier's directions, and all the requisite measurements and weighings of the cylinder were made by Lavoisier and Hauy. Hitherto few details of the actual processes of this scientific determination have been given to the public, and the whole credit of determining the weight of a cubic decimetre of water, upon which the kilogram, the unit of metric weight, was based, has been attributed to Lefèvre-Gineau, to whom, in conjunction with Fabbroni, the work was entrusted after Lavoisier's death. In point of fact, Lefèvre-Gineau appears to have repeated, in the winter of 1798-9, all the observations made by Lavoisier and Hauy five years before, using the same instruments and obtaining nearly similar results.

The facts are stated as follows by Bugge, the Danish member of the Commission, in the thirtieth of his letters describing his visit to Paris, and published in 1800:—

"The final results of the labours of this special commission, consisting of Lefevre-Gineau and Fabbroni, to whom Van Swinden and Trallès were afterwards joined), was that the true kilogram, the weight of a cubic decimetre of water at its maximum density, or at 4° C., was 18,827 French grains of the old French pound, *poids de marc*.

"By the laws of August 1, 1763, and April 7, 1795, the kilogram is determined to be 18841 grains of the old French pound, *poids de marc*, in accordance with the experiments of Lavoisier and Hauy. This determination was adopted by the Chief Office of Weights and Measures in France, and the Standards have been hitherto made for the Departments accordingly. So that there now exist two kinds of kilograms, the legal or provisional, and the scientific or true kilogram. The difference between them is fourteen old French grains."

The difference is partly attributable to Lavoisier's determination having been made at the temperature of melting ice, instead of that of the maximum density of water adopted for Lefevre-Gineau's determination. The unit of Metric weight, the Kilogramme des Archives, appears to have been based on the later observations of Lefevre-Gineau, and to have been legalised by the law of Dec. 9, 1799, after Bugge's letter was written.

H. W. CHISHOLM

THE COMMON FROG*

VIII.

THE skeleton of the ankle as developed in the frog's class presents us with some characters, which, more than even those of the wrist, suggest the passage of the line of affinity directly from Batrachians to mammals, leaving both reptiles and birds on one side.

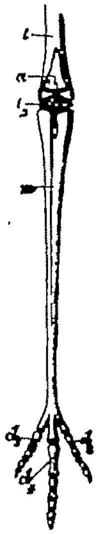

FIG. 51.

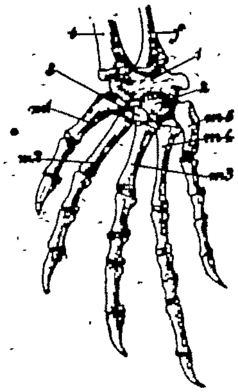

FIG. 52.

FIG. 51.—Right foot of Emeu. a, astragalus; d_2—d^4, second, third, and fourth digits; m, metatarsals anchylosed together except at their distal ends; t, tibia; t_2, distal tarsal element.

FIG. 52.—Left foot of a Monitor Lizard (*Varanus*). f, fibula; m^1—m^5, the five metatarsals, m^1 being that of the hallux; t, tibia; 1, astragalo-calcaneum; 2, cuboides; 3, ecto-cuneiforme.

In the first place we meet in the frog with certain extra ossicles in the inner side of the foot, which present the appearance of a broad rudiment of an extra digit on the inner side of the great toe. Now we find a structure very similar in form in animals remote enough from Batrachians, yet rarely do we find such in any intermediate kinds. Thus in certain tree-porcupines the ankle is furnished in like manner—another instance of the independent origin of strikingly similar structures.

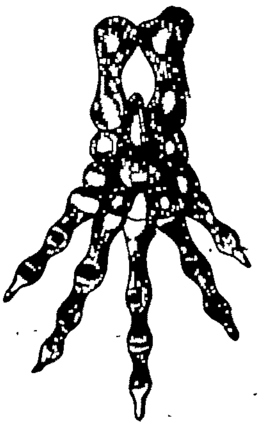

FIG. 53.

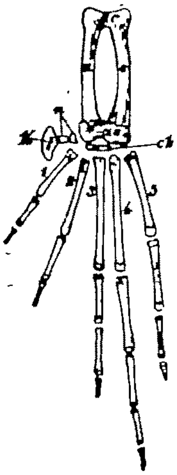

FIG. 54.

FIG. 53.—Skeleton of posterior extremity of an eft.

FIG. 53.—Bones of foot of Frog.—a, astragalus, c, os calcis; ac, united portions of these bones; hl, extra ossicle of inner side of foot; cb, ossicle representing cuboid and other tarsal bones—1, 2, 3, 4, 5—the five metatarsals.

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* Continued from p. 150.

There are other matters, however, more important than this. It has been remarked that the wrist shows an amount of resemblance to the same part in beasts which is wanting in most reptiles and in all birds. The same observation may be repeated with far greater force as regards the ankle.

FIG. 55.—The Maholi Galago.

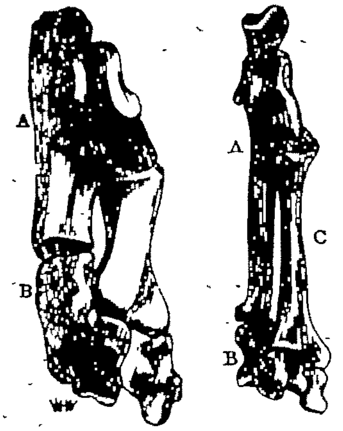

FIG. 56.—Elongated tarsus of Lemuroids. Left-hand figure, tarsus of *Cheirogaleus*; right hand figure, tarsus of Tarsius. *A*. calcaneum; *B*. cuboides; *C*, naviculare.

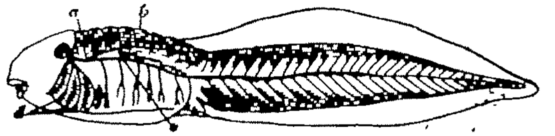

FIG. 57.—Tadpole of Bull Frog, partly dissected, to show the muscles of the tail and the branches of the 8th nerve or the *vagus*. a, great lateral branch giving off—b, a dorsal branch, and c, the lateral branch (or *nervus lateralis*), d, branches descending and passing along the branchial arches. The descending branches seen behind the branchial nerves on the side of the belly are not branches of the vagus at all, but spinal nerves, which come out from beneath the muscles and pass down under the *nervus lateralis*, and without having any communication with it.

In all beasts, as in man, the motion of the leg on the foot takes place by means of a joint between the shin-bone of the leg and the small bones of the ankle; and though in some beasts (as in the orang) there is considerable power of motion between the first and the second row of ankle bones; this is small compared with the mobility of the foot and ankle taken together, upon the leg.

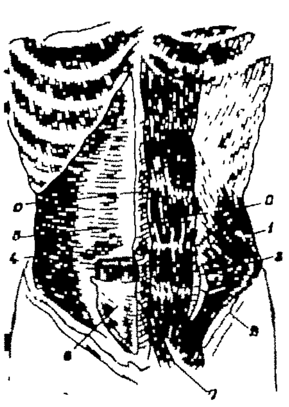

FIG. 58.

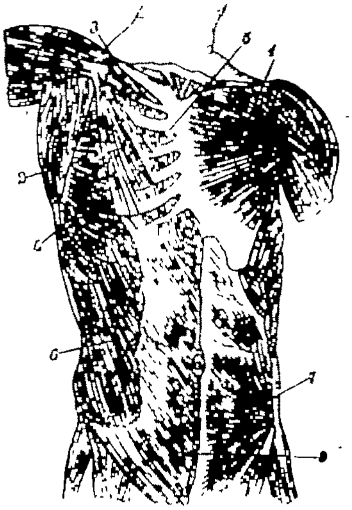

FIG. 59.

FIG 58.—Anterior muscles of the Trunk: the pectoralis major of the right side and the left external oblique being removed. 1, pectoralis major; 2, pectoralis minor; 3, subclavius; 4, serratus magnus; 5, internal intercostals, 6, external oblique; 7, internal oblique; 8, linea alba.

FIG. 59.—Deeper Abdominal Muscles—the external oblique being removed from the left side of the body, and the internal oblique and part of the rectus also, from its right side. 1, the internal oblique; its outer tendon (2) is cut and reflected from the outside of the rectus to show its deeper tendon (3), which passes within the rectus except towards the pubes, 4, transversalis, 5, its fascia; 6, sheath of the rectus—near the pubes, the conjoined aponeuroses of the abdominal muscles pass in front of that muscle; 7, pyramidalis; 8, rectus of left side, showing the tendinous intervals, or *lineæ transversæ*.

In all birds, on the contrary, not only is there no motion between the ankle-bones (as a whole) and the shin-bone, but the two rows of ankle-bones actually anchylose respectively with adjacent parts—the row nearer the leg coming to form one with the shin-bone; the second row coming to form one with the bones of the foot. Thus in birds the motion of the foot on the leg takes place not between the ankle and the shin-bone but between the two rows of ankle-bones.

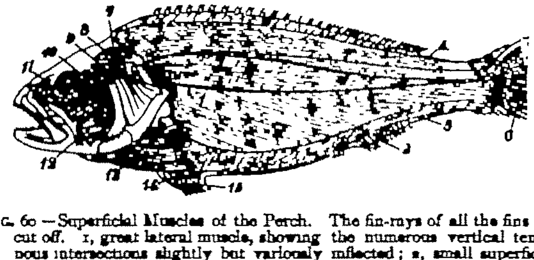

FIG. 60.—Superficial Muscles of the Perch. The fin-rays of all the fins are cut off. 1, great lateral muscle, showing the numerous vertical tendinous intersections slightly but variously inflected; 2, small superficial muscles inserted into the fin-rays of the dorsal and ventral fins; slender longitudinal muscle running (in the interval of the summits of the two great lateral muscles) between the dorsal and caudal fins, 5, similar muscle on the ventral margin, which also appears between the anal and ventral fins, 6, small radiating muscles of the caudal fin, 7, part of the great lateral muscle inserted into the skull, 8 and 9, elevators of the operculum; 10, elevator of the palato-quadrate arch, 11 and 12, muscular mass by which its contraction closes the jaws; 13, superficial muscles of the pectoral fin, 14 and 15, muscles of the ventral fin.

The same thing to a less degree takes place in reptiles; the ankle-bones do not indeed *anchylose* with the shin-bone and foot respectively, but they nevertheless unite with those parts so firmly that motion takes place between the bones of the ankle and not between the whole ankle and the leg.

Now in the frog's class, *e.g.* in the order *Urodela*, we meet with a condition which is mammalian rather than reptilian or avian. Motion takes place freely between the leg and the whole tarsus. Moreover, the number and proportions of the ankle-bones themselves far more closely agree with the condition of the same parts exhibited to us by certain beasts than it does with that which is possessed by any bird or of most reptiles.

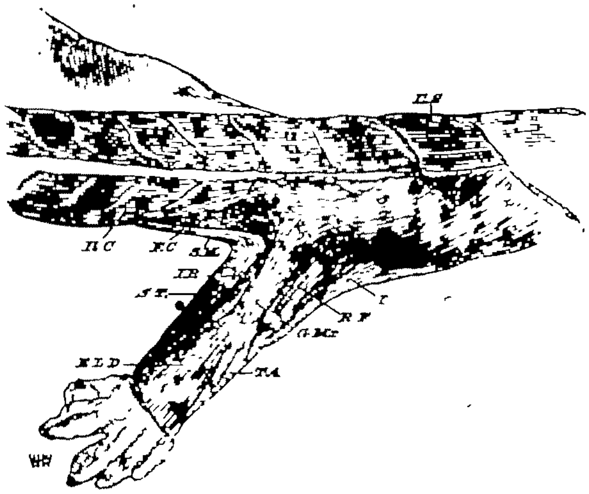

FIG. 61.—Superficial Muscles of Extensor Side of Leg and of parts of Trunk and Tail of Menopoma. *ES*, erector spinæ—directly continued into dorsal half of tail, *ELD*, extensor longus digitorum pedis, *FC*, femoro-caudal; *GMx*, probably rectus femoris, *I*, muscle resembling iliacus; *ILC*, ilio-caudal, *IP*, ilio-peroneal, *RF*, part of great extensor of thigh; *SM* and *ST*, muscles like the semi-membranosus and semi-tendinosus.

The frogs and toads, however, differ from the *Urodela* and present us with a peculiar condition of the ankle-bones, in that the two which represent the bones of the first row are so greatly elongated as to give to the limb an additional segment—as it were two "long bones" more.

We should search in vain through every other order of the Batrachian class, through every known group of birds and reptiles, both living or fossil, to find any analogous structure. None of the lowest mammals, no marsupial, no rodent, no insectivorous or carnivorous beast, no hoofed mammal, presents us with anything of the kind. Nevertheless, at almost the other end of the series, in the very highest order, that to which man himself belongs, we actually find a very similar development.

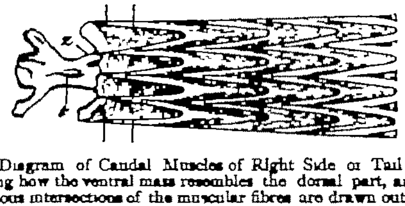

FIG. 62.—Diagram of Caudal Muscles of Right Side of Tail of *Iguana*, showing how the ventral mass resembles the dorsal part, and how the tendinous intersections of the muscular fibres are drawn out into cones. *N*, neural spine, *H*, hypapophysial spine, *z*, zygapophysis, *t*, transverse process, 1, dorsal series of cones, 2, upper lateral series of cones 3, lower series of cones, ventral series of cones.

Amongst the very peculiar beasts which inhabit the island of Madagascar, there are certain small creatures, "Half-Apes," belonging to the genus *Cheirogaleus*, in which two of the ankle-bones are elongated in a manner similar to that of the frog. The same character is more marked in an African genus of half-apes (*Galago*), and still more so in a third half-ape (*Tarsius*), from the island of Banca. Now it is absolutely impossible to believe that a special genetic affinity connects together by a peculiarly common descent, Half-Apes and Frogs! We are then driven to the conclusion that we have here again a striking similarity of structure in two instances which are quite independent in their origin.

That the power of rapid and prolonged "jumping" does not carry with it as a necessary consequence the elongation of ankle-bones, is demonstrated by the fact that in other animals which, to say the very least, jump no less than do these half-apes—as for example in the kangaroos, jumping shrews, and jerboas—it is not bones of the ankle but bones of the foot proper, which take on an augmentation in length.

The Muscles of the Frog

We may now pass to the consideration of some points exhibited by another set of structures—namely, the muscles.

The muscles of an animal constitute its flesh, which as the most ordinary inspection shows us, is composed of different portions of soft fibrous substance separated from one another by interposed layers of membrane. Each such portion, so separated, is a muscle, and is attached at its two ends to two parts (bones or what not), which may be adjacent or more or less distant. The fibres which compose it have the remarkable property of contracting under certain conditions, and, when contracted, the whole muscle is shorter and thicker than before, and the two parts to which it is attached become consequently approximated.

Muscles may be large expanded sheets of flesh (as in the abdomen) or long and more or less narrow, as in the limbs.

Muscles are said to be "inserted," or to "take origin from" the parts to which they are attached, and they may be so inserted either by their own muscular fibres or by the intervention of a tough membrane or a dense fibrous cord called a "tendon."

All the motions of an animal are produced by means of the contractions of its muscles pulling the bones, which act as so many levers (of different kinds according to circumstances), and so effecting locomotion.

These muscular contractions are in life produced by the agency of certain of the nerves proceeding from the nervous centres, *i.e.* from the brain and spinal marrow, and which carry an influence outwards to the muscles. Other of the nerves so proceeding convey an influence inwards to the nervous centres from an irritated portion of the body's surface.

The muscles, however, especially in the frog may, for a time, be made to contract after death by direct irritation of the nerves themselves.

After the skeleton, it is the muscular formation of the body which mainly determines its general form and aspect, though occasionally—and often in the Frog's order—the voluntary inflation of the lungs will alone produce a vast modification in an animal's appearance.

The curious and grotesque resemblance which exists between the figure of the adult frog and that of man has been a common subject of remark. It may then be less surprising to some to learn that there is a great degree of resemblance between the muscles of the Rational and of the Batrachian animals; though the much greater gulf which separates the Batrachian than the Reptilian class from mammals may lead others to anticipate a greater divergence than in fact exists.

The frog, however, in its immature stage of existence, is widely different from the adult in its muscular (or myological) furniture, and this from one obvious reason.

"Muscles" are, as we have shown, *par excellence*, "organs of motion," and the motions of the tadpole are essentially different from those of the frog.

The frog, as all know, progresses on land by jumps, and swims through the water by a series of movements which are in fact aquatic jumps. This action is familar to many of us, not only from observation but also by imitation (the frog being a swimming-master given us by nature), but it is none the less a mode of swimming which is very exceptional indeed.

The tadpole progresses through the water in a very different manner, namely, by lateral undulations of its tail, which is the usual mode of swimming among vertebrate animals—that made use of by sharks and porpoises, as well as by the overwhelming majority of fishes.

Studying the life-history of this one animal, then, we become acquainted with a process of direct transition from the condition of a fish to that of a quadruped, as regards a most important group of organs.

In ourselves, the back is provided with muscles which extend along its length in a complex series of longitudinal divisions, from the middle line outwards.

The abdomen of man is inclosed and protected by successive muscular layers laid one upon another, the fibres of the successive muscles being differently directed. Thus we have (1) the external oblique (the fibres of which pass obliquely downwards and backwards, (2) the internal oblique (the fibres of which pass obliquely downwards and forwards), (3) the *Transversalis* (with transverse fibres), and (4) the *Rectus abdominus* (situated in the middle line of the body, and with fibres directed antero-posteriorly).

In the frog we also meet with the vast sheets of muscle with oppositely directed fibres (the external and internal oblique) and with a median, antero-posteriorily directed rectus muscle.

A very different condition exists in fishes, where there is indeed a median antero-posteriorly directed rectus, but where the abdomen and tail are encased with a mass of muscular fibres not arranged in superimposed sheets, but as a series of narrow segments separated from each other by layers of membrane. The edges of these membranous layers, when the skin is removed, appear as a successive series of undulating lines proceeding from the back to the belly.

Now the tadpole exhibits a muscular condition (Fig. 56) quite similar to that of the fish, and in the great persistent larva the axolotl, we find no truly oblique abdominal muscles, but only as it were a hypertrophied rectus.

In other species of the frog's class which retains a tail throughout life, the marked superimposed lamellæ are distinctly developed, but more or less distinct traces are also retained of the successive membranous partitions separating the muscular segments of both the dorsal and ventral regions.

Another stage of development may be detected in the tail-muscles of certain reptiles.

Here the membranous partitions have become drawn at short intervals from above downwards out into a formal shaped condition, so that the muscular fibres enclosed, assume the forms of cones. Moreover, the apices of the membranes enclosing the cones, become denser in substance, and so modified into ligaments.

We come thus to have a key to the process of development, by which the muscles of the back may be conceived to have arisen.

The muscles of the back may be conceived as having arisen through increasing obliquity, conical prolongation, and partial detachment (from muscle) of the separating membranous lamellæ; the produced ends becoming condensed with firm tendons directed more or less obliquely forwards.

The muscles of the abdomen may be conceived as having arisen through atrophy, in that region, of the separating membranes and subsequent splitting up of the muscular mass into super-imposed sheets of differently-directed fibres.

This filiation between piscine and mammalian myology could hardly have been detected but for the remarkable series of gradations which the frog's class exhibits—

gradations both between species, and between different ages and conditions of one and the same species.

St. George Mivart

(*To be continued.*)

BEES VISITING FLOWERS

On the cliffs at Llwyngwril, near Barmouth, *Lathyrus sylvestris* grows in large patches, and is freely visited by humble-bees. Where a plant grows in considerable masses, a great number of bees are naturally attracted, and the competition among them becomes severe. In this case the flowers are not sucked in the usual manner, but the bees bite holes through the corolla, and obtain in this way illegitimate access to the honey. Hermann Müller has shown that when flowers grow in any quantity, they are so diligently worked at by the bees that only comparatively a few contain any nectar; it is therefore important for the bees to find out as quickly as possible whether a flower is worth anything or not. These holes, bitten through the corolla, enable the bees to visit the flowers more quickly, and are thus a great saving of time. He also says that, although the bee which first gnaws the hole loses time in doing so, yet the advantage of being able to get the honey from the young and as yet unvisited flowers, fully makes up for the loss of time.

In *L. sylvestris*, as in many Leguminosæ, the honey is secreted within a nectary formed by the filaments of nine of the stamens soldered together. The trough-like cavity thus formed is covered in above and converted into a tube, by the tenth stamen. But at the base, where the trough enlarges into a bulb, the stamen is not wide enough to cover it, so that it leaves a pair of holes piercing the tube one on each side. It is through these "nectar-holes," as they are called, that the bee, after passing its proboscis down the tube of the corolla, or, as in the case already mentioned, through the holes bitten at its base, gains entrance to the staminal tube, in its search for nectar.

In *L. sylvestris* the hole is gnawed through the tube of the vexillum, close to the edge of the calyx, and exactly over the left nectar-hole. (Throughout this paper I mean the right and left of an observer looking at the front of the flower.) I think the reason of this constant choice of the left side of the corolla is that the left nectar-hole is usually somewhat larger than the right. I found this to be the case in sixteen out of twenty-four specimens of the wild *L. sylvestris*, and in eleven out of sixteen in the garden variety (the Everlasting Pea). It is difficult to say how the bees have acquired this habit. Whether they have discovered the inequality in the size of the nectar-holes in sucking the flowers in the proper way, and have then utilised this knowledge in determining where to gnaw the hole; or whether they have found out the best situation by biting through the vexillum at various points, and have afterwards remembered its situation in visiting other flowers. But in either case they show a remarkable power of making use of what they have learnt by experience.

The united filaments not only form the nectary, but also a sort of casing in which the ovary is enclosed; and out of which the growing pod has to break its way as it increases in size. In *Vicia cracca* it does so by lifting up the tenth stamen, but in most *Lathyri* the filament is too stiff to allow of such a movement, and the growing pod was to squeeze its way between it and the edge of the trough formed by the nine united filaments. In doing this it enlarges and at last splits open one of the nectar-holes. In *L. sylvestris* the left nectar-hole, usually the larger of the two as I have before said, is almost always the one which is thus opened. In *L. pratensis*, on the other hand, where the nectar-holes are equal, the pod emerges indifferently to the right or left of the tenth stamen.

I am inclined to believe that the want of symmetry in the growth of the pod and the inequality in the size of the nectar-holes are in some way correlated, and that both are connected with a third asymmetrical character in the flower of this species. In most Lathyri the brush of hairs on the pistil is directed straight backwards towards the stalk of the flower. This is the case with *L. pratensis*, and also with the flower-buds of *L. sylvestris*, while very young; but, as they get older, the pistil rotates on its own axis, so that, in the adult flower, the brush is turned towards the left. I have often watched the bees sucking the flowers of the Everlasting Pea in the ordinary way, and have observed that the pistil, in consequence of being slightly bent as well as twisted on its own axis, emerges from the keel on the right side of the bee. The function of the brush is, as Mr. Farrer has shown (Nature, vol. vi. p. 479, 1872), to sweep the pollen out of the keel, so that it may be transferred to the bees visiting the flower, and may be in this way subservient to the cross-fertilisation of the species. I believe that the twisting of the pistil helps to ensure this end, since in consequence of the brush being turned towards the left it rubs against the bee and smears it with pollen. Thus the mechanism for ensuring the cross-fertilisation of the plant is made more complete. At present the supposition that the asymmetrical character of the pistil is connected with the above described peculiarities and in the growth of the pod, is merely a conjecture.

These facts have a certain bearing on a peculiarity in the structure of the staminal tube in *Phaseolus multiflorus*, the Scarlet-runner. This flower, in common with many Leguminosæ, has a pair of nectar-holes at the base of its staminal tube; but the tenth stamen differs, as far as I know, from that of any other Leguminous plant, in possessing a little flap which projects from its upper surface just in front of the nectar-holes, and which almost completely blocks up the tube of the corolla. Mr. Farrer supposes (*loc. cit.* p. 480) that by pressing with its proboscis against this flap the bee levers up the tenth stamen, and in this way passes its trunk into the staminal tube. If this occurs at all, it must be like gnawing holes in the corolla, an illegitimate way of treating the flower, since it is impossible to believe that it should have well developed, but totally useless, nectar-holes. I believe the true function of this curious little flap to be as follows:—In many Papilionaceæ, Lathyrus for instance, the insect visiting the flower rests on a platform which is formed of the carina and the expanded alæ, but in the Scarlet-runner this platform is made up by the alæ alone, the carina being tightly coiled into a spinal close up to the entrance to the tube to the corolla. The alæ are attached, one on each side to the proximal part of the carina, so that when an insect rests on them, its weight bears on the carina, and causes the pistil which is contained in it as in a sheath to be forced out. The direction of movement of the pistil is downward and to the left, so that a bee resting on the expanded alæ and pushing in its head to the left of the coiled-up carina would come in contact with the pistil as it darted out of its sheath; but if the insect went to the right of the coil it would escape the pistil altogether. The end of the pistil is covered with hairs, and performs the same function as the brush in Lathyrus in smearing the bee with pollen. It is, therefore, of great importance for the cross-fertilisation of the plant that the bees should go to the left of the coil. As a matter of fact they all but invariably do go to the left; the very few bees that I have seen going to the right appear dissatisfied and unable to find their way into the corolla. Now to reach the nectar-holes the insect's proboscis has to pass down a tunnel formed above by the tube of the vexillum, below by the upper surface of the tenth stamen; the entrance into this tunnel is a narrow archway leaning towards the left, *i.e.* having its highest point to the left of the middle point of its base. As before stated, the flap almost blocks up the tunnel, so that to get to the nectar-holes the proboscis must pass over the top of the flap, and must therefore travel along the highest part of the tunnel, but since at the entrance arch the highest point is to the left, the bee finds it necessary to go to the left of the coiled-up carina to reach the nectar-holes in the easiest way. If this view of the function of the flap, when considered in relation with the disposition of the pistil, carina, &c., be correct, it adds another instance to the long list of mechanisms for ensuring the cross-fertilisation of flowers by means of the visits of insects.

FRANCIS DARWIN

THE FRENCH MUSEUM OF PHYSICAL AND MECHANICAL SCIENCE

THE following official report of General Morin, the director of the Conservatoire des Arts-et-Métiers, Paris, to the Minister of Agriculture and Commerce, which we take from the *Journal of the Society of Arts*, furnishes some interesting details as to the present condition of this magnificent educational establishment, the like of which, dealing as it does with experimental and mechanical science, is entirely wanting in our country, although in the British Museum, the student of Natural History finds all he needs.

"The total number of persons who attended the lectures of the fourteen professors amounted in 1872 to 135,443, at 559 lectures, or in the proportion of 241 to each lecture. The smallest number of lessons given by any one professor was 40, from the opening in the commencement of November, until the last days of April. The total number of persons attending is smaller than in preceding years, which is explained by the decrease of the floating population of Paris. This year, as in all others, the decrease commenced when the days got longer, and work kept the people in the workshop.

"I would here limit this report if I did not think it necessary to add a few words upon the means of instruction which the Conservatoire offers to the public and the working-classes of all ranks.

"This establishment, as is known, owes its origin to the illustrious Vaucanson, inspector of factories, who, after having made at the Hotel du Montagne, Rue de Charonne, a collection of machines, instruments and tools, for the instruction of workmen, presented it to the Government, on the sole condition that its original purpose should be maintained. Louis XVI. accepted the gift by an act of council, and the illustrious Vandermonde, member of the Academy of Sciences, was named administrator and conservator of this first industrial museum. Later, by the decrees of the 15th and 18th of August, 1793, the Convention created a temporary commission of arts, to put a stop to the dispersion of objects of art, science, and industry. This commission succeeded in collecting a large number in a depot formed at the Hotel d'Aiquillon, Rue de l'Université. The value of these collections soon after determined the Convention, upon the report of Gregory, to make a decree, the 19 Vendémaire, year 3, that there should be formed in Paris, under the name of Conservatoire des Arts-et-Métiers, a public collection of machines, models, tools, drawings, descriptions, and books of all kinds of arts and science, the use of which should be explained by three lecturers attached to the establishment.

"It may be well to mention that the title of 'demonstrateur' or lecturer, often corresponded to that of professor, and that the professors of the Jardin des Plantes remained long after they had commenced giving regular courses. However that may be, the organisation of the Conservatoire, which was checked by several circumstances, was again mooted by Alquier at the Council of the Ancients, on the 27th Nivose, year 7, which urged the great advantage of such an institution to workmen, by saying that it is of more use showing them articles than merely speaking of them. It was not, however, until the 12th Germinal, year 7, that the buildings of the priory of St. Nicholas of the Fields were put into the possession of the members of the Conservatoire, who were then composed of Le Roy, Conti, Molard, and Benvelot, designer. The names of these savants, and that of Montgolfier, who soon after replaced Le Roy, did not allow of any comparison being made between the functions of these lecturers and those who are differently named now-a-days.

"At length, in the year 8, all the models and machines belonging to the State were definitively removed to this building, and formed collections destined solely for the instruction at sight. The functions implied by the title of lecturer were never exercised, and this will easily be believed when it is said that the numerous visitors who are attracted by the rich collections sometimes amount to 200,000, which makes all verbal explanation on the spot impossible. But that which is not possible to do for the public has been for a long time afforded by the Conservatoire to persons who are really desirous of information. A complete and methodical catalogue has been made out and published, and to it are added, from time to time, all new acquisitions; this has already passed through four editions. The galleries have been systematically classified, a guide has been placed in each, who, if he cannot give any practical explanation, can at least show where such and such a model is to be found, each of which is ticketed and numbered, both in the catalogue and in the inventory. Should an engineer or a workman wish to examine separately a machine or machinery, a study card for the necessary time is given to him. Or should any more complete information or explanation be required, either the curator of the collections, the under-director, or the director, is always ready to furnish them, their office being freely open to all.

"The staff in charge of the collections consists of the conservator, an assistant conservator, and of fourteen chosen guardians, who, for the most part, are picked from old non-commissioned officers or soldiers. The wish to give explanations by these, even with the aid of written details for the 9,000 models or articles which are there, would lead to great errors and confusion by a zealous but a badly instructed staff. In asking that popular conferences, such as are held at the Polytechnic Institution of London, should be introduced here, account has not been taken of the great difficulties which stand in the way, and greatly exaggerated ideas exist as to their value.

"It is not by common and vulgar explanations that the principles of Science can be spread amongst our workmen, and the facts and experience which are so necessary; their minds and intelligence are developed enough, so no fear need be had to speak to them on difficult scientific questions, if it is done with wisdom.

"All the professors who have followed this mode of teaching have often been convinced, on meeting some of their old hearers in workshops, that what may be termed the knowledge of truth and scientific principle has more deeply entered into their minds than into that of scholars of more celebrated schools. Hence it was not without reason that, in 1819, a decree of the king, brought about by the respected Dean, M. le Baron Charles Dupin, added to the instructions at sight given by the collections, that of oral instruction in the amphitheatres, by professors chosen from among the ranks of science. The number of chairs, at first only three, is now fourteen, and the half of the professors are members of the Institute, who diffuse and popularise science, the progress of which they promote by their labours. This instruction, unique of its kind in Europe, only takes place during winter; it is free to all without any condition for admission or any examination, and the number of persons who have frequented it during the last few years amounts to from 150,000 to 180,000.

"To the honour of workmen it must be said, that a more attentive audience can nowhere be found; never does the slightest disorder arise, and I am happy to say that during the unhappy events which have taken place in France, the Conservatoire was always respected, and underwent no disturbance or invasion.

"But if we think the part of casual lecturers in the galleries useless, and if we are convinced that the real duty of the Conservatoire des-Arts-et-Métiers consists in the classification, maintenance and increase of its collections, and in the teaching of the applied sciences, which it gives on such a large scale, we also believe that the Government should attach great importance to that teaching, which, during twenty years, we have developed under the name of technical education, and which has produced such good results in several of our great industrial centres.

"Your department pursues the realisation of this wish, and we hope it will be able, with the aid of the resources placed at its disposal by the National Assembly, to develop more and more this practical instruction, which, beginning at the primary school, gradually enables men, according to their intelligence and love of study, to rise from the lowest to the highest grades of society."

NOTES

We have with much regret to record the death of Mr. Edward Blyth, on December 27 last, in his sixty-fourth year. Of Mr. Blyth it may be said that he was a Zoologist in the truest sense of the word, and his practical knowledge of the birds and mammals of India and the surrounding countries was probably greater than that of any living naturalist. Up till 1840 he devoted himself to the study of the ornithology of the British Isles, and in that year appeared an English translation of Cuvier's "Regne Animale," in which the mammals, birds, and reptiles were edited by him; many of his own notes suggesting modifications in the then existing systems of classification, have been subsequently fully substantiated and adopted. For twenty-two years after this date Mr. Blyth held the post of Curator to the Calcutta Museum of the Asiatic Society of Bengal, during which time, and in conjunction with Dr. Jerdon, he did more than anyone to advance the study of Natural History in India, and to improve the value of the collection he controlled. After a short visit to Burmah, during which he did much good to zoological work, he returned to England in 1863, since which time he has contributed many valuable papers to ornithological and other journals, and under the very appropriate signature "Zoophilus," a large number of excellent articles to the *Field*. With an unparalleled memory Mr. Blyth combined exceptional powers of observation and a genuine enthusiasm for natural history, which is but rarely seen; these made his impromptu observations and opinions of more than ordinary value, and no one was more willing than himself freely to give all information at his command, towards the assistance of any fellow-worker, or the elucidation of any difficulty in his favourite subject.

Dr. Francis C. Webb, editor of the *Medical Times and Gazette*, died suddenly on the morning of December 24 last, at the age of 47 years.

At a preliminary meeting of the Varley Testimonial Committee, held on November 20, it was resolved to recommend that a Memoir of the late Cornelius Varley, illustrated with a Photographic Portrait, should be prepared and issued under the superintendence of the Committee, and that a copy be presented to his family, in token of the high estimation in which he was held; and further, that some Memorial be erected to his memory at the place of his interment.

Telegrams from Naples of the 3rd and 4th inst., state that Prof. Palmieri announces a severe eruption of Vesuvius to be imminent. A rumbling noise is audible from the mountain, and although fire has not been seen in the interior of the craters, the density of the smoke indicates the proximity of fiery matter

Mr. Manley Hopkins, Consul-General at Hawaii, having written to the *Times* that he had discovered in the Samoan Islands a living specimen of the Dodo, believed to have been extinct a century ago, Prof. Owen wrote to the same paper that the bird referred to is the dodlet. "The extinct dodo of the Island of Mauritius was about six times bulkier. Coloured figures of both birds—that of the dodo, copied from paintings by the Dutch artists, who saw the living bird in the time of their Stadtholder Maurice, that of the dodlet from the bird living in the Zoological Gardens about ten years ago, with the skeletons of both didus and didunculus are given in my work on the Dodo (quarto)."

A very suggestive anatomical point has been made out by Sir Victor Brooke, respecting the tarsus in certain of the *Cervidæ*. He finds that in the species of the genus *Cervulus* (the Muntjacs), the tarsus, instead of consisting of a naviculo-cuboid bone, together with two separate cuneiform bones, has the outer of the two cuneiform masses anchylosed to the naviculo-cuboid mass, to form a single bone, leaving the minute internal cuneiform free. In a very young specimen of *Cervulus muntjac* the cuboid was free, and the naviculare anchylosed to the outer cuneiform bones, showing that the tendency to blend in this direction is greater than that of the naviculare and the cuboid to combine. This same peculiarity is also found in the Pudu Deer of South America.

The question as to the limit of capability of the microscope is investigated by Prof. Abbe, of Jena, in a recent number of Max. Schultze's *Archiv*; and he is led by a series of physical deductions to the remarkable result, that this limit is already as good as reached by our best microscopes, and that all hope of a deeper penetration into the material constitution of things, than such microscopes now afford, must be dismissed. Experiment and theory agree in showing how the changes wrought by diffraction of light passing through fine structures, whose elements are so small and near each other as to call forth this phenomenon, are such as to prevent the object being imaged *more geometrico*. Thus it may happen, on the one hand, that different structures give the same microscopical image, and, on the other, that like structures give different images. Consequently, while objects of the kind (systems of fine lines and the like) may appear ever so distinct and well marked in the microscope, we are not entitled to regard such appearances as of morphological significance, but merely as physical phenomena, from which nothing further can certainly be inferred than the presence of such structural conditions as are capable of producing the diffraction effects obtained. The remark has notable applications to many of the microscopical researches on markings of diatoms, and on striated muscular fibre. And it affects not merely the morphological relations of the objects, but the deductions, made from microscopical observation, as to properties (such as differences of transparence, colours, polarisation, &c.). The author lays down the following principle as basis for determination of a limit:—By no microscope can parts be distinguished (or the marks (*Merkmale*) of a really present structure perceived), if they are so near to each other that the first bundle of light rays produced by diffraction can no longer enter the objective simultaneously with the undiffracted cone of light. Prof. Abbe has also recently described a new illuminating apparatus for the microscope, formed of a condensing system of two unachromatic lenses, which are fixed in the stage of the microscope, and transmit the rays from the mirror below; the purpose being that the object (immediately above the upper lens) may be illuminated by light from a great many different directions.

WE have received from the Science and Art Department "Extracts from the Reports of the Professional Examiners in Science on the Examinations of May 1873." The reports are most thorough and painstaking analyses of the results of the examinations, and give one the conviction that there is little chance for candidates who are not masters of the subjects they profess. On the whole the examiners report unfavourably on the great bulk of the candidates, and it would be for the good of future candidates, especially such as expect to be able to pass by cramming, that these reports should be brought under their notice.

AN observation of particular interest has been made by Mr. C. S. Tomes (*Quarterly Journal of Microscopical Science*). In studying the development of the teeth of the Armadillo (*Tatusia peba*), he finds, contrary to what would have been expected, that in their earliest stages, the first indications of their differentiation are manifested by the formation of an "enamel organ" as in those of higher mammals; whereas in the teeth themselves there is no enamel present, as is well known. Another peculiarity is that behind each primitive tooth a second smaller sac is seen, which corresponds in all its relations with the germ of the permanent tooth in other mammalia. Consequently, *Tatusia peba* at least, amongst the *Dasypodidæ*, is not monophyodont, as has been previously stated by Rapp, Gervais, and Flower, from which it may be inferred that the Edentata as an order, must have descended from a truly diphyodont type, and have become subsequently specialised.

THE U.S. Exploring Expedition under Lieut.-Col. Wheeler has now brought its work nearly to a close, having surveyed about 100,000 miles of territory in Southern Colorado, Southern Utah, Eastern Arizona, and Western New Mexico. The entire geological formation of all this vast country has been carefully studied, and from this study and the survey, accurate maps will be drawn this winter. Over 1,000 species of plants have been collected in Southern Colorado, and over 500 in Arizona and New Mexico. Some plants, supposed to have medical properties, or such as might prove of technical use, have been taken along for investigation. For example, the muskal, which is used by the Indians as a principal article of food, is tasteless when raw, but upon cooking, by being embedded in hot coals, turns sweet and is like the best honey. Attention was also paid to the geographical distribution of plants, and many interesting points elucidated. With regard to the fauna, more than 800 bird skins have been collected and stuffed by one of the naturalists, many of them very rare and beautiful. Some very rare reptiles have also been obtained, among which the gilamonster forms a peculiar feature. This animal, which is repulsive in appearance to many, and generally believed to be exceedingly poisonous, is quite harmless and very interesting. Some new species of rattlesnakes have been found. Very few butterflies have been seen, but bugs and beetles were collected in great quantities. The waters of the different streams were searched for the finny tribe. Skulls of bears and mountain lions and other specimens of the animal creation are included in the list of collections. With regard to the Indians, many interesting facts have been collected; among others the vocabularies of seven languages—the Apache, Navajoe, Tehua, Gohun (Tonta Apaches), Waltoa (Jemes), Isletta, and Moquis.

WE notice with considerable satisfaction from the statistics published in the *Publishers' Circular* that the number of scientific works issued during the past year in England bears a very large proportion to other classes and to the whole number of works published. The number of new books and new editions published during 1873, including 242 importations from America, is 4,991. Of these, 588 are classed under the head "Arts, Sciences, and Historical Works," by which, we presume, is meant Science theoretical and applied and the history of Science, as there are other heads under which history and the fine arts more appropriately come. This number, 588, is inferior only to that of works of fiction, and theological and religious works, the former numbering 831, and the latter, second in the list for the first time, 770. Were we to class "Voyages, Travels, and Geographical Research," 283 volumes, among scientific works, the number would be 871, exceeding even that of works of fiction, not to mention theology. The number of *new* books in Arts, Science, and Geography is 593.

DR. E. REGEL, Director of the Botanical Gardens, St. Petersburg, has published a work on the species of vines met with in North America, Northern China, and Japan, in which he discusses the long-controverted question of the origin of the vine. According to him, the cultivated vine, which forms our vineyards and produces our wines, is not a distinct and separate botanic species; it is a hybrid of two species, belonging equally to the genus Vine, viz. *V. labrusca* L., and *V. vulpina* L. The former of these two species is met with in a wild state in Northern America, in Japan, in Mantchuria, and in the Himalayas. Its leaves have their inferior face covered abundantly with a cotton-like down. The second species, which grows naturally in the same countries, has upon the inferior face of its leaves only small hairs, short and very stiff upon the nerves. The first of these two species has furnished the two most remarkable varieties of American vines, the *Catawba*, much cultivated for the production of wine, and the *Isabella*, the grape of which, sought after for the table, has a perfumed flavour and peculiar odour, agreeable to some, but disagreeable to others.

ON Thursday evening last, by invitation of the Committee of the Post Office Library, a large company assembled in the galleries of the new Post Office buildings, St. Martin's-le-Grand, in commemoration of the reopening of the library. In the south-west gallery there was arranged a museum of early telegraphic instruments and appliances, the latest improvements in the science of telegraphy being illustrated by the mode ot transmitting news to, and receiving messages simultaneously from, nineteen of the larger towns of the kingdom. The new process of despatching messages simultaneously in opposite directions through a single wire by the instrumentality of Mr. Stearn's invention was worked throughout the evening, communication having been effected for the purpose with Southampton. In the central gallery there were wires working in direct communication with Australia, India, Teheran, America, St. Petersburg, Paris, and Berlin, the process being rivalled in interest by the action of the pneumatic tubes which connect the Central Telegraph Station with the principal offices for the collection and delivery of messages in the metropolis. There was also in this gallery a working model of the travelling post-office, with the apparatus for the receipt and delivery of the mails while the train is in motion. In the course of the evening the Postmaster-General briefly addressed the company, sketching in outline the history of the English postal service. The Post Office Library was founded in 1859 for the benefit of the clerks and other officers of the Post Office. It was started by subscription among the employées, but has received large donations of books from authors, publishers, and the public. The library contains at present 2,500 volumes, of which we are glad to hear a fair proportion consists of popular scientific works, which it is hoped will be shortly increased.

MESSRS. LOVELL, REEVE, AND CO., have in the press a volume on St. Helena, comprising a physical, historical, and topographical description of the island, with its geology, fauna, flora, and meteorology. The author is Mr. J. C. Mellis, C.E., F.G.S., F.L.S., late Commissioner of Crown Lands, Surveyor and Engineer of the Colony.

THE *Scotsman* reports that a piece of gold-bearing quartz has been found in the island of Bute.

BEE-KEEPING has become a vocation or avocation of so much importance in America that there actually exists a "North American Bee-keepers' Society," which, like more important associations, meets yearly in one of the towns of the States. This year the society met at Louisville and continued its sittings for several days. Among the papers read was one by General D. L. Adair against the practice, common among apiarians, of clipping the wings of the queen, the paper showing a very considerable acquaintance with the structure of the bee.

THE *Times* takes the following from an American paper, and asks "Why not in London?":—"In Pittsburg, Pennsylvania, an electric clock has been established to move the hands of seventy different clocks, scattered all over the city. The motive clock is powerful, and has a pendulum composed of hollow coils of copper wire. These swing to and fro over the poles of horseshoe magnets, and every time they pass from one pole to the opposite a current of electricity is called up inductively in the coils, flows up the wire, and then to the seventy dials, giving a current of an opposite nature at each swing. Behind each dial is an astatic permanent magnet, suspended on a pivot, and surrounded by a coil of wire, and it rotates under the electric influence from the wires. A small weight may be used to each dial if the hands are heavy, and the pivoted magnet may merely regulate the time. Of course every clock will be exactly alike, and will run with very little attention. To prevent the pendulum of the motive clock from moving too fast by the increase in the length of vibration of the pendulum, a magnetic bridling apparatus is attached."

A LETTER appears in the *Times* of the 30th instant, from a correspondent with the "Livingstone East Coast Expedition," dated Mdaburu, Ugogo, Central Africa, July 15, and is principally occupied with a description of the many annoyances to which the expedition was subjected.

THE finest kitchen garden in France is that of Versailles, which belongs to the State, and brings in a yearly revenue, taking good and bad years together, of about 20,000*f*. The Assembly has determined to apply this valuable property to the formation of a model market garden and school of horticulture. The details of the institution are not yet arranged, but it is presumed that it will be self-supporting, and that it will render valuable assistance in the development of horticultural science in France.

THE additions to the Zoological Society's Gardens during the past week include two Violaceous Plantain-cutters (*Musophaga violacea*) from W. Africa, purchased; two Senegal Touracous (*Corythaix persa*) from W. Africa, presented by Mr. Hawkins; two Chinese Storks (*Ciconia Boycei*) from China, presented by Mr. R. B. Boyce of Shanghai; a Grivet Monkey (*Cercopithecus lalandii*) from W. Africa, presented by Mrs. Couteam; a Coati (*Nasua nasica*) from S. America; three Derbian Screamers (*Chauna derbiana*) from Columbia; a Chinese Water Deer (*Hydropotes inermis*); a Common Otter (*Lutra vulgaris*), British, deposited; two Black-tailed Hawfinches (*Coccothraustes melanurus*) from China, purchased.

ON THE SPECTRA OF COMETS *

THE spectrum-analytic method of examining the light from comets has only been applied hitherto to comets of weak light; yet the observations are fitted to extend considerably our knowledge of these objects. The spectra of all the comets that have been examined have consisted of a few bright lines or bands of light, and a very faint continuous spectrum. The chief part of the comet's light appears, accordingly, to be proper to it, and is probably from glowing gas, while the remaining portion is reflected sunlight.

Among the brightest comets which have appeared since the introduction of spectrum analysis are those of Brorsen (I. 1868) and Winnecke (II. 1868). The spectrum of the former consisted of three bright bands, whose position Huggins sought to determine with great accuracy; but he found no coincidence with the spectral lines of any terrestrial substance. The spectrum of Winnecke's comet, also examined by Huggins, was somewhat different, but similarly consisted of three bright bands (in addition to the continuous spectrum always present), which were sharply defined on the side nearest to the red end of the spectrum, but diffuse on the other. A comparison of the comet's spectrum with that of olefiant gas showed striking similarity between them; and Huggins was able to establish, with some certainty, a coincidence of the three bright bands. The expressed opinion that the material of this comet might be hydrocarbon found general acceptance; and the inference has been extended to other comets, so that it has been taken as demonstrated, that the comets are formed of hydrocarbons. (Dr. Zenker in *Astr. Nachr.* Nos. 1890 to 1893.)

I will now give a summary of all the observations known to me of cometary spectra, from which it will be seen how far the conclusion in question is warranted.

1. The first comet examined by spectrum-analysis is the Comet I. 1864. Donati found its spectrum to consist of three bright bands, which (if one may judge from the figure in *Astr. Nachr.* No. 1488) do *not* coincide with those of the hydrocarbon spectrum.

2. Huggins and Secchi observed Tempel's Comet I. 1866, and got from it a weak continuous spectrum, in which Secchi saw *three* bright lines, Huggins only *one*. The line seen by both was the brightest, and situated in the middle between *b* and *F* of the solar spectrum; accordingly *no* coincidence with the hydrocarbon spectrum.

3. In the spectrum of Comet II. 1867, the continuous spectrum was relatively so strong that Huggins found it difficult to detect bright lines. "Once or twice," he says, "I suspected the presence of two or three bright lines, but of this observation I was not certain. The prismatic observation of this faint object, though imperfect, appears to show that this small comet is probably similar in physical structure to Comet I., 1866." In this case, again, probably no hydrocarbon.

4. Brorsen's Comet I. 1868, was observed by Huggins and Secchi. Both observed three zones of light; the middle one being brightest, and lying in the green; while its brightest part was somewhat less refrangible than the brightest line of the air spectrum (wave-length = 500·3 mill. millim.). From this observation, and the determination of the position of the other two faint bands, it appears that the comet spectrum was *neither* similar to that of *nitrogen*, nor to the *hydrocarbon spectrum*.

5. Winnecke's Comet II. 1868, was also observed by Huggins and Secchi. The measurements and direct comparisons of Huggins gave an agreement of the cometary spectrum with that of carbon in olefiant gas. From Secchi's measurements it appears, that the sharply defined side of the middle band (towards the red end), nearly coincided with the line-group *b* of the solar spectrum; at which part also the beginning of the middle band in the spectrum of hydrocarbons is situated.

6. Comet I. 1870 was observed by Wolf and Rayet; the spectrum consisted of three bright bands, whose position, however, was not accurately determined.

7. Comet I. 1871 was observed by Huggins and myself. Huggins found *three* bands, I only *two*. The measurements of the bands observed in common agree well; the spectrum appears to be identical with that of Brorsen's comet.

8. Comet III. 1871 (Encke) was observed by Huggins three days, by Young four, and by myself six; it showed, as usual, a spectrum of three bands. Huggins thought this agreed with

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* Abstract of paper in Poggendorff's Annalen, by H. Vogel.

the hydrocarbon spectrum; while Young and I observed *no* such coincidence.

9. Comet IV. 1871 (Tuttle), examined only by me, gave a spectrum of three bands. Accurate measurement of their position showed *no* coincidence with the hydrocarbon spectrum.

Of these nine comets, there is only one (I. 1870) for which we have no observations as to the position of the bright bands. Of the remaining eight, the spectra of five (1, 2, 4, 7 and 9) have shown *no* agreement with the hydrocarbon spectrum. As regards the Comet II. 1867 the supposition is offered that its spectrum was similar to the spectrum named; as to Encke's Comet III. 1871, it remains uncertain in which class it is to be reckoned (Huggins' observations being at variance with those of Young and myself). There remains only the Comet II. 1868, for which Huggins' and Secchi's observations assert a probability of coincidence of the lines in its spectrum with those in the spectra of volatile hydrocarbons.

It thus appears a somewhat questionable view, that the comets consist of such matter; and we should, I think, content ourselves with the deduction, that a portion of the light emitted by the comet is its own light, and very probably from glowing gas. Perhaps a brighter comet may enable us to find out their nature more exactly, yet it seems to me extremely difficult to determine the nature of the glowing gas of the comet through a comparison of spectra from the electric spark in Geissler tubes; since there must be, in the comet, circumstances of pressure and temperature, which it is impossible for us to imitate, and through which, it is known, the spectrum undergoes great modifications.

Dr. Zenker has further asserted (*Astr. Nach. loc. cit.*) that "in the spectrum of Brorsen's comet, Huggins has recognised the bright line of nitrogen." This statement is incorrect; the observation having been, that the bright band situated in the green of the spectrum, had *nearly* the same position as the brightest line of the nebulæ, which, it is known, coincides with the double line of nitrogen. The band in the comet spectrum is a little displaced towards the red end; and this displacement could not be due to the motion of the comet, for, as Huggins pointed out, the latter was moving towards the earth, and the line would have been displaced towards the *violet*. At an earlier date, Huggins, observing the Comet I. 1866, gave out the opinion that the material forming it might be nitrogen; the spectrum appeared to consist of only *one* band of light, which nearly coincided with the brightest nitrogen line. But Secchi disproved this view, having observed three bands, and the weaker bands showing no coincidence with those of the nitrogen spectrum. The accurate measurements afterwards made by Huggins with the bright Brorsen comet, are of interest specially because they put it beyond doubt, that there is no connection between the spectrum of nitrogen and that of the comet.

Again, Dr. Zenker arrives at the conclusion that there must be water-vapour in the comets; since they have, according to Schmidt, a yellowish-red colour, and the sun's rays, when they pass through a considerable thickness of aqueous vapour, are coloured thus. But apart from the consideration that sunlight has a yellowish-red colour on passing through other vapours, as well as aqueous, I would remark, that we must take the proper light of the comet, which appears from spectral analytic observations, to be generally more intense than the reflected light, as determining its colour. According to the observations made, we should expect that the comet is, on the whole, of greenish or greenish-blue colour, since all the spectra consist, as we have seen, of two or three bands of light, of which one is in the yellow, the second and brightest in the green, and the weakest in the beginning of the blue. Of the (generally very faint) continuous spectrum, only the brightest part—yellow, green, and commencement of blue—is visible. The entire image, therefore, even where the weak continuous spectrum appears, will seem of greenish colour. Colour-data have been furnished by other observers besides Schmidt; and the head of the Comet 1811, *e.g.* had, according to Herschel, a greenish or bluish-green colour; the nucleus was slightly red. The colour of Halley's comet, at its return in 1825, was a bluish-green (Struve). Winnecke says of the comet of 1862, "The colour of the neck appears to me yellowish; the coma has bluish light."

With regard, lastly, to Dr. Zenker's proposition that "every gas belonging to the solar system, as soon as it is visible on the dark ground of the heavens, must appear with the same lines of the spectrum, as, according to its nature, it absorbs out of the sunlight," I may be permitted to remark that I am not quite convinced of this; there is not yet furnished a satisfactory experimental basis for the assertion. But to seek to explain the line spectrum of a nebula thus, and by saying that the nebula is shone upon by a fixed star in its "near neighbourhood," is doubtful, inasmuch as it is a very rare case that bright stars are situated in such nearness to nebulæ (especially the planetary, which best show the gas spectrum), that one can suppose a physical connection between them and the nebulæ.

I have been prompted to the foregoing remarks by the observation that in recent speculations on the constitution of the universe, the value of perceptions of sense, on which these speculations rest, has been greatly over-estimated. The principles on which the edifice of an hypothesis is raised must, above all, be secure, and observations not sufficiently confirmed, or even denoted as uncertain by those who have made them, should preliminarily be disregarded, if it is desired that the hypothesis have a stimulating and furthering influence on the progress of scientific research.

SCIENTIFIC SERIALS

Justus Liebig's Annalen der Chemie. Band 169, Heft 3.—This number of the Annalen contains the following papers:—On the decomposition of nitric acid by heat, by L. Carius. This paper, upwards of seventy pages in length, deals exhaustively with the subject. Very numerous tables of the results of various conditions of temperature, &c., are given, and the paper is illustrated with two plates.—On the chlorides of molybdenum, by Dr. L. P. Liechti and B. Kempe.—Chlorides of the formulæ $MoCl_2$, $MoCl_3$, $MoCl_4$, and $MoCl_5$ are described. The authors point out the parallelism shown by these bodies to the Tungsten chlorides, where, however, Tungsten wants the corresponding trichloride, while molybdenum wants the hexachloride. In both these series the colours of the salts become darker as the chlorine increases in quantity.—On the atomic weight of molybdenum, by L. Meyer. The author from sixteen results deduces the atomic weight 95·86 for molybdenum, chlorine being taken as 35·37 and silver 107·66. This agrees very well with the result obtained by Dumas 96, and by Debray 95·94. The author also points out the following relations in three groups of elements:—

V	51·2	Cr	52·4	Cu	63·3
Plus	43		43·2		44·4
Nb	94	Mo	95·6	Ag	107·7
Plus	88		88·4		88·5
Ta	182	W	184·0	Au	196·2

On chromic dioxide, by E. Hintz. The author describes the preparation, &c., of this body.—The number concludes with a paper on sulpho-ortho-toluidinic acid, by F. Gerver, and one on the specific heat of zirconium silicon, and boron, by W. G. Mixter and E. S. Dand.

THE new number of the *Quarterly Journal of Microscopical Science* contains many papers of interest. Prof. Allman commences by giving an account of Kleinenberg's researches on the anatomy and development of Hydra, in which, while he has confirmed many of the statements of former observers, he has shown the incorrectness of others, and has discovered several important points in its anatomy, specially in connection with the structure of the ectodermic layer, and the subject of development.—Prof. Martin Duncan records some observations on the method of development in *Fucus vesiculosus*, in which, after suggesting that they obtain their nutrition in part at least, from the organic matter always present in sea-water, he describes the growth of the terminal cells of two sets of finger-shaped processes; showing that by in-growths from the lateral walls, membranous septa are formed at the apices of the processes, an active mass of protoplasm occupying the extreme end.—Following this is a translation, with a plate illustrating it, of George O. Sars' paper on the anatomy of that aberrant form *Rhabdopleura mirabilis* (M. Sars), so peculiar in combining a creeping stem in which is an axial cord; lateral cells in which the somites are free, except that a contractile cord binds each to the axial cord; a pair of tentacular arms; a differentiated alimentary canal, and a foot-like process between the alimentary orifices. Mr. E. R. Lankester, in a separate paper, very clearly shows, with the aid of some excellent diagrams, that this animal is a true molluscan form, intermediate between the Polyzoa and Mollusca, and not in reality related to the Hydrozoa as imagined by M. M. Sars.—Mr. Tomes' observations on the development of the teeth of the Armadillo are referred to in our Notes.—A translation follows of the researches of Ph. van Tieghem and G. Le Monnier, on the *Mucorini*, condensed from their memoirs in the

Annales des Sciences Naturelles. It will well repay the study of microscopists.—Rev. E. O'Meare continues his researches on the Diatomaceæ, describing the *Achnanthes*, *Gomphonemes*, *Amphipleurae* and their allies.—Dr. Bowditch, of Harvard University, gives a new method of injecting the Lymph Spaces in fasciæ, by stretching fascia over the neck of a bottle; and injecting in several places a turpentine solution of alcannine with the point of the syringe partially perforating the fascia; allowing the whole to dry, during which process the fluid penetrates the finest lymph spaces.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 18, 1873.—"On the Nervous System of *Actinia*," Part I., by Prof. P. Martin Duncan, F.R.S.

After noticing the investigations of previous anatomists in the histology of the chromatophores, the work of Schneider and Rotteken on these supposed organs of special sense is examined and criticised.

Agreeing with Rotteken in his description, some further information is given respecting the nature of the bacillary layer and the minute anatomy of the elongated cells called "cones" by that author. The position and nature of the pigment-cells is pointed out, and the peculiarities of the tissues they environ also. It is shown that the large refractile cells, which, according to Rotteken, are situated between the bacilli and the cones, are not invariably in that position, but that bacilli, cones, and cells are often found separate. They are parts of the ectothelium, and when conjoined enable light to affect the nervous system more readily than when they are separate. Further information is given respecting the fusiform nerve-cells and small fibres noticed by Rotteken in the tissue beneath the cones, and the discovery of united ganglion-like cells, and a diffused plexiform arrangement of nerve is asserted. The probability of a continuous plexus round the *Actinia* and beneath each chromatophore is suggested, and the nature of the physiology of the structures in relation to light is explained.

The nature of the minute construction of the muscular fibres and their attached fibrous tissue in the base of *Actinia* is noticed; and the nervous system in that region is asserted to consist of a plexus beneath the endothelium, in which are fusiform cells and fibres like sympathetic nerve-fibrils. Moreover, between the muscular layers there is a continuation of this plexus, whose ultimate fibrils pass obliquely over the muscular fibres, and either dip between or are lost on them.

The other parts of the *Actinia* are under the examination of the author, but their details are not sufficiently advanced for publication. The nervous system, so far as it is examined, consists of isolated fusiform cells with small ends (Rotteken), and of fusiform and spherical cells which communicate with each other and with a diffused plexus. The plexus at the base is areolar, and its ultimate fibres are swollen here and there, the whole being of a pale grey colour.

Anthropological Institute, Dec. 30, 1873.—Prof. Busk, F.R.S., president, in the chair.—The following paper was read:—"Ethnological Data from the Annals of the Elder *Han*," Part I. Translated by A. Wylie, of Shanghai, with an introduction by H. H. Howorth. The Imperial Chinese Annals of the various Dynasties which are as yet almost untouched are distinguished by the extreme accuracy of their details, and in them is to be found a minute account of the intercourse of China with its neighbours, reaching back in contemporary annals to at least the second century B.C. The series of Chinese annals begins properly with those of the Han dynasty which reigned from about 202 B.C. to about 220 A.D. That was the golden prime of Chinese history, when the empire reached its furthest limits, when Buddhism was introduced and when a great literature flourished. During the dynasty of Cheou, the old imperial unity had been invaded by the creation of various feudatories who became almost independent. It was the aim of the immediate predecessors of the Han dynasty to destroy those feudatories and to restore the unity of the empire; and to effect that purpose all the ancient books and histories were ordered to be burnt. The annals, in the present communication, contain an account of the numerous conquests from the date of the Elder Han and embrace the history and migrations of a large portion of the peoples of Central and Eastern Asia. Mr. H. H. Howorth communicated the twelfth and concluding paper on the Westerly Drifting of Nomades: the Huns.

EDINBURGH

Royal Society, Jan. 5.—Prof. Sir William Thomson, president, in the chair.—The following communications were read:—A New Method of Determining the Material and Thermal Diffusivities of Fluids, by Sir William Thomson.—Continuants: A New Special Class of Determinants, by Thomas Muir, M.A.—Remarks upon the Foot-Prints of the Dinornis on the Sand Rock of Poverty Bay, New Zealand, and upon its recent Extinction, by T. H. Cockburn Hood.

DUBLIN

Royal Irish Academy, Nov. 29, 1873.—Prof. Jellett, president, in the chair.—Samuel Ferguson, LL.D., read a paper on "The completion of the biliteral key to the values of the Letters in the South British Ogham Alphabet."—The president read a paper on "The question of Chemical Equilibrium," the determination of the law, according to which an acid divides itself between two bases which are present in the same solution, has been long known to be one of the obscure questions of chemistry, it is generally admitted by chemists that there is a division, and that the relative masses of the two bases exercise an important influence upon the law which governs it, but the law itself remains unknown, and the object of Prof. Jellett's paper was to give at least a partial, possibly a final, solution to the problem, treating the question as one of chemical equilibrium, and defining these terms as follows:—Two or more substances may be said to be in chemical equilibrium if they can be brought into chemical presence of each other, without the formation of any new compound or change in the amount of any of the substances which are thus brought together. If an acid be added to a mixture of two bases, four substances will be present, *i.e.* two salts and two portions of bases remaining uncombined, these four are in chemical equilibrium—the question is why—and the author showed that there can be but one equation of equilibrium, inasmuch as the quantities of the four substances which are present in the solution, are functions of three independent variables, namely:—the original quantities of each base (2) and the original quantity of acid (1) denoting by b_1 b_2 the quantities of free base, and by s_1 s_2 the quantities of each salt respectively, the equation of equilibrium is necessarily of the form $U = F(b_1, s_1, b_2, s_2) = 0$, and the object of the author's investigations was to determine the form of the function F. The bases selected for experiment were quinia and brucia In quinia the rotatory power of any of its salts exceeds the rotatory power of the base. In quinia the reverse is the case, and as the result of careful and long continued experiments, it was proved that equilibrium is not troubled by dilution, for a disturbance could not arise without altering the rotation—there was no alteration, and the equilibrium, therefore, depended only on the ratios of the four substances, hence:—

$$U = F\left(\frac{I_1}{s_1}, \frac{b_2}{s_2}, \frac{b_1}{b_2}\right)$$

By a second series of experiments it was proved—putting r_1 = rotatory power of brucia, ρ_1 = rotatory power of hydrochlorate of quinia, r_2 = rotatory power of brucia, ρ_2 = rotatory power of hydrochlorate of brucia, r = actual rotation for acidulated mixture, a = total amount of acid corresponding to the unit of bulk of solution, x = amount which combines with the quinia, it is easily seen that

$$r = \left(\frac{\beta_1 x}{a}\rho_1 + (b) - \frac{\beta_1 x}{a}\right) r_1 + \frac{\beta_2 (a - x)}{a} \rho_2 + \left(b_2 - \frac{\beta_2(a - x)}{a}\right) r_2$$

where β_1, β_2, a are the atomic weights of the two bases and the acid respectively, and b_1 b_2 are the quantities of each base contained in the bulk of the solution. Solving this equation for x, we have

$$x = A a + B (r - b_1 r_1 - b_2 r_2), \text{ where}$$

$$A = \frac{\beta_2 (r_2 - \rho_2)}{\beta_1 (\rho_1 - r_1) + \beta_2 (r_2 - \rho_2)}$$

$$B = \frac{a}{\beta_1 (\rho_1 - r_1) + \beta_2 (r_2 - \rho_2)}$$

If r_o be the actual rotation caused by the unacidulated mixture, it is evident that $r_o = b_1 r_1 + b_2 r_2$. The foregoing may therefore be written

$$x = A a + B (r - r_o)$$

By a third series of experiments it was seen that if a solution of quinia is acidulated so that the quantity of uncombined base bears to the acid the same ratio as in the foregoing mixture between the uncombined quinia and the quantity x, and a solution of brucia is prepared so as to preserve the ratio of the uncombined brucia to $a - x$. Then the ratios in these of b_1 to s_1 and of b_2 to s_2 are the same as in the case of equilibrium, the rotation caused by these fluids being r_1 and r_2. Let them be mixed in the proportions $m : n$, and the rotation caused by the mixture is $\frac{m r_1 + n r_2}{m + n}$, and whatever be the ratio of $m : n$, there being no rupture of equilibrium, it is evident that if the ratios $b_1 : s_1$ and $b_2 : s_2$ have the values proper for equilibrium, the latter will be preserved, however the ratio $b_1 : b_2$ may vary. Hence, in mathematical language, $U = F\left(\frac{b_1}{s_1}, \frac{b_2}{s_2}\right)$ By a fourth series of experiments a mixture of solution of quinia and brucia was made, in which these bases have to each other the same ratio as the uncombined bases in the second series of experiments. A second mixture is made of the same solutions in which the bases have the same ratio as the combined bases in the second series of experiments. Sufficient acid is added to the latter mixture to convert the bases into salts. Here the ratios $b_1 : b_2$ and $s_1 : s_2$ have the values for equilibrium. If these now be added to each other in the proportion of $m : n$, the rotation caused by the mixture is—

$$\frac{m r_1 + n r_2}{m + n}$$

$r_1\ r_2$ being the rotation caused by each of the added fluids separately, it is inferred as before that $U = \int\left(\frac{b_1}{b_2}, \frac{s_1}{s_2}\right)$, but if U satisfy both these conditions it is easily shown mathematically that $U = \int\left(\frac{b_1}{s_1} + \frac{b_2}{s_2}\right)$, hence it is evident that the required equation of equilibrium is $\frac{b_1}{s_1} \div \frac{b_2}{s_2} =$ constant. The author showed the bearing of the law upon the theory that chemical combination is not statical but dynamical, observing that this theory is quite in accordance with the results obtained by him. (This valuable memoir will appear in full in the Transactions of the Royal Irish Academy.)

PHILADELPHIA

Academy of Natural Sciences, Aug. 19, 1873.—Dr. Ruschenberger, president, in the chair. "The Composition of Trautwinite." The author gave a few additional details concerning this new mineral, which was described in the Proceedings of the Academy for January 1.

Sept. 9.—Mr. Gentry communicated a notice of a great swarm of ephemerids which passed through the town of Lewisburg, on the Susquehanna River, on the afternoon of August 22. The swarm was estimated to be about a mile in length by nearly a half mile in width, and was so dense as even to obscure passers-by on the opposite side of the street.

Sept. 15.—The following papers were presented for publication:—"On a new American species of Glyptocephalus," by Theo. Gill; "Description of fifty-two species of Unionidæ," by Isaac Lea. The last-named paper was, on report of the committee, ordered to be published in the Journal of the Academy.

BOSTON, U.S.

Society of Natural History, Nov. 5, 1873.—Mr. F. W. Putnam read a paper on *Myxine*, a low genus of fishes, known to fishermen as *hags*, giving an account of its anatomy, which was illustrated by a series of specimens exhibited. The several species described by various authors must be reduced to one, having a wide geographical distribution, being found on both sides of the Northern Atlantic, and also on the southern coast of South America. Mr. Putnam showed that the variations in the number of lingual teeth, which are from eight to eleven in each row in specimens from the North Atlantic and from the Straits of Magellan, could not be considered as of specific importance. The different varieties of this species he considered as follows:—*Var. septentrionalis*, the short and thick form, from the North Atlantic, *var. limosa*, the long and slender variety, also from the North Atlantic; while the southern variety may be called *australis*, the name under which Jennyns described it as a true species.—Dr. Thomas Dwight read a paper on the "Structure and Action of Striated Muscular Fibre." His studies had been made on the muscles of the legs of the small water beetle *Gyrinus*. Their covering is quite transparent, and after the leg has been cut off and put into a drop of water under a covering glass, the contractions can often be observed for over an hour. He found that the fibre, at rest, consisted of narrow granular transverse stripes, with broad light-coloured bands between them. Close to the black stripe there was a glaring white reflection, but midway between two stripes the fibre was gray. When the fibre contracted the black bands came nearer together, and their granular structure became more obscure; the gray band disappeared, so that there was merely an alternation of black and white stripes. The ends of the white stripes bulged out during contraction. As the wave of contraction moved along, it was easy to see that there was no interchange of position between the black and the light substances, and no homogeneous transition stage, as is maintained by Merkel. When one part of the fibre is in contraction, the part from which the wave is running is put upon the stretch; the black bands are divided into two rows of granules, and there is less distinction between the white and gray substances.

PARIS

Academy of Sciences, Dec 29, 1873.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the formation of equations of the condition which results from the observations of the Transit of Venus on December 8, 1874.—A new answer to M. Pasteur, by M. Trecul. This was a general review of M. Pasteur's views as to the origin of yeast. M. Pasteur briefly replied.—A theoretical essay on the equilibrium and elasticity of pulverulent masses and on the thrust of non-cohesive earth, by M. J. Boussinesq.—On the isomerism of albumenoids, by M. Béchamp. The author gave many details with regard to various albumenoids; he had discovered three in cow's milk. M. Dumas confirmed the latter result, which he had himself attained by different means.—Action of water on sheet lead, by H. Marais.—Note on hybernating *Phylloxera* and on their agility and artificial restoration, by M. Max. Cornu.—Observations on a note of M. Menabrea relating to Lagrange's series, by M. Genocchi.—Researches on arsenious hydride, by M. Engel. The author has been repeating Wiederhold's researches on the supposed As_2H; he, however, did not obtain the substance in question.—Note on the action of iodine on uric acid, by M. F. Wurtz. The author found that when these bodies were allowed to act in the presence of water, alloxan and hydriodic acid were formed, and probably also urea with other bodies.—Synthesis of oxalyl urea (parabanic acid), by M. E. Grimaux.—On a new arrangement of the sulphate of copper battery, by M. Trouvé.—Observations on the existence of certain relations between the colouring and geographical distribution of birds, by M. A. Milne-Edwards.—On fossil remains of Batrachia, Lacertia, and Ophidia found in the phosphate of lime deposits at Aveyron, by M. Filhol.—On the development of the *phragmostracum* of the *Cephalopoda* and the zoological connection of the *Ammonites* with the *Spirula*, by M. Munier-Chalmas.—On waterspouts and cyclones, by M. E. Mouchez.—On the effects of Indian hemp, by M. A. Naquet.—During the meeting, elections were held for the posts of correspondent of the astronomical section, vacant by the deaths of Encke and Admiral Smyth, to which Messrs. Lockyer and Roche were elected.

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THURSDAY, JANUARY 15, 1874

THE POLLUTION OF RIVERS

THE Rivers-pollution Commissioners (Prof. Frankland and Mr. J. C. Morton), having, during the last five years, made and published more than 2,000 analyses of river and other waters throughout England, Scotland, and Wales, before and after pollution, and no less than 1,200 examinations of impure drainage waters, and having visited and reported concerning the effluent waters from 245 Chemical-dye and Print-works, Paper-, Cotton-, and Woollen-mills, and mines and works of various kinds spread over the country, have recently published their fifth report on river pollution from mining operations, reserving to themselves in their forthcoming and last report the consideration of the potable waters of Great Britain.

The importance of this systematic and thoroughly scientific examination of the composition of the running water on the surface of our country can hardly be over-estimated, and the value to the nation of the mere analyses of the waters is such as amply to repay the cost of the Commission. In addition, however, to the special value to each district, to corporate bodies, or even to individual manufacturers and riparian proprietors which these analyses of river and drainage waters possess, they are of the highest importance as forming the body of evidence upon which alone action can be taken with regard to legislation on the subject of the prevention or abatement of river pollution throughout the country. That some measure to ensure a greater degree of purity in our rivers, especially those passing through the manufacturing districts, must, before long, be carried out by the Legislature, is admitted not only by those who opposed Mr. Stansfield's proposals of last year, but even by the manufacturers who are now helping to foul the streams. The question which has to be settled is not whether anything is to be done to remedy the certainly disgraceful state of some of our streams, but rather to what extent can the purification be pushed without detriment to the industry of the district; and when this has been decided comes the next question, how this partial purification is to be effected. That it can only be a partial purification is clear from the conclusions of the Commissioners themselves, who do not propose any plan by which the water of our rivers, in populous districts at present little better than sewers, shall be so purified as to be fit for drinking purposes.

Without attempting to give even a part of the data upon which the Commissioners base their conclusions, and declining altogether, as inopportune, to criticise their scheme, it appears to be desirable that conclusions arrived at after so much labour and consideration should be made widely known.

The proposal, then, which the Commissioners make as in their opinion the best and most feasible means of legislating on the prevention of river pollution is the establishment of certain standards, the infringement of which shall render the water liable to be deemed polluting and inadmissible into any stream, provided always that no effluent water shall be deemed polluting if it be not more contaminated with any of the polluting ingredients than the stream or river into which it is discharged.

The standards are as follows:—

(*a*) Any liquid which has not been subjected to perfect rest in subsidence-ponds of sufficient size for a period of at least six hours; *or* which having been so subjected to subsidence, contains, *in suspension*, more than one part by weight of dry organic matter in 100,000 parts by weight of the liquid, *or* which, not having been so subjected to subsidence, contains, *in suspension*, more than three parts of dry mineral matter, or one part by weight of dry organic matter in 100,000 parts by weight of the liquid.

(*b*) Any liquid containing, *in solution*, more than two parts by weight of organic carbon, or 0·3 part by weight of organic nitrogen in 100,000 parts by weight.

(*c*) Any liquid which shall exhibit by daylight a distinct colour when a stratum of it one inch deep is placed in a white porcelain or earthenware vessel.

(*d*) Any liquid which contains, in solution, in 100,000 parts by weight, more than two parts of any metal except calcium, magnesium, potassium, and sodium.

(*e*) Any liquid which, in 100,000 parts by weight, contains, whether in solution or suspension, in chemical combination or otherwise, more than 0·05 part by weight of metallic arsenic.

(*f*) Any liquid which, after acidification with sulphuric acid, contains, in 100,000 parts by weight, more than one part by weight of free chlorine.

(*g*) Any liquid which contains, in 100,000 parts by weight, more than one part by weight of sulphur, in the condition either of sulphuretted hydrogen or of a soluble sulphuret.

(*h*) Any liquid possessing an acidity greater than that which is produced by adding two parts weight of real muriatic acid to 1,000 parts by weight of distilled water.

(*i*) Any liquid possessing an alkalinity greater than that produced by adding one part by weight of dry caustic soda to 1,000 parts of distilled water.

(*k*) Any liquid exhibiting a film of petroleum or hydrocarbon oil upon its surface, or containing, in suspension, in 100,000 parts, more than 0·05 part of such oil.

The Commissioners further add that any law having for its object the prevention of river pollution, should

1. Absolutely forbid, under adequate penalties, the casting of solid matters into river channels.
2. Enact the foregoing standards of purity below which any liquid discharges into water-courses should, with the exceptions already mentioned (certain few short mining rivers), be forbidden.
3. Give power to all manufacturers in towns, except those of gas, paraffin oil, pyroligneous acid, animal charcoal, tin-plate, and galvanised iron, to discharge their drainage water into the town-sewers under suitable regulations.
4. Confer additional powers on corporations, local boards, manufacturers and mine-owners, to take land compulsorily under "Provisional orders" for the purpose of storing their waste refuse, or of cleansing sewage or other foul liquids either by irrigation, filtration, or otherwise.

They are of opinion that Government Inspectors (similar to the Inspectors under the Alkali Act) should be appointed, to whom should be committed the duty of detecting and proving offences against the law, and of procuring the conviction of offenders. They consider that the formation of River Conservancy Boards for authorising and carrying out river improvements will in course of time become imperative, but they are convinced that the thing of immediate importance in connection with river improvement throughout the country, is simply the prohibition, under adequate penalties, of the gross pollution which at present renders so much of the running water of this country useless to manufacturers, agriculturists, and the like.

The time has not yet arrived for the full discussion of these proposals. We shall doubtless hear much on this subject in the approaching or in the next session in parliament. It is, however, certain, from the opposition made to the bill of last year, that manufacturers do not as a rule agree with the Commissioners as to the feasibility of enforcing the proposed standards of purity, as regards the effluent water from works of various kinds. Nor is public opinion respecting the other and far more important source of pollution, the sewage of towns, in a sufficiently advanced or satisfactory condition to render legislation easy. It is not, for instance, clear how one and the same system, say of irrigation, can be applied to all districts possessing different soils, rainfalls and situations. Indeed, the more we consider the whole question of the prevention of the pollution of rivers, the more difficult does any general method of treatment appear to be. Each locality has its own peculiarities, and a system of prevention which is suited to one district may be inapplicable or inexpedient in another. But even supposing that when the subject comes before Parliament that difficulties are found to be of such a character as to render it impossible to legislate upon the exact basis laid down by the Commissioners, still the value of their conclusions, and of the mass of experimental evidence which they have collected, is extreme: and they have most fully earned the gratitude of all those interested in the satisfactory solution of one of the most important, though most difficult, questions of our social economy.

THE CONSERVATION OF ENERGY

An Elementary Treatise on Energy and its Laws. By Balfour Stewart, M.A., LL.D., F.R.S., Professor of Natural Philosophy at the Owens College, Manchester. (Henry S. King & Co., 1873.)

IT is the proper function of Science to discover, among the ever-changing phenomena of the world, the permanent relations which are the conditions of reasonable thought. When we understand these relations well enough to express them in words we call them "Laws of Nature." When they rise to a higher stage of development and have become invariable habits of thought, we call them "Things."

Thus ice, under certain conditions, ceases to be ice. We observe that when the ice melts water appears in its place, and we find that there is always so much water in place of so much ice. We therefore obtain, in the first instance, a *law* of equivalence between a certain quantity of ice and a certain quantity of water, and finally, we arrive at the conclusion that water and ice are the same *thing* in different forms.

We are thus led to inquire what it is which remains permanent in the midst of all apparent changes, and the result of this inquiry has been the enunciation of a consistent definition of the quantity of matter in a body, and the establishment of the doctrine that the quantity of matter in a body is invariable, whatever transformations it may undergo.

This doctrine of the "Conservation of Matter" lies at the foundation of all reasoning, whether in physics or in chemistry. When the progress of Science rendered it possible to form exact ideas about the motion of bodies, men were again impelled to seek for something permanent, even in motion itself. They endeavoured to form some definition of the "Quantity of Motion" which should enable them to treat this quantity as a *thing* having a continuous existence. The long war between the followers of Newton and those of Leibniz as to whether, in estimating the quantity of motion, the mass must be multiplied into the velocity or into the square of the velocity, was not a mere debate about words and names, for it involved the question whether momentum or *vis viva* were the more fully possessed of that character of permanence which would justify its claim to the title of "The Quantity of Motion."

The doctrine of the Conservation of Energy is the most complete expression hitherto given to the belief that all the changes of phenomena are but different distributions of the same stock of energy, the total quantity of which remains invariable. The characteristic feature of scientific progress during the last thirty years has been the application of principles derived from this doctrine to the various branches of Science. The recent progress of the theory of heat is an instance of the direct and conscious application of the doctrine of the conservation of energy. In his electrical discoveries Faraday also was guided by the same doctrine, though less consciously, as he had no opportunity of becoming acquainted with it in the accurate form in which it may now be stated.

In the volume before us Dr. Balfour Stewart has explained, in a very clear and very elementary manner, what is meant by energy in its two forms, the energy of a moving system, and the energy due to the configuration of the system.

This exposition is so carefully drawn up that we think it ought to be intelligible even to students who approach the subject without any previous training in the technical dynamics of the ordinary text-books. This we consider a matter of great moment for the future progress of Science. It is no doubt easier, in dealing with the present generation of students, to gain their assent to doctrines about energy by deducing them from other principles which have been already taught them as the elementary principles of dynamics. But it is by no means always true in science that those principles which have been longest recognised are really the most elementary. The discovery of principles more fundamental and elementary than those which are already received, is not only of great importance in the philosophy of Science, but it tends to render Science less technical, and therefore more easily diffusible through the mass of society.

Dr. Balfour Stewart, however, has not only endeavoured to give to the ideas of Work and Energy their proper position among the most elementary ideas which we can form, but he has displayed an equal amount of freedom in treating the still more modern ideas of the Dissipation of Energy, and of the difference between exact and statistical knowledge.

Thus his very first words relate to

> *"Our Ignorance of Individuals*
>
> "Very often we know little or nothing of individuals, while we yet possess a definite knowledge of the laws which regulate communities.
>
> "The Registrar-General, for example, will tell us that the death-rate in London varies with the temperature in such a manner that a very low temperature is invariably accompanied by a very high death-rate. But if we ask him to select some one individual, and explain to us in what manner his death was caused by the low temperature, he will, most probably, be unable to do so. . . ."
>
> "Nor is our knowledge of individuals greater in the domains of physical science. We know nothing, or next to nothing, of the ultimate structure and properties of matter, whether organic or inorganic.
>
> "No doubt there are certain cases where a large number of particles are linked together so as to act as one individual, and then we can predict its action, as, for instance, in the solar system, where the physical astronomer is able to predict with great exactness the positions of the various planets, or of the moon."

We regret that we have not space enough to quote the whole of this introductory passage, which, in the unpretending language of clear thought, expresses ideas which have as yet been appreciated only by a very small number of scientific men, but which will, in due time, greatly modify the popular notions as to the nature of human knowledge.

The uniformities, therefore, which we observe in our experiments on quantities of matter containing many millions of molecules in continual motion, are uniformities of the same kind as those first explained by Laplace, and in more recent times wondered at by Buckle, and arise from the slumping together of innumerable cases, each of which is by no means uniform with the others.

This statement acquires still greater significance when it is combined with another consideration which Dr. Stewart, if we mistake not, has already insisted on in his opening lecture at the Owens College. This is the distinction between stable and unstable arrangements of matter and motion. A system, whether at rest or in motion, is said to be stable if a slight variation of its initial circumstances will, at the end of a finite time, produce only a slight variation in the configuration or motion at that time. If, on the contrary, a variation, however slight, in the initial circumstances, may produce, in a finite time, a large disturbance, the equilibrium or motion of the system is said to be unstable.

Dr. Stewart illustrates this by several examples, among which we may select a clock as an instance of a stable arrangement in which everything is contrived so that any slight disturbance shall produce as little effect as possible on the position of the hands at any future time. A rifle, on the other hand, is an unstable contrivance, for a very slight pressure on the trigger is sufficient to occasion the motion of the hammer and the explosion of the gunpowder—effects, the energy of which is out of all proportion to the work done on the trigger.

Thus we have stable arrangements which, when at work, are not easily put wrong, and unstable arrangements which are characterised by great delicacy of construction.

The rifle, however, as Dr. Stewart points out, is a machine which, though delicately constructed, is not incalculably so. Its instability is not like that of an egg balanced on its longer axis. But in an animal we find a structure composed of materials which are chemically unstable, so arranged that on account of the changes to which they are liable, the smallest disturbance may produce the most varied states of motion. If, then, an animal is to be compared to a machine, the delicacy of that machine must be incalculable.

It is a metaphysical doctrine, that from the same antecedents follow the same consequents. No one can gainsay this abstract statement. But it is not of much use in a world like ours, in which the same antecedents never again concur, and in which nothing ever happens twice. Indeed, for aught we know, one of the antecedents might be the precise time and place of the event, in which case experience would go for nothing.

The physical axiom which has a somewhat similar aspect is, "That from like antecedents follow like consequents." But here we have passed from sameness to likeness, from absolute accuracy to a more or less rough approximation. The axiom is now applicable only to systems of the kind which we have called stable, in which slight variations in the antecedents produce slight variations in the consequents. In unstable systems, like antecedents do not produce like consequents; and as our knowledge is never more than an approximation to the truth, the calculation of what will take place in such a system is impossible to us.

Dr. Balfour Stewart's discussion of the Dissipation of Energy is perhaps as satisfactory as it could be made in the space allotted to it, and without the use of mathematical methods. Energy is indestructible, but it may cease to be *available*. Here we have a word not familiar in pure science—a word connoting usefulness. We must therefore define what is meant by available, and state the conditions under which we are supposed to be placed

Energy is available when it can be made to do visible work. The conditions under which we attempt to transform energy into work are that we must make use of the interactions of a given system of bodies, moving within a given region of space, out of or into which neither matter nor heat can pass.

If these bodies are in visible motion, we first reduce them to rest by causing them to do a certain amount of work. We thus obtain their energy of visible motion.

If they are now at different temperatures, we convey heat from the hotter to the colder bodies by means of a heat-engine, till the whole system is at the same temperature. We thus obtain a second portion of the available energy.

Finally, if the pressures of different parts of the system are not alike, we allow the portions in which the pressure is great to expand, and so compress the portions in which the pressure is less, the volume of the whole system remaining constant. We thus obtain the third and last portion of the available energy.

The parts of the system are now at rest relatively to each other, and are all at the same temperature and pressure. No more work can be done by the system if it is enclosed within a fixed boundary through which neither matter nor heat can pass. We have exhausted its available energy.

But there are two methods, both of them however unavailable to us, by which the energy of a system, even when rendered in this sense unavailable, may be recovered. One is by allowing the substances to expand into infinite space; the other is by conveying all the heat through a perfect heat-engine into a refrigerator at the temperature of absolute zero. Hence the importance of excluding these two methods, by limiting the statement to a system enclosed by a boundary through which neither matter nor heat can pass.

Now the doctrine of the dissipation of energy asserts that by the mutual action of the parts of such a system its available energy may be diminished, but can never be increased. If there is difference of temperature, conduction of heat takes place, and this is always accompanied by a diminution of the available energy. If there is visible motion, friction occurs, and this renders a certain amount of the energy unavailable.

Here, then, we have an irreversible process always going on, at a greater or less rate, in the universe. If, therefore, there was ever an instant at which the whole energy of the universe was available energy, that instant must have been the very first instant at which the universe began to exist. If there ever shall come a time at which the whole energy of the universe has become unavailable, the history of the universe will then have reached its close. During the whole intervening period the available energy has been diminishing and the unavailable increasing by a process as irresistible and as irreversible as Time itself. The duration of the universe according the present order of things is therefore essentially finite, both *à parte ante* and *à parte post*.

But, according to pure dynamics, every motion of a system may be performed in the reversed direction subject to the same system of forces. If then at a given instant, every particle of the universe were to have the direction of its motion reversed so as to start anew with an equal but opposite velocity, everything would run backwards from the end to the beginning. We might attempt a description of a world thus recoiling upon itself—the rivers running up into the hills, heat flowing from cold bodies to hot, and men passing over the stage of life from their graves to their cradles, ignorant of the past and remembering only the future, as Shelley sings, in his musical delirium:—

> "We have passed Age's icy caves,
> And Manhood's dark and tossing waves,
> And Youth's smooth ocean, smiling to betray;
> Along the glassy gulfs we flee
> Of shadow-haunted Infancy,
> Through Death and Birth, to a diviner day."

But then we must remember that every characteristic of the past is now transferred to the future, so that if this reversal of nature were actually to occur, we would be quite unconscious of it.

> "Thus
> Our weakness somehow shapes the shadow, Time."

Now why is this state of things, though dynamically possible, physically absurd? Simply because it requires the *exact* reversal of the motion of every atom in the universe. If but one atom were to receive a velocity differing infinitesimally from an exact reversal, that atom would leaven the whole universe with that tendency to dissipation of energy which actually exists, and things would go on as they now do.

We must now conclude, by thanking Dr. Balfour Stewart for bringing before the general public in so clear and intelligible a form some of the more intellectual results of physical science. We hope, however, that, in the next edition, the comparison between Euclid's *reductio ad absurdum* and the experimental verification of the results of a physical hypothesis, as given in Art. 118, will be re-written, as it is one of the very few passages which remind us of what is called the popular scientific style.

WEBERBAUER'S "FUNGI OF NORTH GERMANY"

Die Pilze Nord-Deutschland mit besonderer Berücksichtigung Silesiens. Beschrieben von Otto Weberbauer Heft I. mit sechs nach der Natur gezeichneten colorirten Tafeln. (Breslau: Kern; London: Williams and Norgate.)

THE mycologist has no reason to complain that he has not ample opportunities for identifying the various objects which fall into his hands, if he has but patience and book-learning enough to enable him to avail himself of all the various sources of information. There are not only abundant collections of dried specimens, like those of Rabenhorst, Fückel, and others, on the Continent, with others at home, but every day is bringing forward some new publication of greater or less excellence, with figures illustrative of obscure, or little known species, as well as those which are of more general occurrence. In that most difficult department, the Hymenomycetes, he has a host of excellent figures in Krombholz, more recent copies of which, are, unfortunately, by no means equal to the original, while the analyses, for the most part, are unsatisfactory, and sometimes altogether deceptive. Eight numbers have already appeared of the Icones by Fries, which have all the advantage of coming from the author himself of nearly half of the species which are contained in the Epicrisis, a new edition of which is now in the press, including all the more recent additions, and which is proceeding with a rapidity which is somewhat wonderful, since the Prince of Mycologists is at least an octogenarian. It would be easy to mention other important works still in progress both in this country and abroad, but amongst them not the least so is the one whose title is given above, though from its nature the progress must, unfortunately, be somewhat slow. The first part now before us contains figures and analyses of twenty-six species in six plates, with descriptive letterpress, and two parts at least are promised every year.

Great care has evidently been taken in the identification, and it is, we think, a great merit that the author has been content to adopt the commonly received nomenclature, without carelessly sanctioning every new name which has been proposed by ambitious or shortsighted observers. We are glad, moreover, that the measurements are given in French millimetres, and not in parts of German inches, which require reduction.

As the asci vary much in length, only the thickness is given, though, under certain circumstances, as in Sphæriæ, it is often quite as variable as the length. In one or two instances we should have been glad to see more critical remarks, as, for example, under *Peziza venosa*, where the larger figure so exactly accords with that of Fries in the "Atliga och giftiga Svampar" of *Discina perlata*, that we should have been glad to have heard whether there is any real distinction between the two. As we used to find it every spring in our younger days, it was more like the figure of Greville's *Peziza reticulata*, than that before us. Something again might have been said respecting the resemblance of our author's very curious *Peziza corium*, to the North American *Peziza craterium*, with which it has evidently a close affinity. There is, we think, no doubt that the *Verpa digitaliformis* of England is the same with that figured by Herr Weberbauer. We shall be truly glad to find that this beautiful work meets with such success as to ensure its continuance.

It is quite curious to observe how an interest in fungi has rapidly increased in this country. The late Fungus Show at South Kensington was so well attended that the Council offer for next year a very ample list of figures, and as especial prizes are to be given for collections of novelties, or for cultivated species, the meeting will be one of much importance. Even in Scotland, where a short time since fungi were looked on as "abominations," there is a very active movement in their favour, especially amongst the clergy, who have made some very interesting additions to our Mycology, and a fungus-show is projected next autumn at Aberdeen. In England, where some of the older students are passing away, it is a great pleasure to know that the subject is taken up by such strictly scientific observers as Mr. Plowwright, Mr. Renny, and Mr. Phillips, not to mention many other names of great promise.

M. J. BERKELEY

OUR BOOK SHELF

Beeton's Science, Art, and Literature. A Dictionary of Universal Information; comprising a complete Summary of the Moral, Mathematical, Physical, and Natural Sciences; a Plain Description of the Arts; an Interesting Synopsis of Literary Knowledge; with the Pronunciation and Etymology of every leading term. Containing nineteen hundred and eighty Columns, and upwards of six hundred Engravings. 2 vols. (London: Ward, Lock, and Tyler. No date.)

THIS book does not pretend to be, and very evidently is not, more than a compilation from other cyclopædias, and from works on the various subjects of which it treats. So far as we have examined it, most of the information contained in it is derived from the former source, and it is impossible that any thoroughly trustworthy reference-book can be compiled in this manner, especially if the compiler or compilers have no special knowledge of the subjects with which they deal. The work pretends to give only a summary of facts, but in many of the articles much space is wasted by comment and reflection. There is absolutely no article on the Spectroscope, which is referred to *Spectrum*, an article without any illustrative cut, occupying one-third of a column, that might have been written twenty years ago. Why is there no article *Evolution*? and why, under *Development*, is the greater part of the short article occupied with the "Vestiges of Creation," and no reference whatever made to the state of the doctrine in Germany and America? Under the very specific heading *Crannoges* the general subject of Lake-dwellings is discussed, the writer evidently not being aware of the important distinction between the Crannoges of Ireland and the Lake-dwellings of Switzerland. A very poorly-executed copy of Keller's restoration of a Swiss lake-dwelling is the illustration to the article *Crannoges*. We say again no work of this kind can be regarded as a standard reference-book unless the editor has at his command a band of master specialists. The illustrations, as a rule, are inferior, and many of them seem well worn; many, moreover, are totally useless, such as those put beside the article *Drawing* and similar articles, which seem to be inserted simply to make the book take with a certain class. We think there is still room for a comprehensive reference dictionary containing information on all subjects compactly put together, No one at the present day, when there are such multitudes of special treatises in every department of human knowledge, would ever think of resorting to an encyclopædia to *study* a subject; and thousands, we believe, would be thankful for an all-comprehensive reference-book which should present in the briefest possible space the leading and latest facts under each heading free of all comment and speculation. Such a work might be as comprehensive as the "English Cyclopædia," or the "Encyclopædia Britannica," perhaps more so, and yet not exceed in bulk of matter the work at the head of this notice. All the scientific articles in such a work, however, and many others besides, could only be written satisfactorily on such a plan by men of special knowledge in each department; such men alone can judge what is of primary and what is of secondary importance.

Scientific Handicraft; A Descriptive, Illustrated, and Priced Catalogue of Apparatus. Vol. I. Mechanics, Hydrostatics, Hydrodynamics, and Pneumatics. By J. J. Griffin. F.C.S. Pp. 186. (London, 1873).

THIS is a useful Catalogue of Apparatus, which contains an account of the method of using the principal pieces of apparatus which are described. There are also suggestions for keeping instruments in good order. It will be found useful by those who select apparatus for purposes of school teaching or public lecturing; and Mr. Griffin has done good service by endeavouring to introduce as many new forms of apparatus, or modifications of old forms, from Germany and France, as he could obtain knowledge of.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Shrinking of the Earth and Terrestrial Magnetism

SINCE writing my previous letter (vol. ix. p. 141) I have received a note from Mr. Darwin, who says that in his work on "Coral Reefs," he arrived at the conclusion that volcanoes are not found in areas of subsidence. As I have succeeded, I think, in eliminating them from areas of upheaval, it may be that they occupy the boundary line of the oscillating land, and are stationed on or near the fissures and joints along which the earth's crust has given way. At all events, I invite a careful examination of those areas which we know to be rising, such as the northern circumpolar region, Australia, &c., in the firm conviction that volcanoes will not be found on any of them.

On another point I am very glad to say Mr. Darwin agrees with me, and I am therefore supported by his great authority. He tells me that in his work on "Volcanic Islands" he arrived at the conclusion that the great continents are rising, and the ocean beds sinking. This, of course, is only an hypothesis, and will remain so until the world has been carefully surveyed, but the large number of facts I have collected, and which will, I hope, be shortly published, go a long way towards proving it. Accepting this hypothesis, the next question we have to decide is whether the rising of the land is an absolute or a relative rising; whether, in fact, the earth's periphery as a whole is undergoing enlargement or contraction, is stretching or shrinking. To decide this by direct observation is not easy; for water being our only measure, the same effect will be produced either by the sinking of the one portion or the rising of the other, that is, of course, if the rising or sinking be general; while if it be a local rising at one place, it may (as is familiarly known, and as I shall point out presently) be due to the lateral pressure caused by an adjacent subsiding area. In the absence of direct experiment, we may be guided by analogies from other facts. These facts are of two kinds—astronomical and geological.

Since the days of Laplace, the nebular hypothesis has been generally received by astronomers, as the one which best meets observed facts. This hypothesis predicates the existence of gravitation everywhere, and shows how, by its influence, the various heavenly bodies have become condensed from nebular matter. It predicates that this force is still active everywhere, and that everywhere within our observation we have a condensation of matter in progress, matter condensing from a highly diffused condition to one of greater density. Thus each member of our own system, it is argued, is gradually and surely nearing the sun, and at the same time is shrinking, and the various planets are, in fact, in so many stages of evolution, and exhibit for us the various phases which the earth has passed through and will pass through before it is landed in the sun. This is all very elementary. I quote it only to show that the evidence of astronomy is that the earth is contracting, that its periphery is diminishing in area, and that therefore it is probable that the subsidence of the ocean-bed is absolute, while the upheaval of the land is relative only.

Geologists argue differently and yet come to the same conclusion. They argue that the original condition of the earth was an incandescent one, and that it has assumed its present form after a gradual cooling, that is a gradual contraction. In Mr. Geikie's words, recently reported in your pages, "Among the geologists of the present day there is a growing conviction that upheaval and subsidence are—concomitant phenomena, and that viewed broadly, they both arise from the effects of the secular cooling and consequent contraction of the mass of the earth." The evidence of geology, then, is at one with that of astronomy in making the shrinking of the earth absolute and not relative merely.

Now it is very clear that if the shrinking earth acquired a certain amount of rigidity, such shrinking would cease to take place uniformly, and the crust would give way along certain weak lines, and that corrugations, *i.e.* mountain-chains, and deep pits, or ocean hollows, would be formed; and not only so, but the sinking of a given area would give rise naturally to a certain thrust upwards of a contiguous area. To quote the graphic words of Mr. Geikie: "Some portions have sunk more than others. These having to accommodate themselves into smaller dimensions would undergo vast compression and exert an enormous pressure on the more stable tracts which bounded them. It could not but happen that after long intervals of strain, some portions of the squeezed crust would at length find relief from this pressure by rising to a greater or less height according to their extent and the amount of force from which they sought to escape." From this we may conclude (what I have not seen mentioned elsewhere), that from the contraction of the earth alone we may deduce the result that the land areas have been gradually growing larger and the ocean areas smaller; that originally when the crust was less rigid, its surface was almost uniformly level and covered with water, and that as it gradually became corrugated, the land first appeared as an archipelago of islands which were gradually joined together into continents in the way Australia was clearly constructed, comparatively recently; or in other words, that the proportion of subaërial to sub-aqueous deposits must diminish as we recede in geologic time, inasmuch as the area of sea, *i.e.* of water-covered surface, increases.

In this statement of the gradual shrinking of the earth there is little that is new, and if it accounted for all the facts I should not have troubled you with another letter. It has been taken for granted hitherto, if I be not mistaken, that areas of subsidence and upheaval are scattered about the world in a sporadic manner, with as little order and aim as plums in a pudding; that the earth being in process of shrinking, areas of subsidence occur at any point where the earth's crust is weak; but the evidence which I have collected and which I hope the Geographical Society will publish, goes far to show that these areas are not sporadic but continuous, and further, that the foci of upheaval are in the circumpolar regions. That it is there where we meet with proofs of current and rapid upheaval almost at every step, and the farther we go north or south from the equator the more rapid does the rise seem to be, while in the equatorial regions the land masses are to a great extent quiescent; we cannot resist the conclusion that the earth is stretching itself in the direction of its shortest axis, that its periphery is being thrust out in the direction of the Poles. Now as we have shown that the earth is absolutely shrinking and that when any local upraising occurs it is due to the lateral pressure caused by a subsiding area, it becomes interesting to inquire what kind of strain upon the earth would produce a squeezing of it out in the direction of the Poles. I can see only one explanation, namely, that the strain is being applied in the way of a stricture about the world's equatorial region, that it is girdled in that region by some force which is tightening upon it, and this tightening produces a partially compensating protrusion of the two polar regions. I conceive that in a spheroid constructed of partially elastic materials, the effect of such a stricture will cause, besides a sensible diminution of the whole periphery of the sphere, a lateral thrust at right angles to the pressure applied, and thus only can I account for it. This would, if I am not mistaken, have another effect, and this a very important one; it would induce magnetism in the earth, and that magnetism would have its poles in the regions of upheaval, and this is in fact so. The magnetic poles are strictly, so far as our evidence goes, in the very foci of upheaval of the circumpolar regions. This correlation of terrestrial magnetism with the force that is causing a tension about the earth's equator, if sustained would surely go far to explain that crux of physical science referred to by Sir William Thomson in his address to the British Association at Edinburgh, namely, the cause of the earth's magnetism; but my letter has already outgrown reasonable limits, and I must ask you to allow me to continue the subject in another. HENRY H. HOWORTH

Derby House, Eccles, Jan. 2

Vivisection

IT has been suggested that the study of Huxley's "Elementary Physiology" is likely to make children indulge in cruelty. Allow me to give the experience of the father of five boys on the subject.

Those old enough to be taught from that book are so; and have attended the professor's lectures and seen some of his experiments. The impression left on their minds, from the reverent and touching treatment of the subject by the able professor, has led to an improved and exalted respect for the rights and life of the meanest thing that crawls.

Although these boys are now at what may be called the "predatory age," the interest and respect they evince for animal life is mainly to be attributed to the beautiful and refining lectures of the worthy and humane Huxley.

G. W. COOKE

London, E.C., Jan. 5

Moraines

MR. FRY, writing in NATURE (vol. ix. p. 103), says that "a glacier which has retreated from its terminal moraine is always the source of a stream of water, and this stream always cuts through the terminal moraine." He infers from this that a lake cannot be formed by a moraine damming up a valley.

I can assure him that this is a fact which at least admits of exceptions. The valley of the Kander in the Bernese Alps is, in its upper part at least, full of the moraines of extinct glaciers, now mostly overgrown with pine forest. One of these dams up a side valley and forms the beautiful Oeschinen Lake. The lake is fed from the glaciers of the Blumlis Alp, and its water is consequently muddy. Except in most unusual floods, it has no outlet above ground, but the side of the dam farthest from the lake is one mass of springs of water as clear as the celebrated streams of Lauterbrunnen, which are evidently fed by the water of the lake filtering through the dam. The dam, being a moraine, is of porous material. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Dec. 24, 1873

Indian Snakes

I HAVE just had the opportunity of examining the cobra mentioned in my letter dated 12th inst., together with a very handsome one belonging to another snake charmer. This latter cobra also devoured a frog in the space of a minute or two after it was placed in the basket, the frog croaking audibly about half a minute after it was swallowed.

I append the description of these cobras for the benefit of those interested in such matters.

Naja tripudians.—Specimen A.—Colour above very pale olive with pair of conspicuous white, black-edged spectacles. A pair of black H-shaped marks on 12th, 13th, and 14th series (transverse) corresponding to spectacles. Posterior edges of hood above, dark olive. Blackish band 17th to 21st ventral and corresponding scales—rest of belly mottled with dark spots.

Lower anterior temporal in contact with three (3) other temporals.

Ventrals 182, sub-caudals 51, scales 23 series.

Specimen B.—Colour above, olive brown, with numerous pale olive irregular transverse bands and blotches. Belly mottled and barred with blackish. A pair of snow-white, black-edged spectacles. Interstitial skin of anterior central portion of hood pure white, scales pale olive; that of posterior portion and margins black, scales dark olive; colour of hood extending across back in strong contrast to the paler hue of the body.

A pair of white dark-edged spectacles beneath the hood, corresponding to pair above, but the white portion very much wider. Central spots below oval, black, situated on 10th, 11th, and 12th series of scales.

Scales of head pale olive, anterior margins of vertical, supraciliary and occipital shields dark olive, forming a double band across the head. Posterior margins of occipitals dark olive. A vertical infra-orbital streak of dark olive.

Lower anterior temporal in contact with three (3) other temporals. The following ventrals blackish, forming distinct bands 17th to 31st, 24th to 30th, 35th to 38th, 48th to 51st, 61st to 64th all inclusive. Beyond these there are dark bands but the ventrals composing them are not as a rule black throughout.

Ventrals 185, sub-caudals 53, scales 23

Sept. 17, 1873 R. H. PRINGLE

The use of Terms in Cryptogamic Botany

IT seems to me that there is a very perplexing want of uniformity in the names employed by different authors to indicate the reproductive organs of cryptogamic plants.

To a private student this want of formality in the nomenclature of homologous organs is very bewildering; especially when he happens to meet with a term which no botanical work or glossary within his reach explains.

In reading the Rev. M. J. Berkeley's "Introduction to Cryptogamic Botany," I have come across a term which I cannot find used in the same sense in any botanical work I have consulted.

In the division of algæ called Rhodospermeæ, he says, in speaking of the fruit, "indefinite spores in distinct nuclei."

In *Callithamnion corymbosum* he calls the expanded wall of the mother cell from whose endochrome the walls have been produced by cell division, the nucleus.

In some other genera, he calls the cluster of naked spore-threads the nucleus. In other genera the spore threads arising from a placenta, together with the conceptacles containing them are called a nucleus.

In *Wrangelliaceæ* it is stated that the nucleus is composed of pyriform spores arising from the endochromes of the terminal cells of the spore-threads.

I had first settled in my mind that nucleus was used by Mr. Berkeley as a general name in this division of algæ, for an indefinite cluster of spores.

On re-consideration it seemed to me that the term nucleus in the division *Gongylospermeæ* was not applied to the clusters of spores, but to the expanded wall of the mother-cell, or walls of the mother-cells, whose contents had been transformed into spores; and in the great division *Desmospermeæ* to the spore-threads] from whose cells the spores are produced. Having at length given up this supposition as untenable, it then occurred to me that "nucleus" did not mean the expanded walls of the mother-cells alone, or the clusters of spores alone, or the spore-threads alone; but was a general term applied to the fruit consisting in some cases of spores and spore-threads, in others spores, spore-threads and conceptacles, and in others of the expanded walls of the mother-cells and their contained spores.

When, however, I again read that in *Wrangelliaceæ* the nucleus is composed of radiating pyriform spores, I gave up all attempts at a solution satisfactory to myself.

Can any of your readers inform me what, in this division of algæ, is meant by the term "nucleus," and why it is only used in this division? Did the term not occur in a book written by so high an authority in Cryptogamic Botany it might be passed over as a piece of affectation on the part of the writer. D. R.

*POLARISATION OF LIGHT**

III.

WE now proceed to the consideration of the colours produced by plates of crystal when submitted to the action of polarised light. A crystal very commonly used for this purpose is selenite or sulphate of lime, which is readily split and ground into flat plates of almost any required thickness. If such a plate be placed between the polariser and analyser when crossed, it will be found that there are two positions at right angles to each other, in which, if the selenite be placed, the field will remain dark as before. The selenite is, in fact, a doubly refracting crystal, and the positions in question are those in which the plane of vibration of the ordinary ray coincides with that of the polariser (or analyser), and that of the extraordinary ray with that of the analyser (or polariser). In every other position of the selenite, and notably when it has turned through 45° from either of the positions before mentioned, or neutral positions as they may be called, light passes through, and the field becomes bright. If the thickness of the selenite be considerable, the field when bright will be colourless; but if it be inconsiderable, say not more than three millimetres, the field will be brilliantly coloured with tints depending upon the thickness of the plate.

Supposing however that, the selenite remaining fixed, the analyser be turned round, we shall find that in the first place the colour gradually fades as before; until when the analyser has been turned through 45°, all trace of colour is lost. After this, colour again begins to appear; not however the original tint, but its complementary; and in fact, there is no more sure way of producing colours complementary to one another than that here used. A general explanation of this change of colour is already furnished by our former experiments. Doubly refracting crystals generally, in the same way as Iceland spar, divide every ray, and consequently every beam of light which passes through them, into two, so that of every object seen through them, or projected through it on to a screen, two images are produced. These two, being parts of one and the same beam of light, would, if recombined, reproduce the original beam; and the same is, of course, the case with the two images. This may be rendered visible by using the double-image prism as an analyser, and throwing both images on the screen together. As the prism is turned round, it will be seen that, just as when no selenite was interposed, the images are alternately distinguished; but that when both are visible, their colours are complementary. And if the distance of the prism be so adjusted that the images overlap, it will be found that, when both are visible, the part where they overlap is always white, whatever be the thickness of the plate used.

An instructive experiment may be made by interposing an opaque object in the path of the beam of light, so that its shadow may fall upon the part of the field common to the two images. The shadow will of course intercept the light forming each of the images, and will consequently appear double. Suppose that the two images are coloured red and green respectively; then one of the shadows will be due to the shutting off of the red light, and the other to that of the green. But in the first case the space occupied by the shadow will be still illuminated by the green light, and in the second by the red. In other words, neither of the two shadows will be black, one will be green, and the other red. If in any part of their extent the two shadows overlap, the part common to the two, being deprived of both red and green light, will be black.

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* Continued from p. 169

But in order to explain how it comes to pass that colour is produced at all, as well as to find a more strict proof that the colours of the two images are complementary, we must have recourse to some considerations based upon the wave theory of light. And first as to the mode in which waves may be produced.

Consider a row of balls lying originally in a horizontal straight line. Let the balls start one after another and vibrate at a uniform rate up and down. At each moment some will be at a higher, others at a lower level, at regular intervals in a wave-like arrangement; the higher forming the crests, the lower the hollows of the waves. The distance from crest to crest, or from hollow to hollow, is called the wave length. The distance from crest to hollow will consequently be half a wave-length. This length will be uniform so long as the vibrations are executed at a uniform rate.

Each ball in turn will reach its highest point and form a crest; so that the crests will appear to advance from each ball to the next. In other words, the waves will advance horizontally, while the balls vibrate vertically.

If the row of balls were originally arranged in a wave form, and caused to vibrate in the same way as before, those on the crests would vibrate wholly above, and those in the hollows wholly below the middle line. When the balls originally on the crests rise to their highest points, those in the hollows will fall to their lowest positions, and the height of the wave will consequently be doubled. When the balls originally at the crests fall, those in the hollows will rise, both to the middle line; and the wave will consequently be annihilated. The first of these corresponds to a condition of things wherein the crests of the new wave motion coincide with those of the old, and the hollows with the hollows; the second to that wherein the crests of the new coincide with the hollows of the old, and *vice versâ*.

Hence, when two sets of waves are coincident, the height of the wave or extent of vibration is doubled; when one set is in advance of the other by half a wave length, the motion is annihilated. The latter phenomenon is called *interference*. When one set of waves is in advance of the other by any other fraction of a wave-length, the height of the wave, or extent of vibration, is diminished, but not wholly destroyed; in other words, partial interference takes place. The distance whereby one set of waves is in advance of another is called the *difference of phase*.

The Wave Theory of Light consists in explaining optical phenomena by vibrations and waves of the kind above described. And according to that theory the direction in which the waves move is the direction of propagation of the ray of light.

The intensity of light depends upon the extent of the vibrations or the height of the waves; the colour upon the number of vibrations executed in a given interval of time. And since throughout any uniform medium the connection of the parts and the rate of propagation may be considered to be uniform, it follows that the waves due to the slower vibrations must be longer than those due to the more rapid. Hence the colour of the light may be regarded as depending upon the wave length.

The substance to the vibrations of which light is supposed to be due, is an elastic fluid or medium pervading all space, and even permeating the interior of all bodies. A full statement of the reasons which have led philosophers to make this hypothesis would involve considerations derived from other sciences besides optics, and would be out of place here. But it may still be pointed out that one strong argument is furnished by the fact of the transmission of light from the sun and from the fixed stars through space, where no atmosphere or known gases can be conceived to exist. That the light so traversing interstellar space must be transmitted by a material substance, is a fundamental proposition of mechanical philosophy; and the hypothesis of the ether simply consists in attributing to the substance or medium the property of elasticity (a property possessed in a greater or less degree by all known bodies), without assuming anything else whatever as to its nature or relation to other substances.

In the illustrations of wave motions given above, the balls would represent successive portions or molecules of the ether; and the means whereby the motion of one molecule is transmitted to its neighbour, is the elastic cohesion attributed to the whole medium in the hypothesis above mentioned.

The difference between ordinary and polarised light has been explained above; and the mechanical contrivances devised for representing wave motion always have reference only to polarised light. But as this is the subject with which we are here concerned, the limitation in question is not of consequence. A variety of instruments have been constructed for showing the effects of compounding vibrations or waves under different circumstances. The best with which I am acquainted is that by Sir Charles Wheatstone.

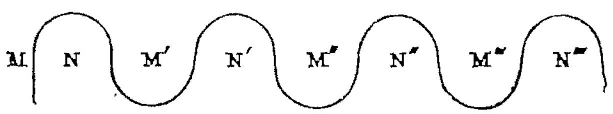
In plane polarised light, such as is produced by tourmalin plates, by double refraction in Iceland spar, &c., the vibrations are rectilinear, and are executed in one and the same plane throughout the entire length of the ray. In circularly polarised light the vibrations are all circular, and the motion is performed in the same direction. In elliptically polarised light the vibrations are all elliptical, the ellipses are all similarly placed, and the motion is in the same direction for the entire ray. These are the only known forms of polarisation, and indeed they are the only forms compatible with the usual, simplest assumption respecting the elasticity of the ether.

These general considerations being premised, we are in a position to trace the course and condition of a ray of light issuing from the lamp or other source, and traversing first the polarising Nicol's prism; secondly, the plate of doubly refracting crystal; thirdly, the analysing Nicol.

The vibrations of the ray on leaving the polariser are all restricted to a single plane. On entering the plate of doubly refracting crystal, every ray is divided into two, whose vibrations take place in planes perpendicular to one another. The angular position of these planes about the axis of the beam of light is dependent upon the angular position of the crystal plate about its centre. The two sets of rays traverse the crystal with different velocities, and therefore emerge with a difference of phase. The amount of this difference is proportional to the thickness of the plate. On entering the analyser the vibrations of each pair of rays are resolved into one plane; and are then in a condition to exhibit the phenomena of interference. If the plane of vibration of the analyser be parallel to one of those of the plate, that ray will be transmitted without change; the other will be suppressed. In any other position of the analyser those monochromatic rays (spectral components of white light whose difference of phase most nearly approaches to half a wave-length, will be most nearly suppressed; and those in which it approaches most nearly to a whole wave-length will be most completely transmitted. The amount of light suppressed increases very rapidly in the neighbourhood of the ray whose difference of phase is exactly a half wave-length; so that with plates of moderate thickness a single colour only may in general terms be considered to be suppressed. This being so, the beam emergent from the analyser will be deprived of that colour, and will in fact consist of an assemblage of all others; or in other words will be of a tint complementary to that which has been extinguished.

Next, as regards the colours of the two images, that is, the two which are formed either simultaneously by a double-image prism or successively by a Nicol in two positions at right angles to one another. In the first place it is to be remembered that the two sets of vibrations into which the selenite has divided the polarised ray are at right angles to one another; secondly, that one set is retarded behind the other through a certain absolute distance, which is the same for every ray, and consequently through a distance which is a different fraction of the wave-length for each colour; thirdly, that these two are re-combined or "resolved" in a single direction in each image by the analyser.

This being so, bend two wires in the following form:—



and place them at right angles to one another about their middle line M N M′ N′.., so that the points M of the two wires coincide, and likewise N, and so on. This will represent the condition of the vibrations as they emerge from the selenite, when the plate is of such a thickness as to cause a retardation equal to one or to any whole number of wave-lengths. Turn the wires about their middle line M N M′ N′ until they meet half way, *i.e.* in a position inclined at 45° to their original directions; this will represent the vibrations as resolved by the analyser in one image. Turn the wires about their middle line as before, but in reversed directions, until they meet in a position at right angles to the former; this will represent the vibrations as resolved by the analyser in the other image. On looking at the wires when so brought together, it will be found that in one case the crests fall upon the crests and the hollows upon the hollows, so that the vibrations combine to increase the intensity of the light. In the other case the crests fall upon the hollows and the hollows upon the crests, so that the vibrations interfere and completely neutralise one another.

The same principle would obtain if we shifted one wire along the middle line so that the points M of the two wires no longer exactly coincide. This would represent the condition of the vibrations as they emerge from the selenite when the plate is of such a thickness as to cause a retardation of a fraction of a wave-length equal to the amount of shift. And on turning the wires as before, we should find that in one image the waves partially combine, and that in the other they partially interfere. The shifting of the wires would represent either the effect of plates of different thickness upon waves of the same length, *i.e.* rays of the same colour; or that of a single plate on waves of different lengths, *i.e.* on rays of different colours. From these considerations we may conclude that the rays which are brightest in one image are least bright in the other; or, in other words, that the colours of the two images are complementary.

It has been remarked that the colour produced by a plate of selenite depends upon the thickness of the plate. In fact, the retardation increases with the thickness, and consequently, if, for a given thickness, it amounts to a half wave-length of the shortest (say violet) waves, for a greater thickness it will amount to a half of a longer (say green) wave, and so on. And if, instead of a series of plates of different thicknesses, we use a wedge-shaped plate, the entire series of phenomena due to gradually increasing retardation will be produced. This is easily seen to consist of a series of tints due to the successive extinction of each of the rays, commencing with the violet and ending with the red. And the tints will consequently have for prevailing hues the colours of the spectrum in the reverse order. This series of colours will be followed by an almost colourless interval, for which the retardation is intermediate between a half red-wave length and three half violet-wave lengths. Beyond this again the series of colours will recur; and the same succession is repeated as the wedge increases in thickness. It will, however, be observed that the colours appear fainter each time that they recur, so that when the thickness reaches a certain amount (dependent upon the nature and retarding power of the crystal) all trace of colour is lost.

It is not difficult to account for this gradual diminution in the intensity of the colours if, by means of a diagram, we examine the mode in which the waves of various lengths interfere with one another; but spectrum analysis furnishes an explanation which is perhaps more easy of general apprehension. If the light emerging from the analyser be examined by a spectroscope, it will be found, in the case of a plate giving the most vivid colour, that the spectrum presents a dark band indicating the colour which has been extinguished. On using thicker and thicker plates the band will be found to occupy positions nearer and nearer to the red end of the spectrum, until the band finally disappears in the darkness beyond the least refrangible rays that are visible to the eye. If the analyser be turned round the band will gradually lose its characteristic darkness, until, when the angle of rotation has reached 45°, the band will have disappeared altogether. The spectrum is then continuous, and when recompounded will give white light. This corresponds to the fact noticed before, that when the analyser is turned round, the colour given by a selenite plate fades and finally disappears when the angle of rotation amounts to 45° If the rotation be continued a band reappears, not, however, in its original position, but in the part of the spectrum complementary to the former.

If the thickness of the plate be further increased, two bands will be seen instead of one; with a still greater thickness there will be three bands, and so on indefinitely. The total light then of which the spectrum is deprived by the thicker plates is taken from a greater number of its parts; or in other words, the light which still remains is distributed more and more evenly over the spectrum, and consequently at each recurrence of the tints the sum total of it approaches more and more nearly to white light.

The following experiment will be found very instructive. Take two wedges of selenite or other crystal, and having crossed the polariser and analyser, place the two wedges side by side in the field of view so as to compare the tints produced by the two. Then place one over the other, first with the thick end of the one over that of the other; next with the thick end of the one over the thin end of the other. If the two plates are exactly similar, the combination in the first instance will be equivalent to a single wedge whose refracting angle is double that of a single wedge; and the number of bands produced will consequently be doubled. In the second combination the angles of the wedges will compensate one another, and the result will be equivalent to a uniform plate whose thickness is equal to the sum of the mean thicknesses of the wedges. The field will then be coloured with a uniform tint, viz., that due to a plate of the thickness in question.

By making use of the principle that the colour produced depends upon the thickness of the plate, selenites have been cut of suitable shapes and thicknesses, so as to produce coloured images of stars, flowers, butterflies, and other objects.

W. SPOTTISWOODE

(*To be continued.*)

ON THE MOTION AND SENSATION OF SOUND*

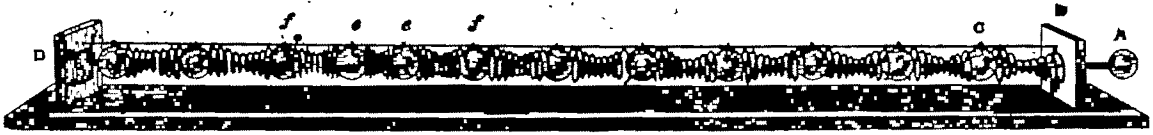
LECTURE I.

It is needless for me to say to the ladies and gentlemen who honour these lectures with their presence, that they are intended more especially for the instruction of boys and girls. As in all other cases where it has fallen to my lot to teach others, I shall endeavour, while avoiding superficiality, to strip the subject of all unnecessary difficulty, and of all parade of learning, and to present it in simplicity and strength to the youthful mind.

The title of the lectures is, The Motion and Sensation of Sound. Now every boy knows what I mean when I speak of the sensation of sound. The impression, for example, of my voice at the present time upon the organ of hearing is the sensation of sound. But what right have I to speak of the motion of sound? This point must be made perfectly clear at the beginning.

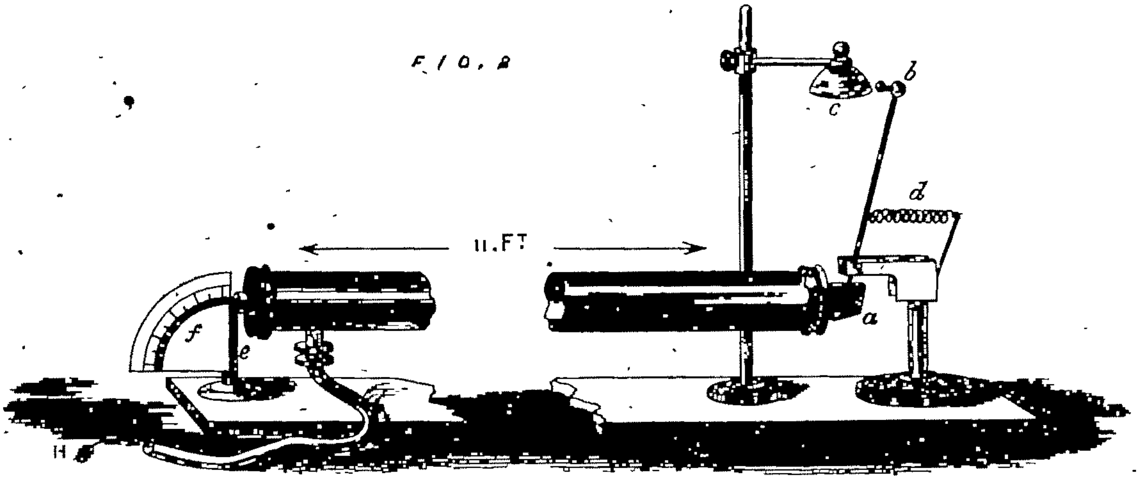
For this purpose I will choose from among you a representative boy, or allow you to choose him, if you prefer doing so. This boy, whom you may call Isaac Newton, or Michael Faraday, will go with me to Dover Castle, make the acquaintance of the general commanding there, Sir Alfred Horsford, and explain to him that we wish to solve an important scientific problem. He is sure to help us: he will lend us a gun, and an intelligent artilleryman; and we will make arrangements with this man to fire the gun at certain times during the day. We set our watches together; and now, before quitting him, we ask the artilleryman to fire one shot. We are close at hand, and we observe the flash and listen to the sound. There is no sensible interval between them. When we stand close to the gun flash and sound occur together.

FIG. 1



Well, we quit the artilleryman, warning him to fire at the exact times agreed upon. Let us say that the first shot is to be fired at 12 o'clock, the second at 12.30, and so on every half hour. We quit our artilleryman at half-past eleven, descend from the castle to the sea-shore, where a small steamer is awaiting us. We steam out a little better than a mile from the place where we have left the artilleryman; and now we pull out our watches and wait for 12 o'clock. Newton at length says, "In exactly half-a-minute the gun ought to fire;" and, sure enough, at the exact time agreed upon, we see the flash of the gun. But where is the sound which occurred with the flash when we were on shore? We wait a little, and precisely five seconds after we have seen the flash we hear the explosion; the sound having required this time to travel over a little better than a mile.

FIG. 2



We now steam out to twice this distance and wait for the 12.30 gun. We see the flash, but it requires ten seconds now for the sound to reach us; we treble the distance, it requires fifteen seconds; we quadruple the distance, and find the sound requires twenty seconds to reach us. And thus, if the day were clear, we might go quite across to the coast of France and hear the gun there. In all cases we should find that the flash appeared at the precise time agreed upon with the artilleryman, which proves that light reaches us in so short a time that our watches fail to give us any evidence that the light requires any time at all to pass through space, while the sound reaches us later and later the farther we go away. I think these experiments give us every right to speak of the "Motion of Sound."

But they also inform us how the velocity of sound has been actually determined. The most celebrated experiments on this subject have been made in France and Holland. Two stations were chosen ten or twelve miles apart; guns were fired at each station, and the interval between the flash and the report was accurately measured by the observers at the other station. In this way it was found that when the air is at the temperature of freezing water, the velocity of sound through it is 1,090 feet a second. On different days we should find it travelling at different speeds—as the weather grows warmer the sound is found to travel faster.

But I must not let you go with the idea that light requires no time at all to pass through space. This great problem has also been solved; and we now know that while sound moves at the rate of 1,090 ft. a second, light passes over the almost incredible distance of 186,000 miles in the same time. Hence at the distances employed in our observations, our watches were entirely unable to inform us that light required any time at all to pass through space.

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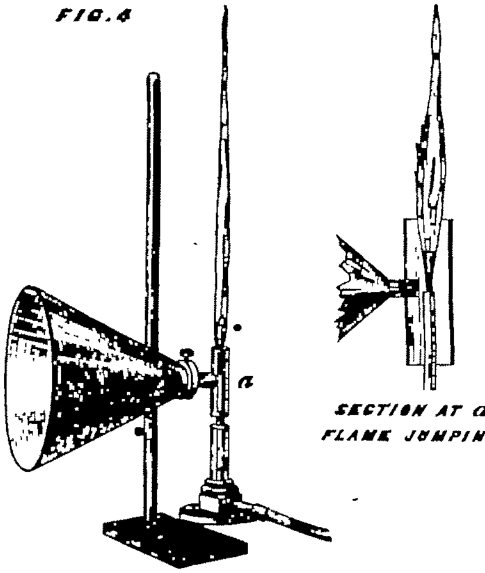
* Royal Institution Christmas Lectures, 1873–4, by Professor Tyndall, D.C.L., LL.D., F.R.S. These lectures have not been written out, much less intended for publication. At the request of our Reporter, Dr. Tyndall has consented to their appearance in NATURE.

But if I stopped here, your next question would be—What is this thing which passes through the air with a velocity of 1,090 ft. a second, and which, when it reaches us, makes us hear an explosion? We must give a thorough and complete answer to this question, but to do this we need a little preparation. Like sailors going into battle, we must clear our decks for action; and here I must ask you to give me your patient and resolute attention.

In order to know how sound is propagated through the air, we must first know something regarding the air itself. Let us examine the air.

First, the air has weight. It presses upon a single square foot of this table with the weight of nearly a ton (144 × 15 = 2,160 lbs.). I have here a glass cylinder covered at the top with a sheet of india-rubber. The air presses on that surface with the weight of nearly 900 lbs. But then you will ask how the india-rubber bears it. Why is it not pressed in? Because air is on both sides of it, and the pressure on the inside is exactly equal to that on the outside. But if I take away the air from the inside of the cylinder, you will soon see the india-rubber pressed down by the weight of air above it.

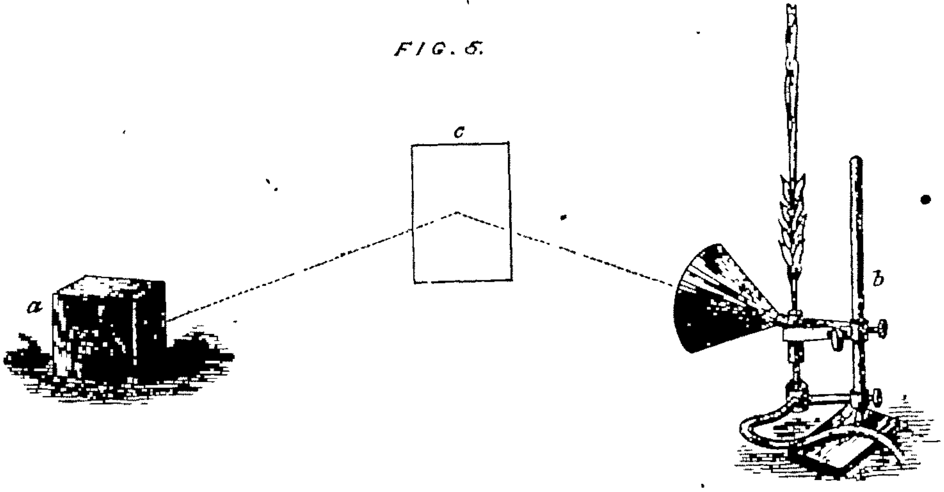
FIG. 4



SECTION AT a FLAME JUMPING

[A tube from an air-pump was then attached to a pipe communicating with the interior of the cylinder, which stood on a brass plate, to which its edges were ground parallel; the pump was set in action, and the india-rubber diaphragm at once sank down, in the end clinging to the sides of the glass, forming a deep vessel lining the inside of the cylinder.]

FIG. 5.



When the air is let in again, you observe the rubber returns slowly to nearly its primitive position; it would entirely, but that the india-rubber is a little over-stretched.

We have thus seen the effect of removing the pressure from the inside. What would occur if we took the outside pressure away? The india-rubber would expand. Instead of trying to remove the whole of the air from this room, which is impossible, I will cover these two slack and collapsed bladders with this glass vessel, fitting accurately on to the plate, over which they are suspended; and then draw off by the air-pump the air surrounding them. See how they gradually blow out; the folds are now nearly abolished; now they have become quite smooth.

Why is this? Because the air particles have the power of pushing one another apart, and thus take up sufficient space to fill the bladders when the external pressure is removed. The air in this room is pressed upon by the weight of the whole atmosphere. The repelling force which the air particles exert upon each other is called the elastic force of the air.

Now we have to consider how the sound of the gun is propagated through air. Does the gun fire anything through the air? No. We may in a rough way represent the particles of air by the solitaire balls arranged in a row close together in this groove. I take the first one and roll it against the second. You observe the row does not move, only the end one goes away. The first delivers up its motion to the second, and then stops, the second delivers its motion to the third, the third to the fourth, and so on until the last, which, meeting no resistance, flies off. In this way we may figure the motion as transmitted from particle to particle of the air.

A still better idea may be derived from this model (Fig. 1), which has been devised by the ingenuity of my assistant, Mr. Cottrell.

In my hand I hold a stem (A), passing through the upright (B), by which a shock can be sent from a ball (C) through a spring to another ball, thence through another spring to another ball, and so on, until at last the shock reaches the last ball, which is projected against the india-rubber pad at the end (D), placed there to represent in a rude mechanical way the drum of the ear. I press the stem (A), with a sudden motion of my hand, and you see that though the ball (C) only moves to and fro, yet it sends forward a kind of pulse (*f i i f*), which travels along the line, and ultimately causes the last ball to give a smart stroke against the pad (D).

If you could creep into the tube of the ear you would find, a little way in, a beautiful fine membrane called the tympanum, or tympanic membrane. The shock of the pulses of air falling on this membrane causes it to shiver; its tremors are transmitted to the auditory nerves, and by them are conveyed to the brain and cause you to have the sensation which we call sound.

You ought to be able now to figure the way in which the explosion of this pop-gun is transmitted through the air. I place a ramrod in the tube, there is a cork in the other end, and pushing the rod towards the cork, I cause a crowding together of the particles of air, this they resist, as I can feel by the force I am compelled to exert, and at last their combined resistance takes effect by blowing out the cork at the other end with a sort of explosion.

The suddenly expanding air communicates its motion to the air adjacent to it; this again to the air farther off; finally the condensed pulse strikes the tympanum of your ears, and you hear the noise.

I can show you the passage of a pulse through air in another way. We have here a tube 11 feet long, and about 4 inches wide, its two ends are closed by thin sheet india-rubber. Against the india-rubber surface at one end a cork gently presses (as in Fig. 2, *a*), to the cork a slender stem is attached having a little hammer at its upper end (*b*), kept from striking the bell (*c*), against which it abuts by a slender wire spring (*d*). If now a pulse be sent from the other end of the tube the india-rubber will drive away the cork, and will drive the hammer against the bell. A dull push will not ring the bell at the farther end. The particles of air are very mobile and readily slip round one another, so that it requires a sharp shock to generate a sound wave in the tube and make the bell ring outside the tube. I tap sharply with my fingers on the india-rubber and the sound of my tap and the blow of the hammer upon the bell at the other end of the tube are audible at one and the same time. This tube is 11 feet long, sound travels through air of the temperature of this room at about the rate of 1,100 ft. per second; the time therefore taken by the sound wave in traversing this tube is $\frac{1}{100}$th of a second, an interval of time far too minute to be measured by our ears.

Air is therefore a carrier or transmitter of sound. Suppose we remove the air from about a sounding body, will it then be heard? This experiment was made by Mr. Hawksbee a great many years ago (1705). A bell with a hammer worked by clock-work is placed under a glass globe. From the globe we will pump as much of the air as we can. At present you hear the sound with perfect distinctness, the pumping has at first apparently little effect upon the sound, but very soon it dies away, and now you see the hammer thumping away upon the bell, without producing any noise. It is doing its work in perfect silence. I allow the air to re-enter the glass globe, the tinkling sound of the bell is soon heard, and quickly grows up into the usual musical ring.

We have therefore proved that when the air is removed we have no sound, and when the air returns the sound returns also.

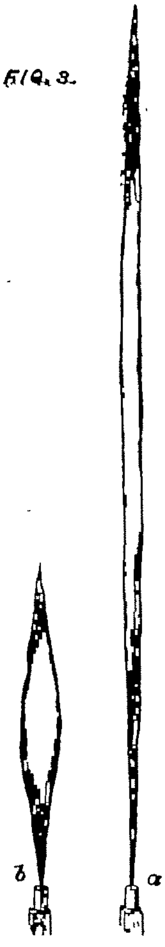
We will now follow the matter up a little further. Prof. Leslie found that when a little air was in the chamber surrounding the bell, and you could hear a little sound, that if the space from which the air had been taken was filled up with hydrogen, that the hydrogen quenched the sound. Now Prof. Stokes has shown us that to create a sound-wave in hydrogen a sharper tap is necessary than in air, so that the shock that produces a sound-wave in air does not suffice to produce a sound-wave in hydrogen (which is a much lighter and less dense gas).

My assistant, Mr. Cottrell, has devised the experiment I am about to show you to demonstrate this effect.

I have a long tin tube (Fig. 2) narrower than the one I used just now, but having like it a piece of india-rubber stretched over each open end, with a hammer and bell arranged against one of them, as before; at the other is a cork hammer fixed to a thin strip of steel, which can be drawn back to any given distance (measured on a graduated card). I have thus the means of sending a pulse along the tube as before and making the bell at the other end sound, but I now do it by a stroke of measured force. I now let hydrogen into the tube at the end adjacent to the striking cork (by the tube H), which is a little lower than the other end, and while the hydrogen is entering I continue to send pulses of measured strength along the tube; the bell continues to sound for a little while, but in one minute a sufficient amount of air has been displaced to cause the bell to cease ringing. When we remove the hydrogen you again hear the bell, showing that the pulse can again be carried from end to end of the tube.

Up to this point our illustrations have been audible; I now wish to render visible to you the action of a tube in preventing the dissipation of the sound. The test that I propose to use is a flame. I have behind the table a good-sized gas-holder, by which gas can be forced through a steatite burner. I light it, and we have that long pointed flame (Fig. 3 *a*), and we shall find that that flame is very sensitive. Chirrup to it, and see how rapidly it answers; a great part of the length of the flame is abolished instantly when the sound wave reaches it (Fig. 3 *b* and *c*). I rattle money, tap two keys, and this flame jumps in response to each jingle that I make. The current of air in the room, owing to our care for your comfort in the matter of fresh air, prevents these phenomena showing themselves as well as they do when the theatre is empty; but they are perfectly manifest. No one in this room can hear my watch ticking; but if I hold it near the flame you can distinctly hear the flame give a little roar, and see it suddenly shorten for each tick of the watch. The regularity with which it jumps indicates the regularity with which my watch is ticking.

FIG. 3.



And now observe the action of a tube in preventing the dissipation of sound. Using a less sensitive flame as the sound-test, I take off the india-rubber ends from our 11-foot tube, and place the flame at the end farthest from myself. The tapping of these two keys together does not affect the flame; but now, my distance from the flame being as great as before, I tap them opposite the open end of the tube, and each tap is rendered, by means of the flame, as visible to your eyes as it is audible to your ears.

Through the unconfined air this small bell does not affect the flame when set ringing; but when I place it at the extremity of the tube the flame dances to each stroke. Speaking-pipes possess their value solely from their preventing the dissipation of the sound pulses; they act precisely as this tube does.

As you know, light cannot well get round a corner; neither can sound, though it does so more easily than light. This little bell acts automatically. I wind it up and start it. At a few feet distance the flame answers to each stroke. Placed behind a board, the flame becomes tranquil. I again bring it out from behind the board, and the flame jumps to each movement of the hammer. (For this experiment the sensitive flame was arranged as in Fig. 4, with a large glass funnel having its tubular end opposite the root of the flame; the board was held about 10 feet distant from the mouth of the funnel.) Sound therefore can be shaded off in the same way that light can be.

In this box, which is well padded, is a bell which I can set ringing at pleasure. The only way by which the sound can get out is this small square opening at one side of it. The bell is now ringing without affecting the sensitive flame (arranged as in Fig. 4); but when this box is turned round, so that its opening faces the quiet flame, we have it dancing and jumping as before.

In other respects also there is a similarity between the mode of action of sound and light.

When a beam from the electric lamp is allowed to fall upon the glass mirror in my hand, it is reflected from the mirror, and the track of the beam being marked by the dust floating in the room, you can see the direction which it takes. This is in accordance with a well-known law, namely, that the angle of incidence is equal to the angle of reflection. It is perfectly plain to you that a line drawn so as to fall at right angles upon this mirror would divide that large angle formed by the two beams of light into two equal angles.

I hope now to make visible to your eyes the reflection of sound in obedience to the same law.

At one corner of the lecture table I place our sensitive flame (*b*), at the opposite corner the padded box containing the electric bell (*a*) with its opening directed in the path taken a moment ago by the beam of light, and I will hold this board (*c*), when everything is ready, where I before held the glass mirror. My assistant will now set the bell ringing. You observe that the flame is uninfluenced by it, but when I bring the board forward, the shortening of the flame at each stroke of the bell, proves that the law of the reflection of sound is the same as the law of the reflection of light: the angle of incidence is equal to the angle of reflection. In this case the flame is knocked down by an echo.

We have thus considered the reflection of sound from a plane surface; let us now see if it behaves like light when reflected from plane surfaces.

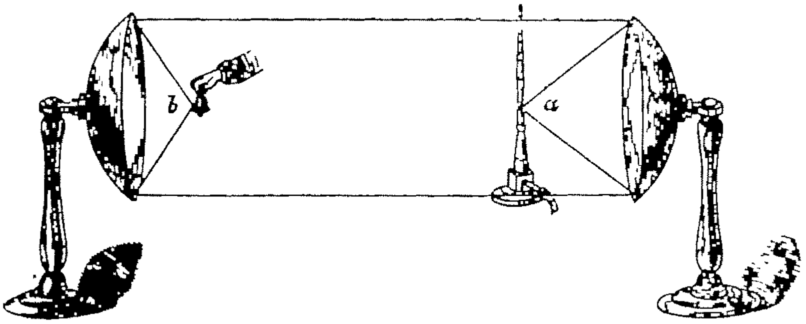


Fig. 6

The beam of the electric lamp is now directed upon the concave mirror. You can see the band of light marked in the fine dust floating in the air; as soon as it strikes the polished surface it is thrown back, but the rays no longer pursue parallel paths, they are converged, thrown together into one spot. By holding a piece of tracing paper at the point where they meet, termed the focus, the brilliant little star of light caused by their convergence is made visible.

Substitute for the lamp a small bell, and for the tracing paper at the focus of the mirror our sensitive flame, and the conditions are the same as in the previous experiment, sound-waves taking the place of the waves of light. You cannot see the track of these aerial pulses as you could the luminous ones, but their obedience to the same law of reflection is very manifest by the shortening of the sensitive flame as each sound wave reaches it. The flame when out of the focus of the mirror is unaffected; replace it in the spot when the sound waves are crowded together, and it responds to each stroke. Move the bell so that the sound pulses, though only having the same distance to travel to the flame, no longer fall on the mirror: the flame remains perfectly quiet.

We may go further still. Here are a pair of mirrors, the curvature and size of which is the same. They are arranged so as to face one another. A light is placed in the focus of one, that its rays which fall divergent upon the curved surface are reflected from it parallel, they travel to the opposite mirror, and are again converged; a piece of tracing paper held at the focus of the farther mirror shows the spot of light as before (Fig. 6).

Sound is reflected in precisely the same way, and the sensitive flame when carefully manipulated can be used as a means of proving this fact. For these experiments it is essentially necessary that the flame be reduced to the proper pitch of sensitiveness. By reducing the pressure of the gas we can regulate the flame so that it will not respond unless strongly agitated. The flame is placed in the focus of the mirror (*a*), and when the bell is rung, not being in the focus of the conjugate mirror, there is no action. I now bring it into the focus (*b*) and the flame shows a very strong action.

By other modes of experimenting it has long been ascertained that sound was thus reflected from plane and curved surfaces; but never before have these phenomena been made visible. Hitherto these effects have been investigated by the sense of hearing; I have now been able to prove them by appealing to your eyes.

(*To be continued.*)

SCHOLARSHIPS AND EXAMINATIONS FOR NATURAL SCIENCE AT CAMBRIDGE, 1874

THE following is a list of the Scholarships and Exhibitions for proficiency in Natural Science to be offered at the several Colleges in Cambridge during the present year:—

TRINITY COLLEGE.—One or more of the value of about 80*l.* per annum. The examination will commence on

April 10, and will be open to all undergraduates of Cambridge and Oxford, and to persons under twenty who are not members of the Universities. Further information may be obtained from the Rev. E. Blore, Tutor of Trinity College.

St. John's College.—One of the value of 50*l.* per annum. The examination (in Chemistry, Physics, and Physiology, with Geology, Anatomy and Botany) will be in December, and will be open to all persons who have not completed a term of residence at the University, as well as to all who have entered and not completed one term of residence. Natural Science is made one of the subjects of the college examination of its students at the end of the academical year, in May; and Exhibitions and Foundation Scholarships will be awarded to students who show an amount of knowledge equivalent to that which in classics or mathematics usually gains an Exhibition or Scholarship in the college. In short, natural science is on the same footing with classics and mathematics, both as regards teaching and rewards.

Christ's College.—One or more, in value from 30*l.* to 70*l.*, according to the number and merits of the candidates, tenable for three-and-a-half years, and for three years longer by those who reside during that period at the college. The examination will be on March 24, and will be open to the undergraduates of the college, to non-collegiate undergraduates of Cambridge, to all undergraduates of Oxford, and to any students who are not members of either University. The candidates may select their own subjects for examination. There are other Exhibitions which are distributed annually among the most deserving students of the college. Further information may be obtained of Mr. John Peile, Tutor of the College.

Gonville and Caius College.—One of the value of 60*l.* per annum. The examination will be on March 24, in Chemistry and Experimental Physics, Zoology, with Comparative Anatomy and Physiology, and Botany, with Vegetable Anatomy and Physiology; it will be open to students who have not commenced residence in the University. There is no limitation as to age.—Scholarships of the value of 20*l.* each, or more if the candidates are unusually good, are offered, for Anatomy and Physiology, to members of the College.—Gentlemen elected to the Tancred Medical Studentship are required to enter at this College; these Studentships are five in number, and the annual value of each is 100*l.* Information respecting these may be obtained from Mr. B. J. L. Frere, 28, Lincoln's Inn Fields, London.

Clare College.—One of the value of 60*l.* per annum, tenable for two years at least. The examination (in Chemistry, Chemical Physics, Comparative Anatomy and Physiology, Botany with Vegetable and Animal Physiology, and Geology) will be on March 24, and will be open to students intending to begin residence in October.

Downing College.—One or more of the value of 40*l.* per annum. The examination (in Chemistry, Comparative Anatomy, and Physiology) will be early in April, and will be open to all students not members of the University, as well as to all undergraduates in their first term.

Sidney College.—Two of the value of 40*l.* per annum. The examination (in Heat, Electricity, Chemistry, Geology, Zoology and Physiology, Botany) will be on March 24, and will be open to all students who intend to commence residence in October.

Emmanuel College.—One of the value of 70*l.* The examination on March 24 will be open to students who have not commenced residence.

Pembroke College.—One or more of the value of 20*l.* to 60*l.*, according to merit. The examination in June, in Chemistry, Physics, and other subjects, will be open to students under 20 years of age.

King's College.—One of the value of about 80*l.* per annum. The examination, on April 14, will be open to all candidates under 20, and to undergraduates of the college in their first and second year. There will be an examination in elementary Classics and Mathematics, in addition to three or more papers in Natural Science, including Physics, Chemistry, and Physiology.

Although several subjects for examination are in each instance given, this is rather to afford the option of one or more to the candidates than to induce them to present a superficial knowledge of several. Indeed, it is expressly stated by some of the colleges that good clear knowledge of one or two subjects will be more esteemed than a general knowledge of several.

Candidates, especially those who are not members of the University, will, in most instances, be required to show a fair knowledge of Classics and Mathematics, such, for example, as would enable them to pass the previous examination.

There is no restriction on the ground of religious denominations in the case of these or any of the Scholarships or Exhibitions in the Colleges or in the University.

Further information may be obtained from the Tutors of the respective Colleges; and the names, with certificates of character, date of birth, &c., must be sent to the Tutor of the College, in each case, several days before the examination.

It will be observed that in several instances the time of the examination is the same, certain of the Colleges having combined together so as to hold one or two examinations instead of each College holding a separate examination.

Some of the Colleges do not restrict themselves to the number of Scholarships here mentioned, but will give additional Scholarships if candidates of superior merit present themselves; and other Colleges than those here mentioned, though they do not offer Scholarships, are in the habit of rewarding deserving students of Natural Science.

It may be added that Trinity College will give a Fellowship for Natural Science, once, at least, in three years: and that most of the Colleges are understood to be willing to award Fellowships for merit in Natural Science equivalent to that for which they are in the habit of giving them for Classics and Mathematics.

ASTRONOMICAL ALMANACS[*]

X.—*Remodelling of the "Nautical Almanac" and the "Jahrbuch."*

NEARLY all the reforms which concerned astronomy were realised by Encke in the *Jahrbuch* for 1830, which appeared in May 1828. The appearance of this volume created an enormous sensation in England. The contest between Young and his opponents was then at its height. Strengthened by the help which had come to it from Berlin, the Astronomical Society redoubled its complaints and renewed its action; but the death of Thomas Young (May 10, 1829) soon occurred to simplify matters. In order that the question might not be hastily decided, the Society got the *Nautical Almanac* provisionally entrusted to the care of the Astronomer-Royal, J. Pond; at the same time it appointed a commission of forty members, composed of the directors of all the observatories and the principal astronomers and mariners, English and foreign.[†]

At last, at its annual meeting in February 1830, the Society awarded to Encke its gold medal for the great service which he had rendered to astronomy. "It would be superfluous," said Sir James South, President of the Society, in the address which he gave on that occasion, "for us to enlarge upon the merit of this well-known work, which, beyond all rivalry, ought to be regarded as the only ephemeris on a level with the requirements of

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[*] Continued from p. 49.

[†] Struve took part in this commission.

Science, as the manual and guide of practical astronomy, wherever it may be cultivated."

But if the English are impartial and generous towards strangers, they could not bear to be for any time inferior in the various services which touch upon their interests. The sub-committee * charged with preparing the plan for reorganising the *Nautical Almanac*, presented its report to the Society at the meeting of November 19, 1830, a report which was adopted by the Council and immediately approved by the Admiralty. The results of this beneficial agitation were of immense value to astronomy and navigation, and the improvements introduced were such that, even from an astronomical point of view, the *Nautical Almanac* easily surpassed the *Jahrbuch*, and from a maritime stand-point it has not yet been equalled.

First of all, it should be stated that the Commissioners laid down as an absolute rule, a rule which has ever since been scrupulously followed, that the *Nautical Almanac* ought to appear four years in advance of the year for which it is calculated. Moreover, the direction of the *Nautical Almanac*, while continuing under the jurisdiction of the Admiralty, was entrusted thenceforth to a single person, the Superintendent of the *Nautical Almanac* Office.

The *Nautical Almanac* has been from that time in reality a special scientific institution, having its offices and its library established in a separate building. The salary of the superintendent was fixed at 500*l*., and the annual parliamentary allowance was made amply sufficient to permit of the employment of calculators numerous enough to insure the greatest possible accuracy in the results. The first superintendent of the *Nautical Almanac* office was Lieut. S. Strafford, well known for the part which he took in the publication of Baily's "Zodiacal Catalogue."

The first volume of the new almanac, the *Nautical Almanac and Astronomical Ephemeris for the Year* 1834, appeared in July 1833. It embodied *all* the reforms which we have already mentioned; it would therefore be useless to indicate its contents in detail. We shall content ourselves with adding, that all the calculations relative to the principal planets were made under the direction of Schumacher of Altona, and those relating to the telescopic planets by the celebrated Encke. Moreover, as it is absolutely essential that a single list of stars of the moon should suffice navigators of all nations, the Royal Society came to an understanding with Encke on this subject. The stars of the moon ("moon's culminating stars"), ceased from that time to figure in the *Jahrbuch*, and the *Nautical Almanac* obtained the monopoly of this useful publication, a monopoly which it has since preserved.

The tables employed were nearly the same in the *Jahrbuch* and the *Nautical Almanac*. For the sun there were the new tables of Carlini, with the corrections of Encke and Bessel; for the moon the tables of Burckhardt; for Mercury, Venus, and Mars, the tables of Von Lindenau, with the corrections of Schumacher; for Jupiter, Saturn, and Uranus, the tables of Bouvart; for the satellites of Jupiter, the new tables of Delambre, with the corrections of Jenkins and Woolhouse.

XI. *The "Connaissance des Temps" since* 1832.

While this great work of reform and renovation was being carried on, the Bureau des Longitudes of France did not remain inactive. "The Bureau have recently appointed a commission, chosen from its own members, to examine some modifications which it would be expedient to introduce into the *Connaissance des Temps*. In considering this object, the commission has not lost sight of the fact that it is dealing with a work specially designed for mariners, with which they have been familiar for many years, and of which it was, above all, desirable that the price should not be increased." *

On the one side maritime necessities required that the *Connaissance des Temps* should, like the *Nautical Almanac*, appear four years in advance; although for long it has been published on an average only a year and a half before its date, being a delay of two years and a half. On the other side, the ephemeris then published by the *Connaissance des Temps* was evidently insufficient for French astronomers; the following modifications were therefore adopted, and were completely embodied in the *Connaissance des Temps* for 1835.

The mean time was the only time used (although astronomers needed the equatorial co-ordinates of the sun for true noon); the co-ordinates expressed in time were given at $0^{s}{\cdot}01$, and those in arc, $0''{\cdot}1$; the latitude and longitude of the moon were given for midnight and noon of each day; to the lunar distance were added those of the planets Venus, Mars, Jupiter, and Saturn; lastly were given for every tenth day, the apparent positions of sixty-four fundamental stars (the *Nautical Almanac* gave 100).

On the other hand, as it came to be seen that the solar tables of Delambre were defective, the Bureau invited Savary to amend them, but all he did was to remedy errors here and there by means of the corrections of Bessel. Moreover, Delambre's tables of satellites having been discontinued till 1840, Damoiseau was ordered to continue them. But all this was so insufficient, that the *Connaissance des Temps* could not be more serviceable to astronomers.

Thus, in 1838, the positions of the planets, which were given until then for every tenth day, were calculated to a minute of time and of arc; every third day for Mercury; every sixth day for Venus and Mars; every seventh day for Jupiter; every tenth day for Saturn; every fifteenth day for Uranus; and the value of the radius vector was added to the other elements. This was very far behind the *Nautical Almanac*, which gave the positions for every day to $0^{s}{\cdot}01$ and $0''{\cdot}1$.

In 1849, the number of fundamental stars whose apparent positions were given was carried to 115, and the apparent position of α Ursæ Minoris, was given for every day in the year. The *Nautical Almanac* had given those of α and δ Ursæ Minoris for 1834.

In 1854, M. Mathieu was specially appointed to the editorship of the *Connaissance des Temps*. Very soon after he entered upon his duties he had to sustain an attack which recalls that already referred to between Young, Baily, and Sir James South. For some years a sharp controversy was maintained between M. Mathieu and M. Leverrier, and at length the *Connaissance des Temps* of 1862 published for the first time the positions of the moon for every hour, with the differences for 10 minutes. "This innovation," the learned editor said, "is valuable to mariners; it simplifies the calculations of interpolation, and now sailors will be able to make use of the ephemeris of the moon with as much ease as that of the sun." The calculations of the moon are, moreover, made according to the tables of M. Hansen,† which the *Nautical Almanac* had employed since 1858. Lastly, in the same year, are given the positions of δ Ursæ Minoris for every day.

In the following year the ephemeris of the planets was improved, and there were given for every day the heliocentric and geocentric positions at mean noon of Mercury, Venus, Mars, Jupiter, and Saturn; for Uranus and Neptune the positions were calculated only for every fourth day. This was in imitation of a modification suggested by Mr. (now Sir G. B.) Airy to the superintendent of the *Nautical Almanac*, and applied by him from 1857 (*Almanac* for 1861). But, since 1839, besides the preceding information, the *Nautical Almanac* gave, first for every day, and from 1861 for every two days, the ephemerides of all the planets for the time of their passing the meridian of Greenwich, information which French astronomers would have been happy to find in the *Connaissance des Temps* for the meridian of Paris. Let us add, moreover, that the positions of Neptune were only given in the *Connaissance des Temps* in 1861 (for 1863), while they were in the *Nautical Almanac* from 1857 (for 1861).

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* Composed of Sir James South, president, F. Baily, reporter, C. Babbage, Capt F. Beaufort, J. F. W. Herschel, J. Pond, Rev. Dr. Robinson, Lieut. S. Strafford, W Struve.

* Advertisement of the *Connaissance des Temps* for 1832

† "Tables of the moon constructed according to the Newtonian law of Universal Gravitation," by P A. Hansen, Director of the Ducal Observatory of Gotha. (Printed at the expense of the British Government, 1857)

It was also in this same year, 1863, that the *Connaissance des Temps* gave for the first time the values of the constants of Bessel, intended to transform into apparent positions the mean positions of the stars given by the catalogues; as well as the elements of the occultations, according to Bessel, in a form which enabled voyagers to calculate, for the very place where they happened to be, the principal circumstances of the phenomena. The *Nautical Almanac* had published all this since 1834.

In 1864, the positions of the sun, which for many years were calculated with the tables of Delambre, reconstructed in part by M. Mathieu, were published according to the tables of M. Leverrier; the same was done for the positions of Mercury, and in the following year for those of Venus and Mars. The *Nautical Almanac* had used the tables of M. Leverrier since 1860 for the sun and Mercury (*Almanac* for 1864), since 1861 for Venus (*Almanac* for 1865), since 1862 for Mars (*Almanac* for 1866). On the other hand, the *Connaissance des Temps* for 1864 appeared in February 1863, and consequently six months after the *Nautical Almanac* for 1866. Finally, the *Connaissance des Temps* for 1864 contained the rectilinear co-ordinates of the sun referred to the plane of the equator; they are found in the *Nautical Almanac* from 1849.

This collection of reforms raised considerably the value of the *Connaissance des Temps*, which, it was unanimously agreed, had fallen very low as compared with foreign ephemerides. The reform accomplished in France in 1864 was analogous to that of the *Jahrbuch* in 1829 and of the *Nautical Almanac* in 1830. But even at the present time the *Connaissance des Temps* does not contain any ephemeris of Ceres, of Pallas, of Juno, nor of Vesta, which has appeared in the *Nautical Almanac* and the *Jahrbuch* since 1830; nor of any of the numerous small planets discovered since 1845, for which the other two works publish a supplement each year. Yet for a long time past the continued observation of these telescopic planets has formed one of the most important occupations of most of the observatories.

In 1870 the direction of the *Connaissance des Temps* passed into the hands of Puiseux, who, however, kept it for only a very short time. His period of office, nevertheless, was marked by an important improvement. He indicated, by a figure in the proper place, the day on which, in consequence of the difference of length between the sidereal day and the mean solar day, each star passed twice across the superior meridian of Paris. This was a sad omission; such an indication is found in the *Nautical Almanac* for 1822.

At present the direction of the *Connaissance des Temps* is entrusted to M. Lœwy; Mr. Hind has been superintendent of the *Nautical Almanac* Office since 1853, and Herr Fœrster succeeded, in 1864, the celebrated Encke in the direction of the *Jahrbuch* of Berlin.

TELEGRAPHING EXTRAORDINARY

AT the Telegraph Office, Washington, on Dec. 11, 1873, an experiment was carried out in the presence of Mr. Creswell, the Postmaster-General of the United States, the practical results of which will be of immense importance as regards the future of telegraphy throughout the world.

On that occasion the president's last annual message of 11,500 words was transmitted from Washington to New York, a distance of 290 miles, over a single wire in 22½ minutes, the speed obtained being over 2,500 letters per minute.

At New York the message was delivered from the automatic instrument printed in bold type in presence of the Postmaster of New York. This achievement in telegraphy is the more remarkable as the principle involved is not new, but was well known in 1848. The experiments made at that date were practically without result. By the new American combination of chemistry and mechanism the speed is apparently almost unlimited, messages at the rate of 1,200 words, or 6,000 letters, a minute being afterwards transmitted with equally satisfactory results.

Hitherto the speed attainable over circuits of similar length in this country by the Wheatstone automatic system, at present in use for the "high speed" service by the Postal Telegraph Department, does not exceed 200 letters a minute.

The new American instrument has a great advantage in the extreme simplicity of its construction, mechanical detail giving place to chemical action. One important result of this experiment is that it demonstrates that hitherto the speed of transmission of electric currents through a metallic conductor has been restricted from mechanical imperfections in the mechanism of the recording or receiving instrument, and that by the substitution of chemical decomposition for mechanical action, an almost unlimited speed of transmission may be obtained. It is to be hoped that this new transmitting and recording instrument may be the agent by which our present tariff of 1*s.* for twenty words, may be reduced to 6*d.*, or less, for a similar message. Scientific progress, practically applied, is an heirloom to a nation.

NOTES

MR. HENRY LONSDALE is preparing a biography of John Dalton, the great chemist, and would be glad of any letters or other aid in his important work.

AT the meeting of the Paris Academy of Sciences held on Monday, January 5, M. Fremy was elected president for the ensuing year, and MM. Chasles and Decaisne were elected to serve on the central committee.

A COMMITTEE, consisting of Lord Cathcart, Mr. C. Whitehead, Mr. Jabez Turner, Mr. Wakefield, Mr. Brandreth Gibbs, Mr. J. Bowen-Jones, Mr. W. Carruthers, F.L.S., and Mr. J. Algernon Clarke, appointed by the Royal Agricultural Society to carry into effect the suggestions of the judges of the potato disease essays held a meeting on Monday at Hanover Square. They will recommend the Council to offer three prizes of 100*l.* each for disease-proof potatoes. Competitors will probably be required to send in one ton of each variety by the middle of February. Each sample will be distributed among growers in many different parts of England, Wales, Scotland, and Ireland; and the produce of potatoes which resist disease during the first year's trial will be tested for two years longer. With a view of encouraging the production of new varieties, handsome prizes are to be offered also for disease-proof sorts raised from potato plums to enter into competition in the spring of 1879. The terms and conditions will be decided upon at the next meeting of the Council.

THE INDIAN MUSEUM, as it now stands, situated on the highest story of the India Office, has been found to be useless for all the purposes for which it was intended. It has therefore been resolved to erect on the plot of vacant ground in Charles-street, directly opposite the India Offices and facing St. James's Park, a new museum and public library. To this building, which will be very handsome and commodious, all the treasures exhibited in the present museum, as well as those now stowed away for want of space, will be removed. The public library and reading-room will form a marked feature in the new building, and will be constructed more especially with a view to the wants of those preparing for competition in the Indian Civil Service Examinations.

A STATEMENT, drawn up by the Council of the Trades Guild of Learning describes at great length the objects which are contemplated by the organisation. It states that its purpose is to provide education for workmen (1) in the sciences underlying their respective industries, and (2) in various branches of higher education; and that it has sprung spontaneously from the workmen of this country, and its responsible direction will devolve in the main on them, with the support of others who can undertake to advise and help in their educational work. It will accept of no aid from the State, but will make use of the National Universities as the best source of general education for the people of the great towns, enabling them to acquire, not only the results of scientific research, but the most thorough and scientific methods of teaching. By means of branches, which it proposes to establish in the large towns, it hopes to supply what is required to render the work already begun by the University of Cambridge continuous and permanent. It will endeavour to form local Boards, consisting mainly of workmen, who will be responsible for the preliminary formation of classes and the collection of the funds necessary in order to obtain University teaching.

DR. SMALLWOOD, one of the oldest meteorological observers in Canada, and Professor of Meteorology in McGill University, died on December 22. He had carried on observations for more than thirty years; in the first instance at St. Martin's, and afterwards, under the auspices of McGill University and the Canadian Government, in Montreal.

THE Geological Parties of the Canadian Geological Survey and the Boundary Commission have now returned from the West, bringing much material of scientific interest. One of the most important practical results appears to be the establishment of the existence of very large and valuable beds of coal and lignite in various parts of the Canadian territory, between Red River and the Rocky Mountains. This must greatly promote the settlement of these territories and the extension of railway communication into them.

THE *XIXe. Siècle* announces that one of the most distinguished officers of the French navy, as well as an eminent explorer, M. F. Garnier, has been assassinated by the Chinese rebels of Tonquin. It would appear that M. Garnier was in the month of November last engaged in an expedition in Tonquin, his object being to enforce the treaties by expelling from the country a Frenchman who had supplied arms to the people of Yun-nan. M. Garnier had captured a town and made prisoners, who are now on their way to France. It is possible that he fell in a subsequent engagement, but the telegram distinctly states that he was assassinated. On this point full details are expected to arrive on the 18th or 20th inst. M. Garnier was only 35 years of age, having been born at St. Etienne on July 25, 1839. Appointed a midshipman in 1860, he was attached in the same year to the staff of Admiral Charner, and in that capacity he made the campaigns of China and Cochin China. Three years later he was appointed Inspector of Native Affairs, and soon afterwards he published a pamphlet in which he propounded an elaborate scheme for an exploring expedition into the interior of Indo-China, with a view to the opening up of commercial communications between Southern China and the French possessions. M. de Chasseloup, at that time Minister of Marine, nominated a scientific commission to carry out this expedition, the importance of which he fully appreciated. On June 5, 1866, an expedition, under the command of Capt. de Lagrée, and comprising among other officers Lieut. Garnier, left Saigon, went up the river Me-Kong, explored Indo-China, and proceeded as far as Yunnan. After the death of his chief, Lieut. Garnier assumed the command of the expedition, which he brought back to Saigon, along the Blue River. This voyage of exploration, one of the most important which has been accomplished in the present century, occupied two years and a few days. The death of this young and intrepid traveller is an irreparable loss for France and for the whole scientific world.

LAST Thursday, Sir Bartle Frere, speaking at Glasgow on Dr. Livingstone, said that he was often asked what benefit and practical result he expects from Dr. Livingstone's labours. "I answer," Sir Bartle Frere said, "that the geographical problems alone which he will have solved must exceed in importance and interest those of any other explorer since the days of Columbus. But apart from all questions of geographical science, I believe that the commercial, political, and moral consequences must prove far more important than anything of the kind which has been effected since the discovery of the New World."

THE members of the Cambridge Natural Science Club concluded their fifth series of meetings on Saturday, December 6. Each member in turn brings some subject of scientific interest before the notice of the club, in the form of a paper or otherwise, and the discussions which follow have been in many cases both lively and prolonged. The following subjects were discussed during the October Term, 1873:—"Mechanics in Nature," by Mr. A. F. Buxton (Trin. Coll.); "Zoological Colonies," by Mr. A. J. Jukes Browne (St. John's Coll.); "The Magnetism of Crystals," by Mr. J. E. H. Gordon (Caius Coll.); "Some Transformations of Energy," by Mr. C. T. Whitmell, B.A. (Trin. Coll.); "The Neocomian Strata," by Mr. J. J. H. Teall, B.A. (St. John's Coll.); "Cone in Cone-structure in the Lower Silurian Rocks," by Mr. R. D. Roberts (Clare Coll.); and a paper on "The Continuity of the Chalk," read by Mr. P. H. Carpenter.

IN the last number of the *Journal* of the Scottish Meteorological Society is a paper by Prof. Mohn on "Certain Effects of Currents on the Temperature of the Sea and Air," of which the following are the results:—1. That the surface of the sea in currents in narrow sounds in summer is colder than in neighbouring places, where there is a wider sheet of water. 2. That an effect of the reverse kind takes place in winter, but in a much smaller degree. 3. That both effects together diminish the yearly range of the temperature of the surface of the sea. 4. That these circumstances influence the temperature of the air in the same direction at such places, and that hereby a part of the anomalous, strongly-marked oceanic character which places in such situations exhibit, is accounted for. Other papers in this number are—"Letter on some Meteorological Questions requiring Investigation," from Mr. Robert Tennent; and a valuable paper on "Atmospheric Ozone and its Sources," by Dr. T. Moffat.

MESSRS. MACMILLAN have issued a cheap "Special Edition for Schools," of Edward Clodd's "Childhood of the World," in the style of the "Science Primers."

THE Meteorological Committee of the Royal Society have considered Mr. Meldrum's "Notes on the form of Cyclones in the Southern Indian Ocean, and on some of the rules given for avoiding their Centres," of so much practical importance, that they have thought it right to print and circulate it in a separate form as a non-official paper.

WE can do little more than name the following books which have been sent us:—Professor Blackie's neat little volume "On

Self-Culture, Intellectual, Moral, and Physical, a Vade-mecum for Young Men and Students" (Edinburgh: Edmonston and Douglas), contains many old and valuable truths forcibly expressed, and is calculated, we believe, to benefit the class for whom it is intended.—"Darwinism and Design; or, Creation by Evolution" (Hodder and Stoughton), is an attempt to show that although "Evolution is true, the Design argument remains unshaken;" that indeed "Evolution is the method of Creation."—"From January to December; a book for Children" (Longmans), is a miscellany consisting of stories, poems, papers on natural history, &c., arranged, for no reason that we can see, under the twelve months of the year. We fear even grown-up children will find the visits to Kew, the Zoological Gardens, and other papers, dull; besides, it is a great blunder to send forth a book like this without a single illustration.—"The Alps of Arabia; Travels in Egypt, Sinai, Arabia, and the Holy Land," by William Charles Vaughan (King and Co.), goes over well-trodden ground, and tells us nothing new; though those who contemplate a similar journey will find the book useful, fresh, and interesting.—"The Expanse of Heaven; a Series of Essays on the Wonders of the Firmament," by R. A. Proctor, B.A. (King and Co.), is sufficiently described by its title.

"GEOLOGICAL Sketch of the Province of Cadiz," by J. McPherson, is an abstract of a similar work written by the author in Spanish, and is printed at Cadiz. It is a valuable study of the subject, and is illustrated by well-executed maps and sections.

WE take the following from *Ocean Highways*:—Captain Heaviside, R.E., of the Great Trigonometrical Survey of India, is completing the work of Captain Basevi by forming a base station for the India pendulum operations, at the Kew Observatory. With this object he is now engaged in swinging Captain Kater's original convertible pendulum; and a re-measurement of its length will be undertaken probably at the Ordnance Survey Office at Southampton, by Colonel Clarke, the highest authority in England, and probably in Europe, as regards the measurement of standards.

THE Dublin College of Physicians, the *Lancet* says, has opened its portals to female aspirants for its degrees, a lady holding the M.D. of Zurich having been exempted from the first half of the examination for the L.K.Q.C.P. The College is also said to be willing to confer its midwifery diploma on all ladies who may, under certain regulations, apply for it.

WE have received an Italian publication by A. Manzone, on the fossils of Monte Titano in the republic of San Marino, their age and mode of origin. It is published at Florence by G. Barbèra.

THE principal papers in the last number of Petermann's *Mittheilungen* are, a Memoir of Colonel Eemel von Sydow a long account, by Prof. Nordenskiold, of the Swedish Expedition to the North-east of Spitzbergen, from April 24 to June 15, of last year, an account of the Exploration of North-west Texas, by an expedition sent out in 1872 by the Texas Land and Copper Association, and a summary of the work of the *Challenger* Expedition in the North Atlantic. The last two are illustrated by well-executed maps.

THE additions to the Zoological Society Gardens during the past week include a Lioness (*Felis leo*) from Kattywar, presented by Mr. J. Humfrey, of the Bombay Staff Corps; three common Marmosets (*Hapale jacchus*) from S.E. Brazil, presented by Le Chevalier d'Albuquerque; a Crested Ground Parrakeet (*Calopsitta novae-hollandiae*) from Australia, presented by Vice-Admiral Wallis Houston; and a Drill (*Cynocephalus leucophaeus*) from W. Africa, purchased.

SCIENTIFIC SERIALS

The Monthly Microscopical Journal, for this month, contains four papers, besides the notes, record of the progress of Microscopical Science and the Proceedings of the Royal Microscopical Society.—Mr. S. J. McIntire, in Notes on so-called *Acarellus*, discusses the point whether the specimens described and named *A. muscae* and *A. pulicis*, by Mr. Tatem, are related to a form known by him as a parasite on *Obisium*, and elsewhere; and whether it is one of the early forms of *Gamasus*, as thought by Mr. Tatem and Dujardin, though not by himself.—A second paper by Mr. W. H. Dallinger and Dr. J. Drysdale, contains further researches into the life-history of the Monads. The importance of prolonged study of the same form is insisted on, and this shows that the method of multiplication is not, as generally supposed, entirely by fission, but sometimes by an absorption into one of two individuals, the resulting mass clearing like an ovum, and giving rise, somewhat as in *Gregarina*, to a multitude of new individuals. Sometimes more than two, as many as four or six, were observed to unite.—Prof. E. Hull describes the microscopic structure of a granitoid quartz-porphyry from Galway, in which "the silica has consolidated into individual sub-crystalline grains before the other minerals, whereas in all true granites the silica has been the last to consolidate. The presence of aqueous (?) vapour during the consolidation of this rock is shown by the existence of numerous fluid cavities, and is another feature in which it resembles true granites."—Mr. G. W. Morehouse's paper on the structure of the scales of *Lepisma saccharina*, is reprinted from the *American Naturalist*.

Poggendorff's Annalen der Physik und Chemie, No. 8, 1873.—In this number, M. Riess criticises four different methods for determining the duration of discharge of a Leyden battery; that of Wheatstone, with rotating mirror; that of Lucas and Cazin, with rotating slitted disc; the electrical thermometer; and the electro-dynamometer. He shows, from experiments with the first two, that the light-duration of the spark consists of two time-parts; the discharge-time of the battery, and the duration of the after-glow of particles of metal present in the spark. These two parts vary, sometimes in the same direction, sometimes in opposite directions. Thus the duration of the discharge and the luminous duration of the discharge-spark stand in no fixed relation to each other.—In a paper on polarisation of electrodes in the voltaic arc, M. Herwig obtains results different from those of Grove on the same subject. His final mode of experiment was with a ball of pure silver as one electrode, and a plate of copper as the other; the ball being moved from point to point over the plate. Only the silver was here pulverised and any repeated action on what of it passed to the plate was prevented. M. Herwig found that the waste of silver was not even remotely equivalent to the quantity of hydrogen developed in the voltameter.—Dr. Rink has a paper on the velocity of sound, in which he raises some objections to the conclusion to which M. Regnault was led by experiments with the gas and water pipes of Paris, viz., that the velocity of sound is dependent on its intensity, and that a weak wave is propagated less quickly than a strong one.—A lengthy article by M. Riecke treats of the magnetisation of soft iron. He gives experimental determinations of the functions of magnetisation for different kinds of iron (by which is meant the induced magnetic moment divided by the magnetising force).—In a note on the relations between capillary and electric phenomena, M. G. Lippmann describes a capillary electrometer and an electro-capillary motor (with illustrations). In the latter, two bundles of fine glass tubes, dipping in separate vessels of mercury, are moved up and down alternately through the changes of form the mercury surface undergoes from polarisation with a galvanic current; and this motion is converted into rotary by a system of levers.—M. Bergh proposes an application of solar heat as a motor force,—vaporising sulphurous acid contained in vessels on the roof of a workshop. He would add to the solar machine, Natterer's apparatus for condensing carbonic acid; the force thus stored up might be used when solar heat was deficient.—M. Leyser describes a new form of Holtz's machine; and among the remaining subjects treated in this number are, heat relations and decompositions in solution of mixed salts in water (Winckelmann), absorption of heat by pulverised carbon (Vierordt), determination of the relation of specific heat to the velocity of cooling of certain gases (Kohlrausch).

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Jan. 8.—"On the Brom-Iodides," by Dr. Maxwell Simpson, F.R.S., Professor of Chemistry, Queen's College, Cork.

"Contributions to the History of the Orcins.—No. IV On the Iodo-derivatives of the Orcins," by Dr. John Stenhouse, F.R.S.

"A Memoir on the Transformation of Elliptic Functions," by Prof. Cayley, F.R.S.

"On Electro-torsion," by George Gore, F.R.S.

This communication contains an account of a new phenomenon, of rods and wires of iron becoming twisted whilst under the influence of electric currents; and a full description of the conditions under which it occurs, the necessary apparatus, and the methods of using it.

The phenomenon of torsion thus produced is not a microscopic one, but may be made to exceed in some cases a twist of a quarter of a circle, the end of a suitable index moving through a space of 80 centimetres (= 31 in.). It is always attended by emission of sound.

The torsions are produced by the combined influence of heliacal and axial electric currents, one current passing through a long copper-wire coil surrounding the bar or wire, and the other, in an axial direction, through the iron itself. The cause of them is the combined influence of magnetism in the ordinary longitudinal direction induced in the bar by the coil-current, and transverse magnetism induced in it by the axial one.

The torsions are remarkably symmetrical, and are as definitely related in direction to electric currents as magnetism itself. The chief law of them is—*A current flowing from a north to a south pole produces left-handed torsion, and a reverse one right-handed torsion, i.e.* in the direction of an ordinary screw. Although each current alone will produce its own magnetic effect, sound, and internal molecular movement, neither alone will twist the bar, unless the bar has been previously magnetised by the other. Successive coil-currents alone in opposite directions will not produce torsion, neither will successive and opposite axial ones.

Signs of electro-torsion were obtained with a bar of nickel, but not with wires of platinum, silver, copper, lead, tin, cadmium, zinc, magnesium, aluminium, brass, or German-silver, nor with a thick rod of zinc, or a cord of gutta-percha.

Zoological Society, Jan. 6.—Dr. A. Gunther, F.R.S., vice-president, in the chair.—Dr. A. Leith Adams exhibited and made remarks on the horns of a feral race of *Capra hircus*, from the Old Head of Kinsale. The horns were very remarkable for their large size and very close resemblance to those of *Capra ægagrus*.—Mr. P. L. Sclater, F.R.S., read a synopsis on the species of the genus *Synallaxis*, of the family *Dendrocolaptidæ*. The specimens of this difficult group in nearly all the principal collections of Europe and America had been examined, and the existence of 58 species ascertained, beside three of which the types were not accessible, and which were considered to be doubtful.—Mr. George Busk, F.R.S., read a paper on a new British Polyzoon, proposed to be called *Hippuria egertoni*, after Sir Philip Egerton, who had discovered it growing upon the carapace of a specimen of *Gonoplax angulatus*, dredged up at Berehaven in the course of last summer.—Mr. Alfred Sanders read a series of notes on the myology of *Phrynosoma coronatum*.—A communication was read from Dr. J. E. Gray, F.R.S., containing a description of the Steppe-cat of Bokhara, which he proposed to designate *Chaus caudatus*.—Sir Victor Brooke, Bart., read a paper on Sclater's Muntjac and other species of the genus *Cervulus*. In pointing out the distinctions which characterise the three existing species, *Cervulus muntjac*, *C. sclateri*, and *C. reevesi*, the author showed *C. sclateri*, the species of most northern range, to be intermediate in specific characters and size between the two others. Sir Victor pointed out an advance in the specialisation of the tarsus of *Cervulus* not hitherto observed. In this genus the navicular, cuboid and second and third cuneiform bones were anchylosed together and formed one single bone, the first cuneiform being represented by a very small and separate bone.—A second paper by Sir Victor Brooke contained the description of a new species of deer from Persia, a pair of horns of which he had received from Major Jones, H.B.M. Consul at Tabreez in Persia, and which he proposed to call *Cervus mesopotamicus*.—Major H. H. Godwin Austin read a paper on some birds obtained by him in 1872-73 along the main water-shed of the Brahmaputra and Irrawaddy Rivers. Of these ten were considered as new to Science, viz.:—*Sitta Nagaensis, Garrulax galbanus, G. albosuperciliaris, Trochalopteron cineraceum, T. virgatum, Actinodura waldeni, Layardia rubiginosa, Prinia rufula, Cisticola munipurensis, Munia subundulata*.—Mr. Garrod made some remarks upon the morbid symptoms presented by the Indian rhinoceros that had lately died in the Society's Gardens, and upon certain points in its anatomy.—Mr. Edwin C. Reed communicated a paper on the Chilian specie of the Coleopterous families *Cicindelidæ* and *Carabidæ*.

Royal Microscopical Society, Jan. 7.—Chas. Brooke, F.R.S., in the chair.—Mr. Chas. Stewart gave an interesting *résumé* of a paper contributed by Dr. H. D. Schmidt of New Orleans on the origin and development of red blood-corpuscles in the human embryo, and illustrated his remarks by black-board diagrams enlarged from a number of most beautifully executed drawings which accompanied the paper.—A discussion followed, in which Dr. Lawson, Dr. Matthews, Mr. Stewart, and the president took part.—A paper was also read by Mr. Alfred Sanders on the Zoosperms of crustacea and other invertebrata.—The secretary read a paper by the Rev. W. H. Dallinger, giving a description of his method of preparing drawings of microscopical objects for class illustration, &c.—Mr. Richards exhibited a new arrangement for a tank microscope for the examination of objects under water to a depth of eight inches; and some beautiful slides of diatoms were shown under the society's instruments sent up by Capt. John Perry of Liverpool, containing the following species, viz.:—*Aulacodiscus formosus, Aulacodiscus margaritaceus*, and *Auliscus racemosus*, all recent.

Society of Biblical Archæology, Jan. 6.—Dr. Birch, F.S.A., president, in the chair. The following papers were read:—"The Sallier Papyrus containing the Wars of Rameses Meriamun with the Khita," translated with Annotations by Prof. Lushington.—This well-known text was supplemented by a fragment from the Raifet Collection; it contains perhaps the most vivid picture of a pre-Homeric battle extant: the king himself, the chief actor, frequently speaking in the first person. The two finest passages, the prayer of Rameses to his father Amun, and the defeat of the Hittites, possessing peculiar beauty, in addition to the interest attaching itself to a people who, about 1,200 B.C. were formidable enemies to the Egyptians themselves.—"On some illustrations of the Book of Daniel from the Assyrian Inscriptions," by H. Fox Talbot, F.R.S.

MANCHESTER

Literary and Philosophical Society, Dec. 16, 1873.—"On the Destruction of Sound by Fog and the Inertness of a Heterogeneous Fluid," by Prof. Osborne Reynolds, M.A. The author commences—That sound does not readily penetrate a fog is a matter of common observation. The bells and horns of [illegible] are not heard so far during a fog as when the air is clear. In a London fog the noise of the wheels is much diminished, so that they seem to be at a distance when they are really close by. On one occasion during the launch of the *Great Eastern* the fog was reported so dense that the workmen could neither see nor [illegible]. It has also been observed that mist in air or steam renders them very dull as regards motion. This is observed particularly in the pipes and passages in a steam engine. Mr. D. K. Clark found in his experiments that it required from 3 to 5 times as much back pressure to expel misty steam from a cylinder as when the steam was dry. The author then proceeds to explain these phenomena and to show that the particles of water do not, as it has sometimes been supposed, break up the waves of sound by small reflections in the same way as they scatter the waves of light, but that the destruction of sound is due, like the dulness of motion, to the fact that when foggy air is accelerated or retarded the drops of water move through the air and expend energy in fluid friction. He points out, as a well-known fact, that when foggy air is at rest under the action of gravity the drops of water are not at rest, but descend through the air with a velocity proportional to the square root of their diameters, and that consequently the energy destroyed in a given time is proportional to the square root of the diameters of the drops. He then shows that exactly the same is the case when the fog is subjected to a uniform acceleration and a somewhat similar effect when the acceleration is irregular or alternating. He says, This then fully explains the dulness with which foggy air acquires motion. In the passages of a steam engine the steam is subjected to continual accelerations and retardations each of which requires more force in the [illegible] described with misty than with dry steam, and at each of which the particles of water moving through the steam destroy energy in creating eddies. Although not so obvious, the same is true in the case of sound. The effect of waves of sound traversing a portion of air is first to accelerate and then to retard it. And if there are any drops of water in the air these will not take up the motion of the air so readily as the air itself. They will allow the air to move backwards and forwards past them, and so cause friction and diminish the effect of the wave as it proceeds, just as a loose cargo will diminish the rolling of a ship. He then proceeds to examine the relation between the size of the drops and their effects, always supposing the same quantity of water to be present. He says—I do not know that it has ever been noticed whether a fine or a coarse mist produces the most effect on sound; it does not appear, however, that rain produces the same effect as fog, and considering rain as a coarse fog we must come to the conclusion that a certain degree of fineness is necessary. If we examine theoretically into the relation between the size of the drops and the effect they produce, always assuming the same quantity of water in the air, we find in the first place that if the air is subjected to a uniform acceleration, which acts for a sufficient time for the drops to acquire their maximum velocity through the air, the effect of the drops in a given time—that is to say, the energy dissipated in a given time—is proportional to the square root of the diameters of the drops. This appears from the action of gravity. As previously stated, the maximum downward motion of the drops, and hence the distance they will have fallen in a given time and the energy destroyed, is proportional to the square root of their diameters. Hence where the acceleration acts continuously for some time, as would be the case in a steam-pipe, the effect will increase with the size of the drops. This effect may be represented by a parabolic curve in which distances measured from the vertex along the axis represent the size of the drops and the corresponding ordinates represent their effect in destroying energy. If on the other hand the acceleration alternates very rapidly then there will not be time for the drop to acquire its maximum velocity, and if the time be very short the drop will practically stand still, in which case the effect of the drops will be proportional to the aggregate surface which they expose. And this will increase as the diameter diminishes, always supposing the same quantity of water to be present. This latter is somewhat the condition when a fog is traversed by waves of sound, so long as the drops are above a certain size; when, however, they are very small, compared with the length of the waves, there will be time for them to acquire their maximum velocity. So that starting from drops the size of rain, their effect will increase as their size diminishes, at first in the direct proportion, then more and more slowly until a certain minuteness is reached, after which, as the drops become still smaller, their effect will begin to diminish, at first slowly, but in an increasing ratio tending towards that of the square root of the diameter of the drops. This effect may be represented by a curve which coincides with the previously described parabola at the vertex, but which turns off towards the axis, which it finally approaches as a straight line. This completes the investigation so far as I have been able to carry it. The complete mathematical solution of the equations of motion does not appear to be possible, as they are of a form that has not as yet been integrated. However, so far it appears to me to afford a complete explanation of the two phenomena, and further to show, a fact not hitherto noticed, that for any note of waves of sound there is a certain size of drop with which a fog will produce the greatest effect.

EDINBURGH

Botanical Society, Jan. 8.—The following communications were read:—Obituary Notice of Dr. J. Lindsay Stewart, by Dr. Cleghorn.—Note on a Station for *Primula veris* in Coldingham Bay, Berwickshire, by Sir Robert Christison, Bart.—Notes of a visit to Messrs. Dickson and Turnbull's Nurseries, Perth, with remarks on arboricultural subjects, by James M'Nab, V.P.—Note on the destruction by frost of seedling ash trees in Mr. Robertson's nursery ground, near Fettes College, in May 1873, by Alexander Buchan, M.A.—Notice of botanical excursions in 1873, by Prof. Balfour.—Notes on some British fungi, by Prof. Dickson and Mr. John Sadler. Specimens were exhibited.

VICTORIA

Microscopical Society, Oct. 30, 1873.—Mr. W. H. Archer, the president, occupied the chair.—Mr. T. S. Ralph addressed the society relative to a fungus affecting the rye-grass, which has been brought before the notice of the society. He regarded its botanical position as uncertain, but was inclined to think it belonged to a lower form of fungus than *Clavaria*. In a specimen which he had prepared, the mycelium or network of the thread of the fungus would be observed penetrating the cells of the rye-grass, thus robbing the cells of the materials intended for the nourishment of the plant. These mycelial threads travelled through the cells, and ultimately coming to the surface of the leaf, produced the peculiarly reddish film which attracted the eye of the observer, besides the withering of the leaf of the plant.—Mr. F. Barnard exhibited some foraminifera, collected from various parts of the colony and in Queensland, some of which were unnamed and new to recent observers.—The President (Mr. Archer) brought forward living specimens of the polyp *Tyrcha viridis*, and of some freshwater polyzoa, the latter being apparently a new species, and allied to Fredericella, of which he had found species in a pool on the banks of the Yarra.

PARIS

Academy of Sciences, Jan. 5—M. Bertrand in the chair. This being the first meeting of the year the members proceeded to elect a vice-president. [See NOTES.] M. de Quatrefages, the retiring president, then read his report, after which the Academy proceeded with its usual business—the following papers were read.—On the conductibility of magnetic tensions, by M. Jamin.—On a new and simple form of the pro-embryo of echinoderms Stelleridæ (*Asteriscus verruculatus*), by M. H. de Lacaze-Duthiers. A mechanical interpretation of the laws of Dulong and Petit, and Woestyn on specific atomic heats, by M. A. Ledieu. This paper contained a number of mathematical data in relation to the recent papers of MM. Lockyer, Dumas, and Berthelot.—Remarks on the relations between specific heats and atomic weights in simple and compound bodies, by M. A. Pisati. The author states that these relations tend to show that there is no distinction really existing between simple and compound bodies, but that on the contrary the so-called elements behave to a certain extent like binary compounds.—On ammoniacal urine, its dangers, and the means of preventing it, by MM. Gosselin and Robin. M. Pasteur observed in connection with this subject, that it would be of great importance to ascertain if this characteristic of urine is not connected with the presence of an organised ferment.—The perpetual secretary read a note from M. Poey on the connection between sun-spots, earthquakes in the Antilles and Mexico, and volcanic eruptions throughout the world.—Researches on the conditions under which a conoid of a given curve exhibits a contact of a determinate order, by M. Painvin.—An answer to M. Faye's remarks on terrestrial waterspouts, by M. Th. Reye. M. Faye made some remarks in reply.—On the variable period in the closing of a voltaic circuit, by M. A. Cazin. This was an answer to M. Blaserna's remarks.—On the conditions necessary for the formation of octahedral borax, by M. de Gernez.—On the geological conditions of the islands adjacent to the African shore from Morocco to Tunis.—On a Marine Carboniferous flora discovered in the neighbourhood of l'Ardoisière in the valley of Lichon (Forez).—On the geographical distribution of the ferns of New Caledonia, by M. Eug. Fournier.—On the pluvial law of the torrid zone, in the basin of the Atlantic Ocean, by M. V. Raulin.

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ERRATUM.—Vol. ix. p. 132, 1st col. line 19, for "Mr. J. D. Painter" read "Mr. J. D. Sainter"

THURSDAY, JANUARY 22, 1874

SCIENCE AND INDUSTRY

IS England rapidly losing that commercial and manufacturing supremacy which she has held before all the world for generations past? Is she going the way of Venice, of Florence, of Holland? If so, is it because she feels blindly secure that "what hath been, will be," neglecting the means on which success in commerce and manufactures in these days depends—means which are being so industriously used by rival nations, that they are rapidly shooting ahead of England on England's own ground?

Such would seem to be the drift of the utterances which have come from three different quarters during the past week. In Lord Derby's address at the inauguration of the Society for the Promotion of Scientific Industry; in the correspondence in the *Times* of the past few days; and in the statements of the Society of Arts' deputation last Saturday to the Lord Chancellor with respect to the Patent Museum, it is more or less distinctly hinted that the industries of England are perishing from lack of knowledge. Other countries, but especially Germany, we are told, are distancing us, and we fear the proofs of the statement are too convincing to be resisted. To all appearance, Germany is destined to step into the honourable position as an industrial nation, which all the world has hitherto acknowledged as belonging to England. In short, in Commerce as in Science we are losing ground.

One correspondent in Monday's *Times* tells us that in the East "the Germans are carrying everything before them;" "by their energy and enterprise they have gone ahead of their easy-going English neighbours . . . whatever be the causes, there can be no question that they are outstripping us in the race for commercial prosperity in the East." This is in confirmation of what a correspondent in a previous number of the *Times* had stated from observation as to the rapid ascendency of the Germans in commerce. Another correspondent, an "Ex-president of the Liverpool Chamber of Commerce," states in Monday's *Times*, without hesitation, that young Germans make the best business men. Dr. Lunge, in his recent address to the Newcastle-on-Tyne Chemical Society (see NATURE, vol. ix. p. 113), states that in the matter of the applications of chemistry, "foreign countries are taking the wind out of our sails very fast in that line, and that both their rate of progress and the means of attaining it are very much superior to ours," because a better career is open to chemists there than with us. Lord Derby says that if we don't take care we shall find ourselves in the position of a man who succeeds to a ready-made business, and who "does not get up as early nor work as hard as his father, who had to make it." Perhaps, had Lord Derby said all that he thought, he would have put the case much stronger against us.

What are the causes which have led to this state of things? How is it especially that Germany is getting so rapidly ahead of us? All who have inquired into the question, attribute it to the difference between the methods of education in England and in Germany, and the greater appreciation of Science in the former country. The mere fact of the establishment of the Society for the Promotion of Scientific Industry, shows that the eminent men who compose it feel that energetic measures should at once be taken to enlighten the multitudes on whom the success of our industries depend. Lord Derby, in his speech at Manchester, said:—

> "If we mean to keep our old position as the industrial leaders of the world we must throw away no chance, and leave no stone unturned. No doubt, in applied science, whatever discoveries are made or inventions brought into use by one country will soon extend to all. Still there is an obvious advantage in getting the lead; and that advantage we ought, if possible, to secure. . . . We are shut up therefore to one or two conclusions—either we must acknowledge ourselves beaten, or we must contrive to make every day's labour of a man more productive than it has been hitherto by the more general, or by the more skilful use of mechanical and chemical science.

He then goes on to state that:—

> "Now it is the belief of the promoters of this new society that a great deal may be done for technical training without interfering with that training of the workshop which is, in one sense, the best of all. They believe, moreover, that there are innumerable investigations of an experimental kind, having for their object the perfecting of industrial processes, which being everybody's business are nobody's business, which would in their results enormously benefit the trade or industry which they concern, but which individuals are slow to undertake, because they do not bring any certain return of profit to the person who spends time and labour upon them."

Hitherto the vast majority of those connected with our industries have done their work by mere rule-of-thumb, without anything like a scientific knowledge of the material or the machinery on which they are employed. This will no longer do; herein lies our weak point; in this direction it is that the Germans are rapidly excelling us. The secret of the growing success of the Germans in commercial and manufacturing industries lies not only in their thoroughly organised and scientific system of education, in their "Realschule" and their technical training-schools, but in the general interest taken in the advancement of knowledge, the development of new methods. In the Realschule the young German gets a thorough liberal and scientific education, not a mere rule-of-thumb technical training. The literary training is at least as good as that which can be obtained at our best public schools, with the advantage of a thorough instruction in the principles and facts of physical science, *without any narrow views as to their future practical application.* "The consequence is," says a German writing to Monday's *Times*,

> "That the 'Realschule' trains thorough gentlemen who in future life are able to make themselves useful as bankers, merchants, and manufacturers. Many of my friends have acquired such positions; several of them are well-known inventors and chiefs of enormous trading and manufacturing concerns. This system of education produces a class of men who take a warm interest in all practical matters, and find as much pleasure and amusement in the invention and rivalry of machinery, or the production and quality of merchandise, as the young men in England find in horses and billiards. Go among a parcel of young Germans of that class, and though you find them ready for all amusements of youth, you will at the same time perceive that they can talk of a great many

useful things in a spirit of enlightenment which has nothing mean in it, but displays a fitness for cosmopolitan life of which we see the practical results. Besides the 'Realschule,' there are throughout Germany a number of 'high schools of commerce,' where young men enter to learn office-work and technicalities."

This German hits the right nail on the head, when he says that—

> "The English Government would do well to establish such schools upon some definite plan as to unity of teaching. Young Englishmen are quite as well disposed as Germans; in many matters their character is even more stable, but you must give them the opportunity of learning what the Germans do. Proprietary schools will never succeed in this; and no breach of the liberty of the subject would be committed if your Government were intelligent and far-seeing enough to recognise the need of such a system of schools, supernatant on the elementary education."

As another *Times* correspondent says, the maintenance of our commercial prosperity is pretty much a schoolmasters' question. No "association for the promotion of scientific industry" will ever be able to remedy our shortcomings in this respect unless there be a career for men of Science, in which case it will be studied, and unless Science be properly taught. Unless this country is to be entirely outstripped by other nations in the very direction in which we have hitherto prided ourselves as being supreme, Government must take the matter up and see that there is put within the reach of all who are in any way to carry on our industries the means of making themselves thoroughly acquainted with the sciences and scientific principles upon which these industries rest. Let us also, like the Germans, have well-organised Realschule and technical training-schools; and for this purpose let Government take the advice of the deputation which waited on the Lord Chancellor last Saturday, and make haste to appoint a responsible Minister of Education, whose duty it will be to see that our educational machinery in all departments, both in extent and in efficiency, is kept up to the wants of the age. The establishment of mere technical schools is not sufficient; these will be of but little avail unless those who wish to take advantage of them have had a previous thorough training in the scientific principles on which the arts are founded. Thanks to Mr. Cole's wise foresight, there are now tens of thousands of our artisans who have had such a training.

No better instance could be afforded of the evil consequences which arise from the want of a responsible Minister of Education, than the disgraceful condition of the Patent Museum. In a dark rusty iron shed at South Kensington are huddled together so as to be practically inaccessible for purposes of study, the paltry collection which represents the genius of that nation which has been foremost in mechanical invention. Let us hope that the object of the Society of Arts' deputation will be granted, and that no time will be lost in arranging in a suitable building everything necessary for the comprehension of Science applied to our various industries, in such a manner that anyone who wishes may study historically all the improvements that have been made in any department; and that, as in the French "Conservatoire des Arts et Métiers," lectureships will be established, thus furnishing a most efficient means for training the men who are to carry on our industries. If this were done, and if local museums were established in suitable centres throughout the country, and if Government take steps to put within the reach of all a thorough general scientific education, and do besides, what no "society for promoting scientific industry" can do, provide means for carrying on unremunerative scientific research, England will soon regain her industrial supremacy, or at least be placed beyond any danger of being outrivalled.

BELT'S "NATURALIST IN NICARAGUA"

The Naturalist in Nicaragua: a Narrative of a Residence at the Gold Mine of Chontales; Journeys in the Savannahs and Forests; with Observations on Animals and Plants in reference to the Theory of Evolution of Living Forms. By Thomas Belt, F.G.S., Author of "Mineral Veins," "The Glacial Period in North America," &c. With Maps and Illustrations. (London: Murray, 1874.)

MR. BELT is a close, an accurate, and an intelligent observer. He possesses the valuable faculty of wonder at whatever is new, or strange, or beautiful in nature; and the equally valuable habit of seeking a reason for all that he sees. Having found or imagined one, he goes on to make fresh observations and seeks out new facts, to see how they accord with his supposed cause of the phenomena. He is a man of wide experience; having travelled much in North and South America and in Australia, as well as in many parts of Europe—and always with his eyes open—before visiting Nicaragua. He is a geologist and an engineer, and knows how to overcome obstacles whether caused by the perversity of man or the forces of nature.

The book we are noticing has, therefore, a value and a charm quite independent of the particular district it describes. As a mere work of travel it is of little interest. The country and the people of Nicaragua are too much like other parts of Spanish tropical America, with their dull, lazy, sensual inhabitants, to possess any novelty. There is little that can be called adventure, and still less of geographical discovery; and the weakest and least interesting parts of the volume are the detailed descriptions of the daily route in the various journeys across the country. We have here and there good illustrations of Spanish American character, as when staying for the night at a ruinous farm-house, the proprietor, Don Filisberto, informed him that he was busy building a new residence. On asking to see it, "He took me outside and showed me four old posts used for tying the cows to, which had evidently been in the ground for many years. 'There,' he said, 'are the corner posts, and I shall roof it with tiles.' He was quite grave, but I could not help smiling at his faith. I have no doubt that, as long as he lives, he will lounge about all day, and in the evening, when his wife and children are milking the cows, will come out, smoke his cigarette, leaning against the door-post of his patched and propped up dwelling, and contemplate the four old posts with a proud feeling of satisfaction that he is building a new house. Such a picture is typical of Nicaragua."

Mr. Belt has done perhaps more than any other traveller to support the theory originated by Mr. Bates of the purpose and cause of what is termed "mimicry" in the animal world, since it was he who first directly observed insectivorous birds reject the Heliconii and allies as food. In Nicaragua he found that a tame monkey, which was extremely fond of insects, and would greedily munch up any beetle or butterfly given to him, would never eat the Heliconii. He would sometimes smell them, but invariably rolled them up in his hand and dropped them quietly after a few moments. One large spider used to drop them out of its web when put into it, but another spider seemed to like them, showing that the smell and taste is not universally, although very generally, displeasing to their enemies. The Lampyridæ, among beetles, which are almost as frequently mimicked as the Heliconidæ, were rejected by monkeys and fowls, as they are known to be rejected by insectivorous birds. Among the new cases of mimicry observed by our author was a longicorn beetle, which most deceptively resembled a hairy caterpillar—a kind which it is well known are never eaten by insectivorous birds. More remarkable is the account of the behaviour of a green leaf-like locust among insect-eating ants. "This insect stood immovably amongst a host of ants, many of which ran over its legs without ever discovering that there was food within their reach. So fixed was its instinctive knowledge that its safety depended on its immovability, that it allowed me to pick it up and replace it among the ants, without making a single effort to escape. This species closely resembles a green leaf, and the other senses, which in the Ecitons appear to be more acute than that of sight, must have been completely deceived. It might easily have escaped from the ants by using its wings, but it would only have fallen into as great a danger, for the numerous birds that accompany the army of ants are always on the look-out for any insect that may fly up, and the heavy locusts, grasshopers, and cockroaches have no chance of escape."

The view that conspicuously coloured creatures, and those that seem to court observation, have some special protection, and that the gay colouring is a warning signal to their enemies not to touch them, was first applied by myself to explain the brilliant colours of many caterpillars. It is now, however, found to have a very wide application, and Mr. Belt is so convinced of its truth that he is able successfully to predict the behaviour of other animals towards an unusually conspicuous species. Most frogs are of more or less protective tints—green or brown according as they live among foliage or on the ground. They feed only at night, and they are all preyed upon by snakes and birds. One species, however, found by Mr. Belt, was of a bright red and blue colour, and hopped about in the day-time without any attempt at concealment. He was at once convinced, theoretically, that this frog must be uneatable. He accordingly took it home, but neither fowls nor ducks would touch it. At length one young duck was induced to pick it up, but instead of swallowing it, instantly threw it out of its mouth, and went about jerking its head as if trying to throw off some unpleasant taste. The skunk, whose offensive secretion is universally dreaded, is a similar instance among mammalia. Its white tail laid back on its black body makes it very conspicuous in the dusk, when it roams about, so that carnivora may not mistake it for other night-roaming animals. When we consider that such cases as these are probably very numerous; that instances of clearly protective colouring are still more so; that both these kinds of colouring may vary almost infinitely, and that there is certainly some unknown influence which tends to produce certain colours in certain localities; and when we further consider that all these causes have been in a continual state of change with changing conditions of existence, organic and inorganic, and have acted and combined with each other in countless ways for untold generations, we have some ground for concluding that colour in nature may have been produced with less assistance from sexual selection than Mr. Darwin thinks is due to that undoubtedly powerful agent.

A very full and interesting account is given of the leaf-cutting ants (Œcodoma sp.), and though these have been so often described, our author has much that is new to tell about them. In his mining operations he cut through some of their subterranean galleries, and from his examination of these he arrives at the conclusion that the ants do not feed on the leaves which they gather in such enormous quantities, but that they use them to form beds for the growth of a minute fungus on which they and their young live. These ants are so destructive to certain plants by entirely destroying their foliage, that many species cannot be cultivated without constant care and protection. It becomes an interesting point, therefore, to determine by what means many of the less vigorous or less abundant species are preserved. It has long been known that there is a very close connection between certain trees and ants. Many Melastomas have a kind of pouch at the base of each leaf, which serves as a habitation for small ants. These have been described by Mr. Spruce, as well as others on the leaves of species of Chrysobalaneæ and Rubiaceæ, &c., in a paper read before the Linnæan Society but not yet published; and he arrived at the conclusion that these structures had become hereditary through the adaptation of the plant to the constant parasitism of the insect, although he did not consider that the ants were of any actual service to the plant. Mr. Belt figures the leaf of a Melastoma possessing these pouches as well as a curious thorny Acacia, the thorns of which are very large and hollow, and are tenanted by ants. In this case the constant attendance of the ants is secured by a provision of food in the shape of little stalked fruit-like glands on the leaves, which the ant feeds on. The hollow stems of the Cecropias are also infested by ants, and they always abound on Passion-flowers, feeding on the honey glands of the flower. Now Mr. Belt believes, and apparently with good reason, that in all these cases the ants are protectors of the plant against herbivorous insects, such as caterpillars, cockroaches, earwigs, &c., but especially against the leaf-cutting ants; and that on account of this service the plants have in many cases become specially modified so as to supply food or shelter to the ants which are so useful to them. It is a suggestive fact that introduced trees and shrubs are more subject to the attacks of the leaf-cutting ants than native species. They do not possess either the disagreeable juices or the insect protectors that the latter have in the course of ages acquired. We have here an altogether new view of the inter-relations of plants and insects, which may, in some cases, help botanists to account for the presence of the many curious and apparently useless glands and appendages plants often possess.

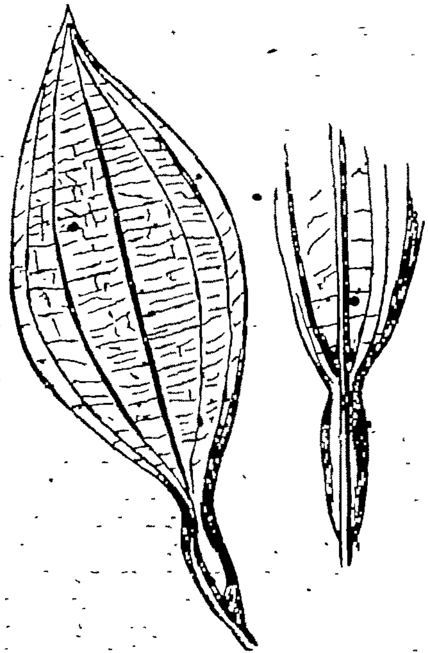

FIG. 1.—Leaf of Melastoma.

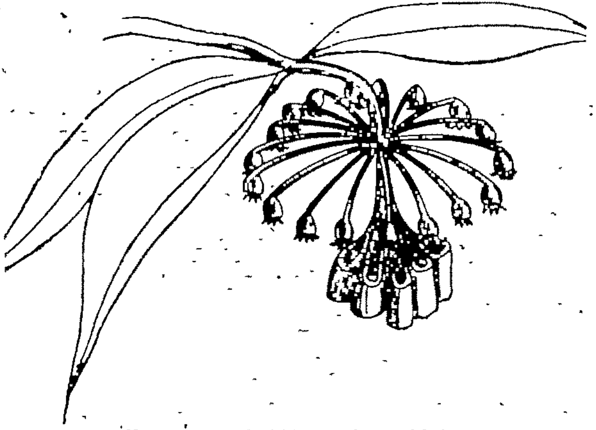

FIG. 2.—Flower of Marcgravia nepenthoides.

Among other natural history information in this work, we find some excellent observations on reasoning power in insects, a good description of the habits of a monkey, and some judicious remarks on the mode of action of natural selection; although the idea that the hairless breed of dogs has been produced because hair favours the increase of *pediculi* and other parasites, is hardly one that will be accepted, seeing that hairless forms, of carnivora at all events, are quite unknown in a state of nature. On the subject of the fertilisation of flowers by insects Mr. Belt remarks, that besides the special adaptations for fertilisation by certain insects, there are often other adaptations for the express purpose of preventing useless insects from robbing the flowers of the attractive nectar, and he illustrates this by a description of our common foxglove. He also furnishes, what I believe are new and very curious cases of fertilisation by birds. In the *Marcgravia nepenthoides* (Fig. 2) there is a group of pitchers below the flowers, containing a sweet liquid which attracts insects; and numerous insectivorous birds come to feed upon these insects, and in doing so necessarily brush off the pollen and convey it to other flowers. In a species of Erythrina having a sword-shaped flower which will only admit very minute insects to the nectary, two species of long-billed humming birds probe the flowers in search after the insects, and in doing so get the pollen on their heads and carry it to other flowers. In this case the nectar is protected by a thick fleshy calyx, which effectually prevents bees and wasps from breaking in and stealing the attractive liquid.

As a geologist our author contributes some important facts on the great question of an intertropical glacial period. He found at from 2,000 to 3,000 feet above the sea, an extensive formation of boulder clay, full of great angular blocks, which he has not the slightest hesitation in pronouncing to be of glacial origin. He decides that this formation must be due to land glaciers and not to icebergs, because the latter would imply a depression of the country fully 3,000 feet, which would have produced a wide channel connecting the Atlantic and Pacific, and have caused more intermingling of the faunas of the two oceans than actually exists. It may, however, be argued, on the other hand, that if there has been no recent communication between the two oceans, then scarcely a single species of fish or mollusc should be common to the two. Yet no less than 48 species of fishes are absolutely identical; and as to the molluscs, Mr. P. P. Carpenter says that, besides those undoubtedly identical (about 40), more than 30 others may be identical, and that 40 more, although distinct, are very close representative species. We have, therefore, over 100 species of molluscs so nearly identical in the two oceans, that we cannot suppose their separation to date longer back than the Pliocene period. It may be fairly argued that this amount of community proves a connection between the oceans at a recent date, and that the number of species in common is quite as great as we can expect, when we consider—firstly, that migration into an already fully stocked area is by no means so easy and rapid a process as was once supposed; and secondly, that the presence of icebergs depositing their loads of clay and gravel in the straits themselves would, perhaps, destroy most forms of marine life, or drive them away to some distance. Mr. Belt further advocates, what seems a very untenable theory, that the glacial period of the northern and southern hemispheres was at its greatest severity at the same time, and that the glacial deposits of Central America and Brazil are synchronous. To get over the enormous difficulty as to what became of the exclusively tropical forms of insect and bird life that abound in such overpowering luxuriance in tropical America, he has recourse to the increased area of low land caused by the lowering of the ocean owing to the vast amount of water abstracted in the form of ice. But Mr. Andrew Murray's map of the 100 fathoms line of soundings shows that the tropical part of South America would not be materially increased in area by a depression of 600 ft., and another 600 ft. would add proportionately less. Besides, if astronomical causes have produced glacial epochs, it is certain that they would occur alternately in each hemisphere;

and this would enable us far better to understand how the tropical forms of life continued to flourish by migrating north or south away from the colder pole. The subject of glacial periods is rendered vastly more difficult by the discovery of signs of glaciation so far within the tropics, and all facts proving such glaciation are of the greatest importance. It seems most probable that the solution of the problem will be only possible by admitting a succession of glacial periods of unequal intensity; so that while in the tropics we have the traces of one of the more ancient and intense period of cold, in the more northern regions we see the results of successive glaciations and intervening denudations.

Much more satisfactory as well as more original, is Mr. Belt's theory of the cause of whirlwinds and cyclones. He well remarks that there is a complete gradation, from the little eddy which whirls up the dry leaves, through the moderate whirlwind, up to the most destructive hurricane; and that a great philosophical mistake has been committed in forming theories to explain the larger phenomena without ever having studied the smaller. The few pages devoted to this subject are well worth reading, and would alone stamp the author as an acute observer in physics as well as in natural history. He gives good reasons why all the received theories of the cause of cyclones are incorrect, and substitutes one founded on observation of the smaller and more easily observed phenomena which is very ingenious, and which appears to have received the provisional approval of the Astronomer Royal, but which would occupy too much space to give an account of here.

We have now sufficiently shown that most of the readers of NATURE will find matter of interest in this volume; and we sincerely trust that the author may soon find himself in a position to work more systematically at some of those branches of science which he has here touched upon. So clear-sighted and intelligent a student will probably make important discoveries.

ALFRED R. WALLACE

PETTIGREW'S ANIMAL LOCOMOTION

Animal Locomotion; or, Walking, Swimming, and Flying. By J. Bell Pettigrew, M.D., F.R.S. (London: Henry S. King and Co., 1873)

PROGRESSION on land, in water, and in air, are phenomena so intimately connected with everyday life, that all of a thoughtful and observant turn of mind cannot help becoming acquainted, unassisted, with most of the details and much of the principle of their production. Many will therefore open a new work on the subject with a wish to have explained to them some of the more difficult and obscure problems connected with it, which are too intricate or uncommon to be within the limits of ordinary powers of observation; and to have the fundamental principles on which the subject is based, fully expounded. With such a feeling we took up the book under consideration, especially as Dr. Pettigrew's name has been always held up as that of the British exponent of the phenomena of flight, and the combatant of the French school. Imagine our disappointment on finding that, instead of the work being by the hand of a master, its author is deficient in the knowledge of the first principles of physics, and of the undoubted meaning of some of the most simple terms employed in the science; his argument, if it may be so called, being but little more than a long series of vague and fanciful analogies, incorrectly stated physical facts, and untenable theories.

In the introduction, and more minutely in a special chapter, the subject of aëronautics is discussed, and the false hope perpetuated that it is quite within the range of human possibility to construct a flying machine, capable of sustained suspension; for we are told that "in order to construct a successful flying machine . . . all that is required is to distinguish the properties, form, extent, and manner of application of the several flying surfaces;" no mention being made of the true difficulty of the problem, which is, that it is at present impossible to obtain from any form of fuel, a sufficient percentage of the potentiality which it possesses for doing work, to work an engine sufficiently compact and light for the wings which it has to drive. In the chapter on progression through the air, one of the paragraphs commences with the astonishing title, "Weight, Momentum, and Power, to a certain extent synonymous in flight," which follows an equally extraordinary and oft-repeated statement that "weight, when acting upon wings, or what is the same thing, upon elastic twisted inclined planes, must be regarded as an independent moving power." After such indications of imperfect knowledge, nothing in the way of mechanical theories could cause surprise, and we are therefore not astonished to find it laid down as the fundamental principle of flight, that the up-stroke of the wing aids in propulsion, and that in the down-stroke the inferior surface of the wing is directed *downwards* and *forwards*. "I repeat downwards and forwards; for a careful examination of the relations of the wing in the dead bird, and a close observation of its action in the living one, supplemented by a large number of experiments with natural and artificial wings, have fully convinced me that the stroke is invariably delivered in this direction," the wings being said to act like a boy's kite during both the down and up stroke. Who can see any close relation between the flight of birds and that of a kite? Dr. Pettigrew seems to forget that a kite needs a string, and yet, backed by his false analogy, he has the presumption to quote the experimental verifications and opinions of such able and ingenious thinkers as Borelli and Marey, the authors of the true theory of flight, only to reject them; bringing forward in opposition such evidence as "from accurate examination, I am fully convinced," and the like, against the sound mathematical arguments and superbly conducted experiments of the two above-named physicists.

Another favourite notion which Dr. Pettigrew reiterates is that "the efficiency of the wings is greatly increased by the fact that when it ascends it draws a current of air up after it, which current, being met by the wing during its descent, greatly augments the power of the down-stroke. In like manner, when the wing descends, it draws a current of air down after it, which, being met by the wing during its ascent, greatly augments the power of the up-stroke. . . . The wing is endowed with this remarkable property, that it creates the currents on which it rises and progresses." This would be all very true, if the problem were as simple as here put; but it is evident that these induced currents are of no real service in flight, because in their production there is as much force lost as there may be gained from their subsequent employment on the reversal of the action of the wing, if the bird's body has not] advanced sufficiently far to be in each stroke beyond the range of their action, which is probably the case.

Physiologists will also be considerably startled by a novel hypothesis of Dr. Pettigrew's, which we cannot give better than in his own words:—"Hitherto, and by common consent, it has been believed that whereas a flexor muscle is situated on one aspect of a limb, and its corresponding extensor on the other aspect, these two muscles must be opposed to and antagonise each other." We are not ashamed to say that such has always been, and still is, our idea, notwithstanding the author's remark that "This belief is founded on an erroneous assumption, viz., that muscles have only the power of shortening, and that when one muscle, say the flexor, shortens, it must drag out and forcibly elongate the corresponding extensor, and the converse. This would be a mere waste of power. Nature never works against herself. There are good grounds for believing . . . that there is no such thing as antagonism in muscular movements. . . . Muscles are, therefore, endowed with a centripetal and centrifugal action."

In conclusion, we must say that we expected better things of Dr. Pettigrew, and regret that he has not, before now, learned that there are errors in his methods and his results that cannot be tolerated by a thinking public, which prefers accurate reasoning rather than dogmatic statement, and well-grounded fact to fanciful analogy.

A. H. GARROD

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

The Famine in India and Meteorology

OUT here in India our attention has been of late called to consider the best means of warding off the effects of one of Nature's laws, that threatens the lives of numbers of our fellow-creatures, for at this present moment the millions of Bengal are threatened with famine.

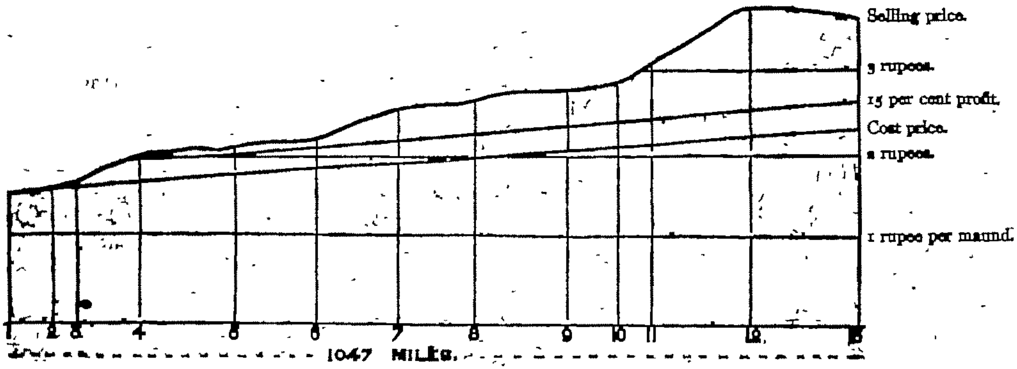

FIG. 1 shows the cost of Wheat per maund at the following Stations on the line of Railway in November 1873:—1, Umritsur; 2, Jullundur; 3, Loodianah; 4, Umballa; 5, Meerut; 6, Allyghur; 7, Etawah; 8, Cawnpore; 9, Allahabad; 10, Mirzapore; 11, Benares; 12, Dinapore, 13, Bhaugulpore.

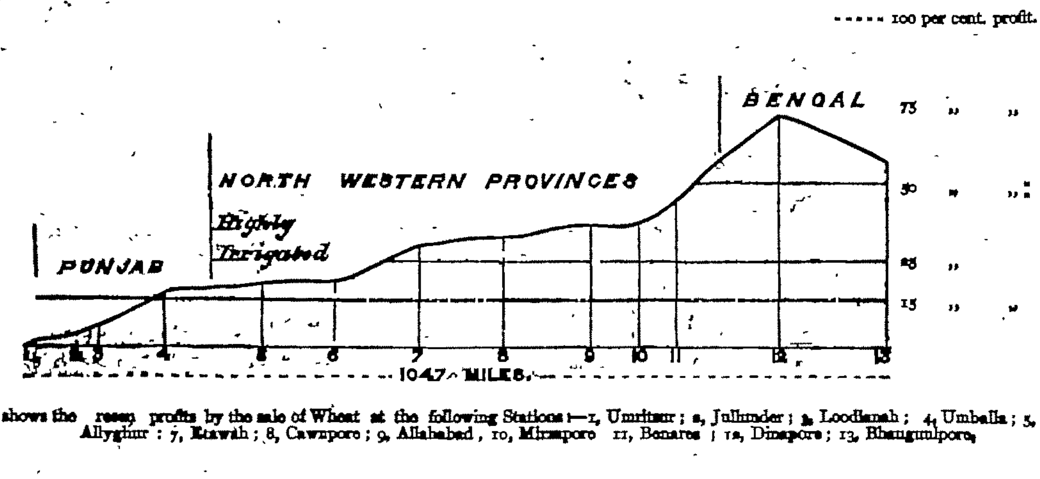

FIG. 2 shows the resp profits by the sale of Wheat at the following Stations:—1, Umritsur; 2, Jullunder; 3, Loodianah; 4, Umballa; 5, Meerut; 6, Allyghur: 7, Etawah; 8, Cawnpore; 9, Allahabad, 10, Mirzapore 11, Benares; 12, Dinapore; 13, Bhaugulpore.

The study of the meteorological effects and changes of climate in India is very interesting, but I have not time to go into the question how one portion of the air laden with moisture moves up the Bay of Bengal, sweeps along the coast of Burmah and Assam; how it begins to part with the moisture in Burmah about the middle of May, continually advancing along the valley of the Ganges, till about a month later it reaches Simla, which is on the ridge which separates the drainage of the Ganges from that of the Indus.

Up the Indus valley another current of air from the Indian Ocean also moves northwards parting with large quantities of its moisture along the western coasts of Madras and Bombay, and then passes up the valley of the Indus, but without watering that large extent of desert lying between Scind and the Punjab, so it is not till within a short distance of the hills that the body of air begins to part with its moisture.

I say this is all a very interesting subject to study, but it is not my intention at present to go into it, but simply to state that this season Bengal has not had its average rainfall, while in the Punjab the rains were later than usual in setting in, yet the general fall has been on the whole seasonable, for though the cotton crop is a failure compared with other years, yet the cereals have been plentiful, and hence grain is cheap.

In Fig. 1 I have tried to show the price of grain along the line of the railway from Umritsur to Bhaugulpore, a distance of 1,047 miles. The full black line shows the actual prices at which grain is now selling in all the several districts through which the railway passes, it being least at Umritsur, which is in the centre of the Punjab, and greatest at Dinapore, in the Patna district of Bengal. At the right-hand side is a scale of rupees showing the cost per maund, which can be easily reduced to English values by considering a maund equal to ¾ cwt. and a rupee equal to 2*s.* This will approximately give the present English value of wheat during November last as published in the several gazettes. The lower dotted line shows how the price of this grain goes on increasing by the distance transported. The usual railway rate was ½ pice per maund per mile, which by a late order of Government has been reduced to half this rate, or approximately 3*s.* 6*d.* for 100 tons a mile.

If to this be added 15 per cent. profit to meet losses and deterioration, the thick dotted line indicates what grain could probably be sold at by Government without any loss, if it became a large dealer; and, as before said, the upper full line shows the actual prices with the large margin there is for profits.

Fig. 2, however, shows this much clearer, and proves that the demand must be greater than the supply; or, in other words, that as much as over some 2,000 tons daily of grain, which was grown more than 700 miles away from the point where the famine is most severe, along the line of railway, is sent down from the Punjab, and the highly irrigated lands of the North-West Provinces, enough to sustain in life as many as there are inhabitants in London, or some four million souls; for 1¼ lbs. for each man, woman, and child, is considered enough to sustain the life of a native of India.

But what is this to the millions of Bengal that are now threatened with famine? It is hardly one-seventh, I am led to understand. So with all our canals and railways, and the great good they are doing in the present state of things, yet there is a larger demand than can be supplied, or the profits could never mount up to 70 per cent., as at Dinapore.

Though the subject of this letter may not be considered exactly a fit one for the pages of NATURE, yet I feel sure that those who study Nature in her works and effects, will be interested in the facts now given.

T. LOGIN
Sup. Engineer, Punjab

Umballa, Dec. 12, 1873

Dr. Tyndall and Sensitive Flames

IN the last number of NATURE a report is given of the first of Dr. Tyndall's Christmas "Lectures to Juveniles," on the Motion and Sensation of Sound. In that lecture Dr. Tyndall shows how the reflection of sound can be made manifest to an audience by means of a sensitive flame; and, according to the closing words of your report, Dr. Tyndall states,—"Never before have these phenomena been made visible. Hitherto these effects have been investigated by the sense of hearing; I have now been able to prove them by appealing to your eyes."

In the *Illustrated London News* a short notice is also given of the same lecture, and there Dr. Tyndall is reported to have said, that no philosopher had ever before witnessed the reflection of sound until that afternoon. I presume, therefore, that the report you have given accurately represents Dr. Tyndall's words. And this being so, will you permit me simply to place the following facts before your readers. In January 1870 I published an article in the *Quarterly Journal of Science* on the "Analogy of Light and Sound." In that article I stated how a sensitive flame can be used as a delicate *phonoscope*, to reveal perfectly well the decay, the absorption, and the reflection, and (less perfectly) the refraction of sound-bearing waves. A sketch is there given precisely the same as that which appears in Dr. Tyndall's lecture (Fig. 6), wherein a sensitive flame is placed in the conjugate focus of a pair of parabolic mirrors. This experiment was shown at a lecture I delivered on January 3, 1868, before the Dublin Royal Society. A copy of my paper in the *Quarterly Journal of Science*, and of the printed abstract of my lecture before the Dublin Society, I myself sent to Dr. Tyndall a few days after they appeared, and if I mistake not, drew his attention to these experiments.

Since 1868 I have so frequently shown to my own class and to large audiences the reflection of sound by a sensitive flame, that I have no doubt many of your readers will have been astonished when they heard or read Dr. Tyndall's assertion which I have quoted. Indeed, probably Dr. Tyndall himself will be able to recall the foregoing facts, and will gladly put his memory right on this matter.

W. F. BARRETT

Royal College of Science, Dublin, Jan. 19

The Potato Disease

SINCE October 1872 I have been growing potatoes, healthy and diseased, under test conditions, principally with a view to a further insight into the winter and subterranean life of the *Peronospora* and also in the hope of meeting with the (to me) apocryphal *Artotrogus*. The figures of the latter referred to by Mr. Berkeley, I am well acquainted with, as I have engraved them three times, once to illustrate Mr. Berkeley's own paper in the *Gardener's Chronicle*. I therefore well knew what to look for in the corroded cellular tissue of my diseased potatoes. I by no means wish to assert (or indeed asserted) that *Volutella ciliata* is positively the same with Montagne's *Artotrogus*, for I have never seen a specimen of the latter, (I know no one who has except Mr. Berkeley), and as far as I am aware no one has met with it since the time of its original publication between twenty and thirty years ago. As no one now (including Mr. Berkeley) ventures to suggest more than the "possible" or "probable" nature of *Artotrogus*, my note was meant to suggest another reasonable direction for future observation.

In my experiments, I have from the first been forcibly struck with the presence of *Volutella* with its mycelial threads, not only outside and just within spent potatoes, but also within the corroded cellular tissue. I have no doubt that the plasma of *Volutella* is equally disorganising with the plasma of *Peronospora* itself, and that the threads belonged to the former plant I have no manner of doubt, as I constantly traced young to mature specimens of *Volutella* from it, and that too from positions within *buried* potatoes. The strong external resemblance between some states of *Volutella* and the figures referred to by Mr. Berkeley, suggested to me that this "will o' the wisp" *Artotrogus*, might perchance eventually turn out to be no other than some condition of *Volutella*.

So far from its being my desire to draw attention from *Artotrogus*, the paragraph in my first letter was written with a view to draw attention to it. Berkeley himself always speaks doubtfully of its nature, and Carruthers, in his recent paper on *Peronospora*, published by the Royal Agricultural Society, has not even referred to it.

Returning for a moment to the principal subject of my first note, viz. the failure of the essays submitted in answer to the offer of a prize on the part of the Royal Agricultural Society for the best essay on the potato disease and its extirpation. In NATURE, vol. ix. p. 212, I observe that the committee are now disposed to view the desired destruction of the potato disease from a different standpoint, and propose to offer three prizes of 100*l.* to dealers, who are to send in a ton each of "disease-proof" potatoes.

It appears to me as unreasonable to advertise for a "disease-proof" potato as for a "death-proof" man. Surely all organised bodies are liable to deviation from health, and though certain organisms may be made (by art) to more or less throw off or resist the attacks of disease, yet none can be said to be in themselves "disease-proof." As regards potatoes, I think I may say, without fear of contradiction, that at present no varieties whatever are either proof against the *Peronospora* or able to resist its attacks, neither is it at all likely that any such varieties will ever arise.

WORTHINGTON G. SMITH

ON DIFFRACTION SPECTRUM PHOTOGRAPHY, AND THE DETERMINATION OF THE WAVE-LENGTHS OF THE ULTRA-VIOLET RAYS[*]

ILLUSTRATED BY AN ALBERT-TYPE PLATE.

THERE are, as is well known, two methods by which spectra may be obtained: (1) by the action of a prism; (2) by a system of closely-ruled lines. In the latter case it is convenient to speak of the contrivance employed as a grating, and of the spectrum as an interference or diffraction spectrum. A casual inspection shows that there is a great difference between the spectra produced by these two methods, and close investigation proves that the diffraction spectrum is by far the more suitable for accurate scientific work. For this reason it has seemed desirable to make a trustworthy map of those parts of the solar diffraction spectrum which can be photographed on collodion, and to attach to it a scale for reading the wave-lengths of the rays.

The plate accompanying this memoir is from collodion photographs made by myself, transferred to a thick piece of glass, the latter process being known as the Albert-type. For the entire success of this transfer I am indebted to my friend Mr. E. Bierstadt, the owner of the patent in America. The glass is then used in a printing press in the same manner as a lithographic stone. The spectrum is absolutely unretouched. It represents therefore the work of the sun itself, and is not a drawing either made or corrected by hand.

The picture consists of two portions: first, the upper, which gives all the lines of the spectrum from near G to O, or from wave-length 4350 ten-millionths of a millimetre to 3440. Above that is placed a scale, which is a copy of Angström's from just below G to H_2, with the same-sized divisions carried out from H_2 to O. The second, or lower, is a magnified portion of the same negative, having H_1 and H_2 about its middle, and extending from wave-length 4205 to 3736.

It follows therefore that the lines in the solar spectrum are correctly represented in their relative positions. The only errors are those which may have arisen from maladjustment of the scale. The precautions that were taken to avoid such errors will be described. With a certain correction, to be mentioned hereafter, it may also be stated that the relative shadings and intensities are preserved.

The value of such a map depends on the fact that it not only represents parts of the spectrum which are with difficulty perceived by the eye (though they may be seen by the method of Stokes and Sekulic), but also that even in the visible regions there is obtained a far more correct delineation in those portions which can be photographed. In the finest maps drawn by hand, such as those in the celebrated "Spectre Normal du Soleil" of Angström, the relative intensity and shading of the lines can be but partially represented by the artist, and a most laborious and painstaking series of observations and calculations on the part of the physicist is necessary to secure approximately correct positions of the multitude of Fraunhofer lines. Between wave-lengths 3925 and 4205, Angström shows 118 lines, while my original negative has at least 293.

For such reasons many attempts have been made to procure good photographs of the diffraction spectrum. The earliest were by my father, J. W. Draper; his results were printed in 1843-44 in a work entitled "On the Forces which produce the Organisation of Plants." This memoir was accompanied by plates drawn from his daguerreotypes, and the wave-lengths, which he first suggested as the proper indices for designating the Fraunhofer lines, were used as a scale.

Since that time the most important experiments in this direction have been by Mascart and Cornu. These eminent physicists have, however, resorted to the plan of taking portions of the spectrum on a small scale and subsequently making enlarged drawings therefrom. This course introduces the defects of handwork, and the artistic difficulties of copying intensity and shading, as well as the omission of fine lines.

In the photographs of the spectrum which I have taken I have tried to get as large a portion as I could at once, and on as large a scale as possible. I have usually obtained images from below G (wave-length 4307) to above O (wave-length 3440) of about 12 inches (·305 metre) long. I have succeeded, however, in photographing from near *b* (wave-length 5167) to T (wave-length 3032) by resorting to a ruled speculum plane and a concave speculum mirror, but the photographic and optical difficulties in securing an enlarged spectrum of that length are great.[*]

Of course, in such a research as this an essential is a finely and evenly ruled plane of glass or other material. Those which I have used were made by a machine devised and constructed by Mr. L. M. Rutherfurd, whose beautiful lunar and prismatic spectrum photographs are so well known to the scientific world. The plate generally employed is of glass ruled with 6481 lines to the inch; the ruled part is $1\frac{6}{100}$ inch (·027 metre) long, and $\frac{64}{100}$ inch (·016 metre) wide. It is unquestionably much more nearly perfect than similar gratings made by Nobert and others, for the character of the photographs and the uniformity of the orders on either side of the normal, together with its behaviour under a searching examination, show that it leaves little to be desired. As it is on glass, and gives a bright transmitted spectrum, I have constructed the remainder of the optical apparatus of glass achromatised, according to the plan used by J. W. Draper in 1843, except that I have not silvered the ruling, and therefore have used the refracted, and not the reflected beam. The slit is $\frac{8}{10}$ of an inch (0·2 metre) long, and $\frac{1}{110}$ of an inch (·00023 metre) wide; the jaws are of steel, and there is not only a micrometer screw for separating them, but also one for setting them at an angle. Occasionally I have taken photographs with the jaws $\frac{1}{90}$ of an inch (·00028 metre) apart at the top, and $\frac{1}{130}$ (·00019 metre) at the bottom, so as to obtain different intensity in the two edges of the spectrum.

Most of the photographs have been of the spectrum of the third order, which has certain conspicuous advantages. In the first place it is dilated to such an extent as to give a long image, and yet one not too faint to be copied by a reasonable exposure of the sensitive plate; and in the second place, the spectrum of the second order overlaps it in such a way that D falls nearly upon H, and *b* upon O. These coincidences are serviceable in determining the true wave-lengths of all the rays.

The only point of special interest in connection with the photographic part of the operation, is the device for avoiding the unequal action on the sensitive plate of different rays of the spectrum. It has been commonly supposed, until the recent memoirs of J. W. Draper, that there are in the spectrum three different types of force in three different but overlapping regions. Heat was supposed to be principally found at the less refrangible end, light in the middle, and actinism at the more refrangible. But he showed that this error has partly arisen from using prismatic spectra, which condense the red end and dilate the violet, and do not present the rays in the true order of their wave-lengths, and partly from the nature of our ordinary photographic substances. He proved that actinism, or the power of chemical decomposition, does not particularly belong to the violet end of the spectrum, but is found throughout its whole length. But bromide and iodide of silver, as used in collodion photography, are more readily decomposed by vibrations of certain lengths and periods than by others, and hence the excess of action seen at the violet end is a function of certain silver compounds, and not of the spectrum. Other substances, as carbonic acid, show maxima elsewhere, as in the yellow region. The solar beam is therefore not compounded of three forces, light, heat and actinism, but it is a series of ethereal vibrations, which give rise to one or other of these manifestations of force, depending on the surface upon which it falls.

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[*] From the *American Journal of Science and Art*, Dec. 1873. Communicated by the author.

[*] Since writing the above I have succeeded in photographing the lines of the visible spectrum from *b* downward, and the picture comprises not only the regions including E, D, C, B, *a* and A, but also the ultra-red rays. The great groups, α, β, γ, below A, discovered by my father in 1843, are distinctly reproduced.

In order to provide against this excess of action in certain parts of the spectrum, I introduced a system of diaphragms placed in the vicinity of the sensitive plate, and removed at suitable times during the exposure. The region from wave-length 4000 to 4350 only requires about one-tenth of the time demanded by that from 3440 in 3510. In the negative which produced the accompanying plate, the line O had 15 minutes and G $2\frac{1}{2}$ minutes, and the former is under-exposed. These exposures seem at first sight unusually long for a wet collodion surface, but it must be remembered that the slit used was only $\frac{1}{110}$ of an inch wide, and that the diffraction grating gives an almost complete circle of spectra round itself, amongst which this thin band of light is divided. A beam $\frac{1}{110}$ of an inch (·00023 metre) wide is spread out in this case into a streak about 78 ft. (23·77 metres) long.

After the production of spectra that were in focus from end to end, it was next necessary to attach a scale to them by which the wave-lengths might be read. At first I tried, by reducing Angström's maps to the proper dimensions, to accomplish this object, but the undertaking proved to be difficult, and was unsuccessful, because, though the original drawing on the stone was undoubtedly correct, the paper proof of it which I had, had stretched unequally in printing, and on applying a photographic reduction to my spectra, coincidence could not be obtained. As, however, the subject of dividing a scale for these diffraction spectra is of prime importance in giving value and precision to the wave-lengths presented in this memoir, I propose to describe fully the method eventually employed in fitting a scale to the photograph.

The wave-lengths of the ultra-violet rays have never as far as I know, been either determined or published except by J. W. Draper in 1844, Mascart in 1866, and Cornu in 1872. J. W. Draper's memoir has a steel engraving of some of the principal lines, from which the wave-lengths may be approximately read.

The large plate which accompanies Mascart's long and valuable memoir is of the prismatic spectrum, but he furnishes in addition the following table of wave-lengths:—

L	3819·0
M	3728·8
N	3580·2
O	3440·1
P	3360·2
Q	3285·6
R	3177·5

These numbers do not entirely coincide in all cases with my photograph, as I will show farther on.

The detailed results of M. Cornu have not appeared in any publication that has reached me.

I have used as a basis the numbers given by Angström for the rays D_3, b_4 and G, and if there should be any small error in his determination, my scale will require a proportionate correction, which can easily be effected. At first sight it seemed better to take G and H as fixed points, but the line H is so broad, and has so many component lines, that its position is uncertain, and moreover, being almost at the limit of visibility in Angstrom's apparatus, it was more open to errors of measurement. These reasons led me to take advantage of the fact that the second spectrum overlaps the third, the ray D of the second being near H of the third, and b of the second near O of the third. It is obvious that we have thus the means of ascertaining the wave-lengths of three points, one at each end, and one in the middle of my photograph. As the rays D and b cannot impress themselves on collodion by any length of exposure that it is convenient to give, and as in my method of working the ultra-violet rays could not be seen simultaneously with them, it was necessary to resort to the following device:—I placed in front of the sensitive plate and close to it two fine steel points, one of which was carefully adjusted to the position of D_3 of the second order, and the other to b_4 of the second order. When, therefore, after a suitable exposure to the ultra-violet spectrum of the third order, the collodion picture was developed, there were two sharply-defined images of the steel points superposed on the spectrum. The point which had been coincident with D_3 of the second order was then found to have cast its shadow on H_1 of the third order, and the point at b_4 of the second order had impressed itself near O of the third order.

By a simple calculation it was thus rendered evident that a given ray in the compound line H_1 was of the wave-length 3930·1 ten millionths of a millimetre, and that another line near O had the wave-length 3444·6. By looking at the photograph, the reader will see that 3930 falls upon a fine division in H_1, which is beautifully shown in both the spectrum with the scale and the enlarged proof below. Of course, the ray G of the third order, the wave-length of which is known, had impressed itself photographically on the collodion.

Having thus ascertained the wave-lengths of three fixed points in the photograph, the next step was to apply a scale reading to a single ten-millionth of a millimetre, and, if possible, fractions thereof. After many abortive attempts to use that part of Angström's map which lies between G and H, and to attach thereto an additional length of scale sufficient to extend to the end of the ultra-violet region, I was compelled to resort to a linear dividing engine, and rule a scale which was about twice the length of the photographic reduction shown in the accompanying plate. Of course this necessitated drawing in by hand the same systems of lines and lettering as are shown on Angström's chart, and this I did as carefully and faithfully as I could.

It only remained to reduce this divided scale to the proper size to fit the spectrum photograph; after many trials it was accomplished.

It is proper in this place to make a criticism on my scale, and to point out a small error, which may be due, however, to an incorrect determination of the wave-lengths that I have used as fixed points. Taking the distance from G (wave-length 4307) in the photograph to the fixed line 3930 in H_1, and dividing it into 377 parts, and then prolonging these divisions toward O, it was found that the third fixed point was not attained, but that there was an error of about two divisions. But if the position of D_3 in Angstrom's determinations should be incorrect to the extent of one ten-millionth of a millimetre, or if this small error should be partly attributed to D_3, and partly to G, my scale would be correct. Future measures of the wave lengths of these rays, and of b_4, can alone settle this delicate point, for the determinations of Mascart and Angström and Thalen differ nearly to the extent mentioned above. The same remark is true of Angström compared with Ditscheiner, while the difference between Angstrom and van der Willigen is more than three times the amount necessary to remove my discrepancy. In any case the photograph is correct, as it is the work of the sun, and is only open to errors arising from imperfect flatness in the field of a fine lens, and that field only subtending an angle of about 4°. The angular aperture of the lens, viewed from the sensitive plate, is 20 minutes. I trust, therefore, that the photograph may be of permanent value to physicists, for any one can affix another scale if this be slightly erroneous.

An examination of the photographed spectrum shows many points of interest, some of which are best seen in the spectrum with the scale above, and some in the portion enlarged below. The latter is magnified about twice, and comprises the region from wave-length 3736 to 4205. I have also made photographs on the same scale as Angström's map, but have not as yet printed them. The capital letters which are attached to the region above H are according to the nomenclature of Mascart, although the wave-lengths assigned by him to those letters do not coincide exactly in all cases with the lines in my photograph; for instance, the line L, which he regards as single, is in reality triple, and does not correspond to 3819, but to 3821; M is correctly designated by 3728, but it is double; N is really at 3583, and not 3580. It has been suggested that it would be proper to return to the old nomenclature of Becquerel and J. W. Draper, who simultaneously discovered these lines in 1842-43, but the designation of position by wave-length in reality renders the letters unnecessary.

The spectrum above H, when compared with the region from G to H, is marked by the presence of bolder groups of lines, and most conspicuous are those between 3820-3860, 3705-3760, 3620-3650, 3568-3590, 3490-3530. The first of these groups is strikingly shown in the enlarged photograph. I am not as yet able to offer an opinion as to the chemical elements producing these groups, for almost all the photographs of the ultra-violet spectra of metalline vapours that I have thus far made were produced by a quartz train, and have not yet been reduced to wave-lengths. Indeed, that is a separate field of inquiry, and could not be comprised in a memoir of this length. I have also tried to utilise the photographic spectra of the late Prof. W. A. Miller, published in the "Transactions" of the Royal Society for 1862, but for some reason, probably insufficient intensity of the condensed induction spark, his pictures do not bring out the peculiarities of the various metals in the striking manner that is both necessary and attainable. The diffraction spectra of metalline vapours that I have made are not yet ready for use.

The probabilities are that each of these groups will be found to be due to several elements, as is plainly seen in the group H. This compound line, which is commonly spoken of as being caused by calcium, iron, and aluminum, is in reality much more complex, for there can readily be counted in it more than fifty lines in the original negative, and a careful inspection of the accompanying paper picture shows a large proportion of them. This observation leads us to a more general statement. *The exact composition of even a part of the spectrum of a metal will not be known until we have obtained photographs of it on a large scale.* The coincidences which were so thoroughly examined by Mr. Huggins (Trans. Royal Society, Dec. 1863) will only disappear when we can, in addition to the position of a line, have a clear idea of its size, strength, and degree of sharpness or nebulosity. The eye is not able to see all the fine lines, or even if it does, the observer cannot map them with precision, nor in their relative strength and breadth. For example, in Angström's justly celebrated chart, of which the G-H portion is copied in this plate, and in the construction of which the greatest pains were taken by him, many regions are defective to a certain extent. The region from 4101 to 4118 is without lines, yet the photograph shows in the enlarged copy seventeen that can easily be counted, and the original negative shows more yet. The reader of course understands that a paper print of a collodion picture is never as good as the original; the coarseness of grain in the paper, want of contact in transferring, &c., effect such a result. Moreover, the Albert-type process depends on a certain fine granulation which is given to the bichromated gelatine, and this forbids the use of a magnifier upon these paper proofs. It is only just, however, to Mr. Bierstadt to state, that without his personal supervision, such sharp and fine-grained proofs could not have been obtained, and that no other printing-press process that I know of could have accomplished this work at all.* As an illustration of the difficulty of depicting the relative intensity of lines, we may examine 3998, which in Angström's chart is shown of equal intensity with 4004, while in reality it is much fainter, and instead of being single, is triple, as is well seen in the enlarged spectrum.

When, however, we compare Angström's chart with the photograph, it requires, as the above remarks show, a critical examination to detect defects, and we have a striking confirmation of the surprising accuracy of the Swedish philosopher.

So also in comparing Mascart's excellent map of the prismatic spectrum with the photograph, the difficulty of depicting all the fine lines is seen. In the group L he shows twelve lines, while even in the Albert-type copy of my photograph twenty-five can be counted, and in the original negative many more. From H to L he exhibits seventy lines; in my plate 138 can be observed, besides many unresolved bands.

In the earlier part of this memoir it was stated that the relative intensities of the lines in the spectrum were correctly represented if a certain allowance was made. If an unshielded collodion plate were presented to the image of the spectrum, there would be produced a stain very dense from G to H, fainter above H, and still fainter below G. But this stain would not represent the actinic force of the sun; it would merely be the index of the decomposability of a mixture of iodide and bromide of silver. I have for this reason adopted the idea of J. W. Draper, that force is equally distributed through the spectrum, and have tried to produce a photograph of equal intensity throughout. This has been accomplished, as I have before stated, by suitable diaphragms. But whether this view be correct or not, lines which are not far distant from one another are presented virtually without any interference by diaphragms, and must therefore be correct both as to shading and intensity.

Besides the points above mentioned, there are many theoretical considerations suggested by the photograph which it does not seem expedient to enter upon fully at present. Among such is the possibility of arriving at an estimate of the sun's temperature, by interpreting the apparent bands, such as those near G and H, by the aid of Lockyer's researches on the temperature of dissociation of compounds. No one has yet ascertained whether there are or are not unresolvable bands in the solar spectrum. If they do exist, the compounds to which they belong, and the necessary temperature for dissociation, remain to be determined.

It would seem also to be possible to find out whether, as asserted by Zöllner, there is a liquid envelope around the sun, by a search for more diffused bands in its photographed spectrum.

In the hope that this photograph may prove to be of value to scientific men for further investigations upon the sun and the elements, I have caused a number of extra copies to be printed, and shall be glad to present them to anyone who can make use of them.

HENRY DRAPER

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* From the original negative of the spectrum 12,000 copies have up to the present been printed, and it is not in the slightest degree injured as yet.

THE "BRONTOTHERIDÆ," A NEW FAMILY OF FOSSIL MAMMALS

VERY nearly a year ago Prof. O. C. Marsh, of Yale College, announced the discovery of a new order of Mammalia, the Dinocerata, huge elephantine forms, with three pairs of horns and large canine teeth, from the Eocene deposits of the country to the east of the Rocky Mountains, including the states of Dakota, Nebraska, Wyoming and the "Bad Lands" of Colorado, which was described and one of its species figured in this journal at the time (NATURE, vol. vii. p. 366). This same able zoologist has the opportunity of adding still another unexpected group of animals, this time from the Miocene beds of the same district, which, though Ungulate and almost certainly Perissodactylate, are very different from any known form.

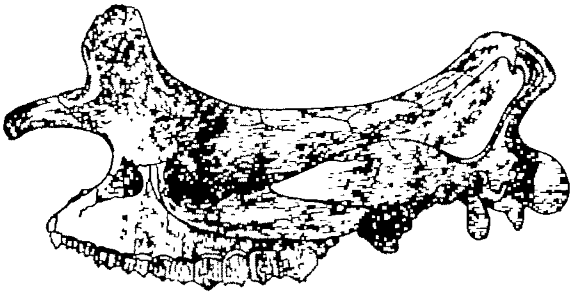
FIG. 1.—*Brontotherium ingens*, Marsh.

Brontotherium ingens is the name given by Prof. Marsh to the animal, the upper and side view of whose skull are shown in the accompanying drawings, copied from his paper in the *American Journal of Science and Art* for this month. The specimen here figured is 36 in. long, and 20 in. between the tips of the two horn-cores. The proportions of the skull are peculiar, the whole being elongate and very slight in depth. The high zygomatic arches, without any well-marked postorbital processes on them, or on the frontal bones, to divide off the temporal from the orbital fossæ, also add to the uncomplicated general appearance of the skull, whose aspect is rendered more abnormal by the development of a huge pair of horn-cores, which spring almost entirely from the firmly "coössified" nasal bones, which make the anterior region of the face exceptionally broad and heavy. The upper part of each horn-core is rugose and the base contains large air cells.

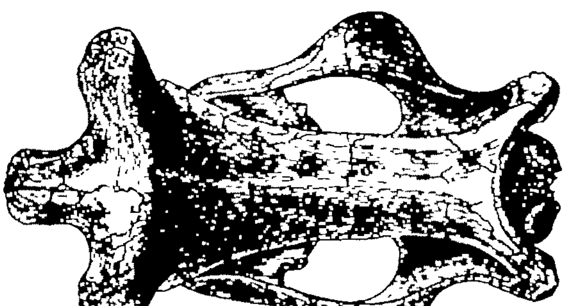
FIG. 2.—*Brontotherium ingens*, Marsh

The teeth present many points of interest. The dental formula is $i.\ \frac{2}{2}\ c.\ \frac{1}{1}\ p.m.\ \frac{4}{3}\ m.\ \frac{3}{3} = 38$. The upper incisors are quite small, and so are the lower. The canines are short, stout, and not removed from the premolars by any interval. The premolars are much smaller than the molars, those of the lower jaw being very Palæotherium-like. The lachrymal foramina are small, and the infraorbital foramina are peculiarly large, as are the occipital condyles. The cervical and most of the dorsal vertebræ are distinctly opisthocœlous. The atlas is much expanded transversely; the odontoid process of the axis is stout and conical. The epiphyses of the vertebræ are, in most specimens, loosely united to the centra. The caudal vertebræ give indications of the tail having been long and slender.

The limbs are shorter than in the Elephant, having the toes arranged as in the Tapir, four in front and three behind. The whole of the distal end of the humerus is occupied by the articulation; the radius and ulna are distinct. The phalanges are all short, and the terminal ones are short and tubercular, as in the elephant. The femur has a small third trochanter; the tibia and fibula are separate, and each complete. The distal facets on the tarsal naviculare are subequal.

Prof. Marsh remarks that "the wide narial opening, the rugose extremity of the nasals, and the very large infra-orbital foramen, naturally suggest that there must have been an elongated, flexible nose, possibly as extensive as in the tapir. That there was no long proboscis, as in the elephant, is indicated with equal certainty by the length of the head and neck, which renders such an organ unnecessary."

That *Titanotherium proutii* of Leidy is closely allied to *Brontotherium*, Prof. Marsh considers very probable; but the former genus was determined from a specimen which wanted the skull, and it differs in some respects. *Megacerops* of Leidy, as well as *Symborodon* and *Miobasileus* of Cope, belong to the same group, but their identification has been established on data too imperfect for complete and correct description.

We have adopted Prof. Marsh's term horn-cores for the large conical bony processes on the nasal bones; but it is not at all certain that such is the nature of these protuberances; for it seems improbable that any large horns could be efficiently employed by its owner at the free end of so elongate and flat a skull; at the same time that if they were directed forward, they would seriously interfere with the animal's power of grazing. It must also be remembered that in *Rhinoceros* the horn is not supported on any osseous core, whilst in the wart-hog (*Phacochœrus*) the wart has a conical osseous support.

The discovery of these entirely new and unexpected types of previously existing animals in the comparatively unexplored region of the Rocky Mountains must give a great stimulus to evolutionary thought; for, more than anything, it helps to illustrate to what extent the geological record is incomplete; and further, how great stress ought to be laid on the imperfection, not of the geological record—but of what seems to vary very nearly inversely as it—human palæontological information. The recent exhumation of these several fully differentiated mammals from American Eocene and Miocene beds, when considered in connection with the occurrence of equally specialised and somewhat parallel lines of development in Europe, tends to substantiate the considerable antiquity and the wide distribution of the higher members of the vertebrate subkingdom, and ought to lead to a more thorough search for prototypal forms in the higher secondary strata, other than the few at present known, so that the vast gap which at present exists in our knowledge of the pedigree of the mammalia, may be filled, partly at least, from the record of Mesozoic formations.

ON THE STUDY OF NATURAL HISTORY*

THE value of Natural History would be more fully appreciated if its higher aims were more perfectly understood. Too many fancied that the study of natural history consisted in mere collecting and naming, and looking at pretty objects. This was, however, mere scientific play; whereas the more thorough study was real work, of use not only as an intellectual training, but also as applied to the practical life of every day. They often heard the remark that the proper study of mankind was man, but to confine their study to him would be to take the first term of a great series, and neglect all the other terms—a proceeding which could lead only to an inaccurate and one-sided view of the order of the universe.

As an illustration of the connection of one class of facts with another he would briefly describe some of the results to which he had been recently led by applying physical methods to the study of the evolution of plants. He had studied the changes that had occurred in the colouring matters in the leaves and flowers during their development from a rudimentary to a perfect state, and the connection between them and the action of light, and had found that there was apparently a most remarkable correlation. When more and more developed under the influence of light, coloured compounds were formed which are more and more easily decomposed by the action of light and air when they were no longer parts of living plants, but dissolved out from them. There was thus apparently some condition in living plants which actually reversed these reactions.

He had also found that in the more rudimentary state of the leaves of the highest classes the colouring matters corresponded with those found in lower classes, and in the case of the petals of flowers their more rudimentary condition often corresponded with some other variety, which thus appeared as if due to a naturally arrested development of a particular kind. This principle would perhaps serve to explain the greater prevalence of flowers of particular colours in tropical or colder regions and at different elevations. Now, since the effect of the various rays of light was different, it became a question of much interest to decide whether an alteration in the character of the light of the sun would produce a somewhat different effect in the case of other classes of plants in which the fundamental colouring matter differed; for example, whether light, with a relatively greater amount of the blue rays, might not be relatively more favourable to the cryptogamia than to the flowering plants. So far this was a mere theoretical deduction; but, if proved to be true by experiment, it might, at all events, assist in explaining the difference in the character of the vegetation of our globe at an earlier epoch, when perhaps our sun was in a somewhat different physical state, and the light more similar to that of Sirius and other stars of the highest and bluer type.

The practical applications of natural history were of course most varied, but he would now merely refer to such as depended upon the equilibrium between different plants and animals. The successful cultivation of useful plants in a foreign country might depend upon very complicated conditions to be learned only by accurate study. The accidental introduction of some plants or animals might prove most injurious if there were no native check to their inordinate multiplication. This was perhaps why in some cases such importations were far more injurious than in their native country, and it became of great importance to learn what means could be taken to provide some adequate check.

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* From an address by Mr. H. C. Sorby at the annual *conversazione* of the Sheffield Field Naturalists' Club, January 5.

TRILOBITES

JOACHIM BARRAUDE has published a preliminary epitome (Prague and Paris, 1871, 8vo) of an intended supplement to his "Système Silurien du Centre de la Bohême."

He therein gives a list of the fossils as yet found in the Cambrian formation :—"PLANTÆ : Palæophycus, 1 species; Fucoides, 2; Archæorhiza, 1; Halopoa, 2; Cordaites, 1; Eophyton, 2; Frœna, 1; Buthotrephis, 1; Scotolithus, 1; Oldhamia, 3; PETRIFICATA INCERTÆ SEDIS : Cruziana, 2; Lithodictyon, 1; ANIMALIA : *Vestigia*, vel Vermium, vel Crustaceorum, vel Molluscorum : Psammichnites, 4; *Spongia :* Astylospongia, 1; *Cœlenterata :* Protolyellia, 1; *Echinodermata :* Spatangopsis, 1; (doubtful Echinoderm?), Agelacrinus, 1; *Vermes :* Micropium, 1; Spirocolex, 2; Scolithus, 4; Monocraterion, 1; Diplocraterion (Arenicolites), 4; Histioderma, 1; *Mollusca :* Dictyonema, 1; Lingula, 2; Lingulella, 1; Discina, 1; Obolus, 1; Hyolithus, 1."

Whilst this formation has only yielded 28 animals, his next epoch, his "Silurische Primordial Fauna" supplies 366 species as follows :—

NUMBER OF GENERA.	CLASSES.	NUMBER OF SPECIES.												DIFFERENT SPECIES.
		CENTRAL EUROPEAN ZONE.		EUROPEAN.		NORTH AMERICA.								
		Bohemia.	Spain.	Scandinavia.	England.	Newfoundland.	Canada—Vermont.	New Brunswick.	New York.	Massachusetts.	Upper Mississippi.	Texas.	Georgia.	
28	Trilobita	27	9	77	61	9	9	18	6	1	37	8	1	252
2	Ostracoda	...	1	5	4	...	...	...	...	...	...	...	...	10
2	Other Crustacea	...	...	...	1	...	...	...	...	...	1	...	...	2
5	Vermes	...	...	...	4	...	...	...	...	...	1	...	...	5
2	Pteropoda	5	...	2	7	...	3	...	...	...	1	...	...	18
1	Heteropoda......	...	...	...	1	...	...	...	...	...	...	...	...	1
2	Gasteropoda ...	...	2	...	...	...	...	...	...	...	2	...	...	4
12	Brachiopoda ..	2	6	8	12	...	5	6	2	...	9	5	...	55
4	Bryozoa	1	...	4	1	...	...	...	...	...	1	...	...	7
6	Cystidea	5	1	...	1	...	...	1	...	...	...	...	...	8
2	Spongia	...	...	...	3	2	2	...	...	...	...	...	...	7
66	TOTAL	40	19	96	95	11	19	25	8	1	52	13	1	366

The author remarks on the discordance between the picture thus offered and that which should appear to give any positive confirmation to Darwinism. He then goes on to remark on some phenomena in the development of Trilobites.

According to the Darwinian theory, the development of the individual should bear relation to the past development of the species. Now Trilobites, as they develope, increase in number of their body segments, and therefore the earliest Trilobites ought to have few such segments. But those of the primordial fauna are generally characterised by the opposite condition, while the number is left in those of the succeeding fauna.

Again, on the Darwinian theory, there ought at first to be but few types, the number increasing later. But, in fact, out of seventy-five genera of Trilobites, no less than seventy-two appear in the first two Silurian faunas, and the three others at the beginning of the third fauna. Moreover, the perfection of organisation by no means gradually increases but is quite irregular.

Once more as regards orders, there is no approximation as we recede in time. The Trilobites, Phyllopoda, and Ostracoda, are as sharply differentiated at their very first appearance as they are later, and the Trilobites of the lowest beds are not less easy to divide into genera than those of a later period. *Bohemilla* might, perhaps, be considered as an intermediate form between *Agnostus* and *Paradoxides* (resembling the former in its segments, the latter in its head), but then its geological position is above, not below, those genera.

In the Primordial fauna no single Trilobite has been discovered which can be regarded as an intermediate connecting link between and two other genera.

Finally, no trace of a Tribolite has been found in the antecedent Cambrian formation, and yet from the number of these fossils found it is eminently likely that had they existed they would have left traces of their existence amongst the Cambrian fossils.

The author concludes that we have here a very important discord between Darwinism and facts.

NOTES

By the kindness of Dr. Draper, of New York, we are enabled to issue to our readers this week a copy, absolutely untouched, of a photograph of a part of the solar spectrum recently obtained by that gentleman by means of the reflection grating suggested by himself, and made by Mr. Rutherfurd. There is no doubt that in all such physical inquiries as those in which Dr. Draper is interested all observations will in time be permanently recorded by means of photography, and to this end the labours of Drs. Draper, father and son, will have contributed in no mean degree.

We learn from the *Athenæum* that the Gold Medal of the Royal Astronomical Society has been awarded by the Council of the Society to Prof. Simon Newcomb, of the United States, for his tables of Neptune and Uranus, and other mathematical works.

The French Academy of Sciences, at their meeting on Monday last, elected Dr. Huggins, F.R.S., and Prof. Simon Newcomb, to fill two vacancies among the correspondents in the Astronomical Section.

We regret to announce the premature death of two eminent French *savants*, Dr. Legros, who has been poisoned in the course of histological researches, and M. Fernand Papillon, well known for his physiological investigations.

Sir Samuel Baker has been appointed Rede Lecturer in the University of Cambridge for the ensuing year. Sir Samuel, upon whom the University conferred the honorary degree of Master of Arts in 1866, will deliver the lecture in the Easter Term.

Sir Samuel Baker was entertained on Monday at a banquet given by the Mayor and Town Council of Brighton, at the Royal Pavilion. In a long and interesting speech Sir Samuel Baker referred to the progress of African discovery, the resources of Africa and the future of the natives, which he does not think hopeful, and of what he did for the suppression of the slave-trade.

There has been a good deal said recently as to the fate of the Memorial to Government in favour of an Arctic Expedition; but we believe the matter stands exactly as it did when our article appeared on Dec. 11 last (Nature, vol. ix. p. 97). The uncalled-for and aggravating delay of Mr. Gladstone in answering the Memorial is only what might be expected.

From the 1st inst. there will be published daily at Copenhagen a "Bulletin Météorologique du Nord," containing the daily reports from the Danish, Norwegian, and Swedish stations. This makes the fourth such publication in Europe, the others being published in France, Russia, and in this country. In almost all other countries the reports appear at least in the newspapers.

Ornithologists will be glad to hear of the safe arrival lately in the Gardens of the Zoological Society, of a pair of very interesting new species of White Stork (*Ciconia boyciana*) from Japan, described by Mr. R. Swinhoe nearly a year ago. This new form presents points of particular interest, as in general appearance it resembles both the common European Stork (*C. alba*) as well as the Maguari Stork (*C. maguari*) of South America, and so tends to favour the impression derived from other facts—such as the geographical distribution of the *Tapiridæ*, of the *Cinclidæ*, and perhaps of the *Cervulidæ*, if, as shown by Sir Victor Brooke, *Cervus pudu* is related to them in important osteological peculiarities—that it is in that direction, and not across the Atlantic Ocean that the European continent was last in communication with the New World.

We direct the attention of our readers to the account, given in another page, of an extraordinary gigantic new form of Miocene mammals, the *Brontotheridæ*, from Colorado, discovered by Prof. Marsh of Yale College.

We are glad to observe that Mr. Dresser's excellent work on "The Birds of Europe" continues to appear with marvellous regularity, considering the amount of work involved. A double number, comprising Parts 23 and 24, is just issued, completing the second volume in fourteen months; and the author states that he has such a large amount of manuscript ready in advance, that he can with confidence promise equal punctuality with the next volume. The work continues to maintain its high character both in letter-press and illustrations. The plates representing the Spoonbill and the Snowy Owl, in the parts just issued, are charming pictures as well as accurate ornithological portraits.

A telegram from Philadelphia announces that the Siamese Twins died on Saturday at their home in North Carolina, aged 63. Eng lived two hours longer than Chang.

We learn that M. J. C. Houzeau has been investigating the directions of the major axes of cometary orbits. He has examined 233 orbits from Madler's catalogue, but for the purposes of his investigation he has eliminated those comets of short period, having their aphelia inside the orbit of Neptune, numbering 15, and also the seven probable appearances of Halley's comet and three others whose elements are uncertain, and so reduces the number 233 to 208; he, however, adds Comet I., 1819, making 209 comets whose orbits he discusses. He finds that there is a decided tendency in the major axes of those orbits to place themselves parallel to the double heliocentric meridian 102° 20′ and 282° 20′, being only 28° from long. 254° 5′, in which the point that the solar system is approaching is situate. The major axes do not, however, show a tendency to aggregate near lat. + 57° 26′, in which the before-mentioned point lies; but he observes that it is probable that a large number of southern comets have passed unseen, and that there may be inaccuracies in the elements of the orbits.

We have received several documents relating to the School of Mines, Ballaarat, which was established in 1870, its primary object being to impart instruction in the various branches of science relating to mining, the theory and practice of mining, mine management, mining surveying, and mining engineering. It grants certificates to all classes of men connected with mining, from mining-engineers and assayers to engine-drivers, all candidates being subjected to a good testing examination. The attendance at the School has increased every quarter since it was started, the number of students in the third term of 1873 having been 59. The benefit likely to accrue to a mining country such as Victoria from an efficient school of this kind is incalculable, an those interested in the welfare of the colony are bound to do all in their power to bring it into thorough working order, and enable it to become a national institution. The school is possessed of a good metallurgical laboratory, but its efficiency is sadly hampered for want of funds, the fees payable by the comparatively small number of students being quite insufficient to maintain the requisite staff of teachers. Government grants only 500*l.* a year, private subscription and fees amounting to about another 200*l.*, but to keep up a full staff of lectures, the Council require an income of at least double what is now at their command. This is surely a case in which the industrial welfare of the whole colony is involved, and we therefore think it is certainly the duty of the Government to see that the Ballarat School of Mines does not fall short of complete efficiency for want of funds.

AMONG Mr. Murray's announcements of new works we notice the following, which may be of interest to our readers:—"A Memoir of Sir Roderick Impey Murchison," based upon his journals and letters, with notices of his scientific contemporaries, and a sketch of the rise and progress, for half a century, of palæozoic geology in Britain, by Professor Archibald Geikie, LL.D., F.R.S. This book will be published, we understand, early in the spring. "The Moon, considered as a Planet, a World, and a Satellite," by James Nasmyth, C.E., and James Carpenter, F.R.A.S. This work will be accompanied by numerous illustrations produced from drawings made with the aid of powerful telescopes, woodcuts, &c. "The Impending Famine in Bengal: how it will be met, and how to prevent future famines in India," by Sir Bartle Frere, D.C.L., with maps, &c. "England and Russia in the East," a series of papers on the political and geographical condition of Central Asia, by Major-General Sir Henry Rawlinson, F.R.S., with a map. A new edition of "Metallurgy," by Dr. John Percy, F.R.S., Lecturer on Metallurgy at the Government School of Mines, vol. i. containing Fuel, Wood, Coal, Copper, Zinc, &c. A new edition, re-constructed and re-written, of the first volume of Prof. Phillips' "Geology of Yorkshire," comprising the coast of the county. It will contain a large number of additional illustrations and be issued in quarto size.

ANOTHER work on the threatened famine in Bengal is announced by Messrs. Trübner & Co. It is by Dr. W. W. Hunter, Director-General of Statistics to the Government of India, and will be entitled "Famine Aspects of Bengal Districts."

DR. SCHWEINFURTH'S account of his travels and discoveries in Central Africa during the years 1868 to 1871 will be published by Messrs. Sampson Low & Co. in the course of a few days. This work will be translated by Ellen E. Newer, and will contain an introduction by Winwood Reade, whose work on Africa was reviewed in NATURE a few months back. It will be copiously illustrated by woodcuts from drawings made by the author, and will be issued in two octavo volumes.

IT is known that the Russian Government have made considerable preparations in view of the great astronomical event of this year. A General Assembly of the Commission charged to study the question has finally adopted twenty-seven stations, the list of which (with latitude and longitude, the instruments available at each, and the chances of good weather) may be found in the *Revue Scientifique* for 10th inst. The probable temperatures of the different stations, at the time of observation, are also estimated; they range from — 20° C. at Kiakhta, and — 10° at Omsk. to + 10° at Naktritchevan and Erivan. At Nertschinsk and some other stations in Eastern Siberia, for which calculations are not had, the cold is expected to be still greater. The observers for the different stations have all been appointed, and have been engaged in practising with their instruments at the observatory of Pulkowa. All the telescopes are mounted equatorially; and the equatorials are fitted with a clock-work movement and a micrometric apparatus. The personal equations of the different observers will be determined by means of an apparatus like that of M. Wolf (Paris Observatory), in which an artificial star is observed in its passage across a net-work of wire. The telegraphic determination of the longitudes of the various towns of Siberia is likely to be completed in 1875. Several of the stations chosen for this determination are also stations for the transit observations. The other transit stations will be easily connected with these by chronometric observations, and as for the stations bordering on the Caspian Sea and the Black Sea, these longitudes are already known with sufficient exactness.

PROFESSOR WHITTLESLEY has given a paper on the fluctuation of the level of Lake Superior—a subject specially studied by him many years ago, and which has as yet received but slight elucidation. In his present communication he has confined himself to the consideration of those fluctuations which are not only transient, but also occurring with the regularity of a wave—those low pendulum-like pulsations which are probably common to all the lakes, but are most noticeable in Lake Superior. Until a better theory can be found, he adopts the explanation that these undulations are caused by atmospheric movements.

THE Tower Hill Microscopical Club holds its first *Conversazione* at 3, Great Tower Street, on Tuesday, the 27th inst.

IN a letter to the *American Journal of Science and Arts*, dated Cordoba, September 18, 1873, Dr. Gould gives an account of a remarkable swarm of large grasshoppers, or locusts, recently witnessed there. Myriads filled the air, invaded the houses, and covered the ground, from which they rose like thick clouds of dust, on approach of man or beast. These, however, seem to have been merely the stragglers of the main body. Going out to observe the phenomenon more closely, Dr. Gould saw, to the eastward, what looked like a long trail of dense black smoke, extending over 160° of the horizon, and to an altitude of about 5°. A strong field-glass showed that it was no smoke, but a swarm of locusts. Its width there was no means of determining, but from the position of the focus needed for resolving the cloud at its point of nearest approach, Dr. Gould estimated that none of the swarm passed within less than three or four miles. The insects were evidently transported with the wind, which blew from the north with a velocity of ten miles an hour. This was at 10 A.M. (on August 13). The head of the column had passed far out of sight, and certainly twenty miles of its length were visible over the far-stretching pampa. They continued to pass in apparently undiminished numbers till daylight failed. On September 1 the phenomena were repeated, the insects being borne back by a south wind; and they were coming directly on the town when the wind hauled to S.E. and carried them past about six miles off. From measurements made, Dr. Gould stated that the height of the dense nucleus must have been at least 2,000 ft., its width here not more than six or seven miles; the whole environed by a penumbra of stragglers. At the time of writing, the wind had brought them on in full force "literally darkening the sun," and "there is probably not a square inch of our grounds unoccupied by them."

"CRONACO DEL VESUVIO," by Prof. Palmieri (Naples: Detken and Rochall, 1874), contains a brief summary of the principal eruptions from 1840 to 1872, by far the greater part of the work, however, being occupied with details of the outbreak on April 26, 1872, Palmieri's account of which has been already noticed in these pages. The present work contains several appendices on subjects of chemical and mineralogical interest in connection with eruptions of Vesuvius.

WE learn from the *Medical Record* that the Geographical

Society of Italy has received from Alexandria, with the news of the death of the explorer Miani, and various ethnological objects, two living individuals whom he had forwarded of the tribes of the Akka or Tikku-Tikki, and whom the learned traveller had bought of the King Munza. These individuals—of whom one is eighteen years old, and forty inches in height, and the other sixteen and thirty-one inches high—are stated by Miani to belong to the race of dwarfs described by Herodotus, and recently re-discovered by the German explorer Schweinfurth, who described them carefully. They are pot-bellied, very thin-limbed, and knock-kneed, spherical and prognathous crania, very long limbs, copper skins, and crisp, tow-like hair.

"Extra No. 14" (Scientific Series) of the *New York Tribune* is devoted to accounts of three scientific expeditions. The first the Hayden Expedition of 1873, an account of which is given in a letter from Prof. W. D. Whitney, and in a review by Dr. F. V. Hayden; of the progress of this expedition we have at various times given news. A "New Route to Yellowstone Park" is described in the account of Captain Jones's Expedition of 1873. The third expedition is that of the late Professor Agassiz to Brazil, the *Tribune* reproducing the six lectures delivered by Agassiz after his return in February 1867.

We have received in a separate form two papers communicated to the French Academy by M. A. Poëy—one on the "Connection between Solar Spots and the Hurricanes of the Antilles, of the North Atlantic and the Southern Indian Ocean," and the other on the "Connection between the Solar Spots, the Storms at Paris and Fécamp, the Tempests and Sudden Storms in the North Atlantic."

We have received from Quebec the "Transactions of the Literary and Historical Society" of that city, for session 1872-3, the longest paper in which is an interesting diary of "A Whaling Voyage to Spitzbergen in 1818," kept by Dr. James Douglas. Another paper, by Dr. H. H. Mills, contains some observations on Canadian Chorography and Topography, and on the meritorious services of Jean Baptiste Duberger, sen., who died in 1821, and who seems to have been an excellent surveyor and map-maker. The Society appears to have been in existence for many years, is in a flourishing condition as to members and income, and possesses a good Natural History Museum. We are glad to see that the Society's programme includes scientific as well as literary and historical subjects.

The *Bulletin* of the French Geographical Society, for December, contains an account of a voyage made last autumn by M. A. Pinart along the south coast of the Aleutian Isles and the Peninsula of Alaska, illustrated by a good map; the continuation of M. J. Halévy's Journey to Nedjràn; and a very long paper by M. Dournaux-Dupéré, on the part which France ought to play in Northern Africa, advocating the complete subjection of the Sahara by France.

"On the Geology of Western Wyoming," is a paper by Mr. T. B. Comstock, reprinted from the *American Journal of Science and Arts*.

The additions to the Zoological Society's Gardens during the past week include two Cinereous Sea Eagles (*Haliaëtus albicilla*), European, presented by Sir Victor Brooke, Bart.; a pair of Pink-headed Ducks (*Anas caryophyllacea*) from India, new to the collection; a Nicobar Pigeon (*Calænas nicobarica*), from the Indian Archipelago; a Cheetah (*Felis jubata*) from Africa; a White-lipped Peccary (*Dicotyles labiatus*), from South America; a Sooty Mangabey (*Cercocebus fuliginosus*), from West Africa; a Verreux's Guinea fowl (*Numida cristata*), from East Africa; a Masked Weaver Bird (*Hyphantornis personata*), from West Africa; and four Grenadier Weaver Birds (*Euplectes oryx*), from Abyssinia, purchased, or received in exchange.

NEW REMARKS ON THE NATURE OF THE CHEMICAL ELEMENTS, BY M. BERTHELOT[*]

THERE will be no necessity for me to remind the Academy of the great importance of the question raised at the last meeting. Between our illustrious master, M. Dumas, and the author of these lines, there should not be any difference of opinion neither on the principles of a science which he himself has taught us, nor as to the originality of his ideas with regard to the chemical elements, their relation to each other or to the organic radicles.

It is therefore more for the purpose of avoiding the reproach of an incomplete knowledge of the science than for further insisting on what I have before advanced, that I ask his permission to quote in this place, p. 280 of his "Leçons de Philosophie Chimique" a passage in which he has approached my own remarks:—

"Before commencing with any confidence to build a system upon this foundation," says M. Dumas, "it is necessary that a great number of exact experiments should increase our knowledge of it. It will therefore be of the greatest importance to study compounds in relation to their capacities for heat, for it cannot be supposed that the relation of the specific heat to the weight of the atom holds only for elementary bodies; it is also found in compounds of the same order. It would therefore be wrong to seek in this direction for a proof of the truth of the ideas which we have imagined of the bodies which appear to us to be elements, and we ought to say that the capacity of their chemical atoms tends towards equality because they are bodies of the same order, without their elementary nature necessarily following from it."

In support of these opinions M. Dumas cites the then recent experiments of Naumann on the specific heats of the carbonates of barium, strontium, calcium, iron, zinc, and magnesium, which, multiplied by their corresponding atomic weights, give a constant product of 131; while the sulphates of barium, strontium, calcium, and lead give the product 155. M. Dumas adds with reason—"For the other compounds we are in want of data sufficiently precise to enable us to make similar comparisons."

It will be seen then that in 1836 the point in question is no longer the relation between the specific heats of compounds and that of their elements, but entirely between compounds of the same order, *à fortiori* the possibility of distinguishing elements from compound bodies in general by means of specific heats was expressly discarded.

Although the specific heats of compound bodies were formerly but little known, the gap was being filled for many series by the researches of M. Regnault. But Regnault, like Naumann, confined himself to the determination of the specific heats of compounds the constitution of which was similar, without seeking to establish any more remote relation.

Woestyn in 1848 was, I believe, the first to announce the approximate relation between the specific atomic heat of a compound and those of its elements, and the partial relations discovered by Naumann and Regnault then became a consequence of this more general law.

In applying it in my turn to the organic radicles and especially to the carbides of hydrogen, I have been led to put in evidence the difference which distinguishes their specific heats from those of the elements, whether taken individually or together, as members of a group of bodies of the same order. The carbides of the ethylene series are bodies of the same order, and present quite as many analogies amongst themselves as simple radicles such as calcium, barium, strontium, iron, zinc, and magnesium do, and the same may be said of the combination formed by these radicles. I repeat, therefore, that as the specific atomic heats of the simple radicles have the same value, and this value being known and considered in connection with their atomic weights, the simplicity of their composition results of necessity therefrom almost always, as I have already established in my preceding note. At the same time the specific atomic heats of the compound radicles are nearly always multiples of one another, and their magnitude is sufficient to establish the complexity of the radicles themselves.

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[*] Translated from the *Comptes Rendus* of December 15, 1873. At the preceding meeting M. Berthelot read a note on the same subject, in which he gave a more detailed description of the observed differences between the specific atomic heats of elements and compounds. The principal points were, that whilst the specific atomic heats of elements whose atomic weights are multiples of the same number, such as the S, Se, Te group, are identical, the specific atomic heats of compounds whose atomic weights are multiples of some common number, as in the group of polymerised hydrocarbons, ethylene, amylene, caprylene, &c., are multiples of each other, being proportional to their atomic weights. He concludes: "This difference is important, inasmuch as the notion of specific heat is a representation of the general molecular work by which bodies are maintained in an equilibrium of temperature with each other and it would also indicate that the decomposition of our elements, if possible, ought to be accompanied by phenomena of another kind than those which determine the decomposition of our compounds."

The combinations of the same order formed by the simple radicles have all of them nearly the same atomic heat, as the observations of Naumann and Regnault have shown. On the contrary, combinations of the same order formed by a series of analogous compound radicles exhibit specific heats which tend to increase proportionally with the variation of their atomic weights. Thus, which is precisely the opposite of the relations which would have been supposed to exist between compounds of the same order at the time of Naumann's researches on the identity of the specific atomic weights of the carbonates and sulphates, is another proof of the complexity of these radicles.

To sum up, the study of the specific heats established by the most recent researches, tends to prove that there is a positive characteristic which, it seems to me, distinguishes the elements of modern chemistry from its compounds, and shows that no known compound body ought to be considered as of the same order as an actually simple one. The importance of such a characteristic cannot be doubted, and it becomes greater on account of the mechanical meaning which modern theories ascribe to specific heat. This I feel bound to put in evidence.

Nevertheless, and I ask permission to return once more to this point, exaggerated conclusions must not be drawn from such an opposition between the mechanical and physical characteristics of our simple and compound bodies.

If our elements have not as yet been decomposed and appear not to be decomposable by the forces which are at present at the command of the chemist, and which, as M. Dumas at the time of his discussion with Despretz justly remarked, have been so often tried in vain, nothing compels us to assert that they are not decomposable in another way than our compounds are; as, for instance, as Mr. Lockyer asserts, by means of the forces acting in cosmical space. Nor does anything prevent the supposition that such a discovery as that of voltaic electricity would enable the chemists of the future to overpass the limits which are imposed upon us.

The possible fundamental identity of the matter constituting our elements, and the possibility of transmuting into one another the so-called elements, can moreover be admitted into the category of more or less plausible hypotheses without it necessarily resulting that there is a single really existing matter of which our actual elements represent unequal states of condensation. In fact nothing compels us to conceive the existence of a final decomposition which shall tend necessarily to reduce our elements either to more simple bodies, from the addition of which they arise, or to multiples of a single elementary ponderable unit. The various states of equilibrium under which the fundamental matter manifests itself would exhibit certain general relations to each other analogous to those which exist between the multiple values of the same function. According to this hypothesis, an elementary body could be broken up without being destroyed in the ordinary meaning of that word. At the moment of its destruction it would suddenly change into one or more simple bodies identical with, or analogous to, our elements. But the atomic weights of the new elements would not show any simple relation to the atomic weight of the element which had by its metamorphosis produced them, the absolute weight alone would remain unaltered throughout the catena of changes.

But I do not wish to insist further on this hypothesis of a matter fundamentally identical, although multiform in its appearances, and characterised in each one of them by a peculiar mode of motion, such, in fact, that no single one of them can be definitely considered as the starting-point of all the others. Nevertheless, we shall only be too glad if Mr. Lockyer, guided by stellar spectral analysis, succeeds in shedding a new light upon these hypotheses, and continues to investigate questions which M. Dumas raised forty years ago in a book which has contributed so much to our scientific education.

R. J. F.

SCIENCE IN LIEGE

WE have received a somewhat bulky volume of Memoirs of the Royal Society of Science in Liege (1873), in which a considerable variety of subjects comes under notice; zoology and mathematics being, perhaps, the most largely represented.

An *Eloge*, by Prof. Morren, on Jean Théodore Lacordaire (who died in 1870), is accompanied with a good portrait of that eminent naturalist. Lacordaire was originally destined for the law, which, however, he left for commerce. Going out to South America in a business capacity, his bias for natural research was quickly developed, and he commenced those labours to which his after life was devoted. In 1835 he became professor of zoology at Liege, where he continued till his death. Lacordaire was a voluminous writer, but his *summum opus* is the *Genera des Coléoptères*, which is remarkable for the minuteness of its details and its rigorous truthfulness.

For some years past we have heard a great deal about the services which birds render to agriculture by destroying injurious insects. The sentiment is widespread, and vigorous measures of bird protection have been taken in various countries of Europe. M. Edouard Perris here brings forward a somewhat opposite view, which he supports by many curious facts from a long experience of country life. His position is, briefly, the following:— 1. Birds are congregated in considerable numbers only in the migration time in autumn and spring, when insects are very much less numerous than in the fine season. The rest of the time they live in pairs, sparsely distributed, and rare in cultivated parts, while the insects come forth *en masse* to do their mischievous work. 2. Birds destroy insects largely, but these insects are, in great part, indifferent, others are eminently useful; and the really hurtful species destroyed are in very small proportion to the whole. Thus the birds do us little service; and they often do injury in destroying our carnivorous and parasitic insects, as well as attacking fruits and seeds. 3. The insects we have most to complain of are, some of them, big enough to brave the birds, others are too small to attract them; others prove disagreeable as food; many are nocturnal in habits, or, by their immobility, escape notice; some live underground, or in houses; and all have an astonishing fecundity which is quite baffling to human resources. 4. Larvæ and caterpillars, which do the most damage, live nearly all concealed under ground, under bark, in the deep parts of wood, in the roots of plants, in fruits, in inhabited places, &c., and furnish little tribute to the birds. Those which develop in open air are generally provided with hairs which protect them; some are nocturnal, some are extremely small. All these facts, in the author's opinion, should greatly modify the ordinary view as to the utility of birds in agriculture. He points out that certain natural influences tend to preserve an equilibrium in the insect world; such are, famine occasioned by the too great multiplication of individuals, meteorological phenomena adverse to their growth or metamorphosis, and the abundant production of parasites. M. Perris does not find fault with measures of bird protection, but objects to the undue merit which is assigned to birds. He urges upon farmers the importance of exercising more discernment in their destructive measures, and of respecting many animals they often regard as nuisances, such as hedgehogs, snakes, lizards, toads, &c.

Medical men will doubtless be interested in two teratological observations communicated by Dr. Eugene Charlier. One is that of a child inferiorly double, or a double ileadelphous monster. It is a kind of monstrosity of which Is. Geoffrey Saint-Hilaire supposed the existence, but of which he did not know any authentic case. The other monstrosity is a new variety of pygomelian chicken; the animal has two accessory limbs joined to the normal wing and leg on one side. The forms are represented in plates annexed to the notes in question.

An important mathematical paper by M. Brasseur, furnishes a "new exposition of the principles of the differential and integral calculus." The following sentences from a commendatory preface by M. Folie, will give some idea of the point of view which the author adopts. "Of all the modes of considering the differential calculus, we do not know of any more philosophical than that of Newton; but it requires, to be properly understood, a mind well trained to metaphysical speculations. We have, indeed, known good analysts who never grasped it, though they had studied at the best sources. The great difficulty of the differential calculus is that it attempts to analyse the idea of continuity; it seeks to express how a function passes in a continuous manner from one state to another; and it is this passage which has given rise to the contradictory idea of the infinitely little, to the indirect idea of limits, and to the philosophic idea of Newton. Brasseur has avoided this great difficulty; he has succeeded in rendering the method of La Grange, who only employs finite analysis, as convenient in its applications, and as rigorous, as that of limits or of fluxions. We will even say that his method has, from the educational point of view, the advantage, over that of fluxions, of not requiring any metaphysical notion; and, over that of limits, of being much more direct, and not exposed to any attack, even specious. Instead of analysing the idea of continuity he studies two successive states of a continuous function; and continuity only comes in so far as that the difference between these two states may become as small as we choose without ever becoming *nil*, as seems to be the case in limits; or infinitely little, in the old signification of the word, a signification simply absurd."

We simply name, in conclusion, the following zoological lists, which make up the greater part of the volume:—Monography of the Malabrides, by M. de Marseul; Synopsis of the Scolytides, by M. Chapuis; and new or little known Araneides from the South of Europe, by M. Simon.

SCIENTIFIC SERIALS

Zeitschrift für Ethnologie (1873). The fifth number of the journal for last year is of less than average importance to English readers, since the principal article—a most valuable and comprehensive one on the descriptive ethnology of Bengal—is a translation of Colonel Dalton's digest of the official reports drawn up by the different Commissioners of the province, and published at the cost of the Indian Government. This work, which supplies information that can nowhere else be found in regard to the tribes occupying the Brahmaputra and Gangetic valleys, must henceforth be considered as indispensable to every student of Indian ethnology, and the editors of the *Zeitschrift* have done good service in making it known to their readers. In an article on a proposed improvement in the methods of craniometry now in use, Dr. Jhering passes in review the difference in the values of the indices, proposed by Blumenbach, Retzius, Broca, and others, for the definition of Dolichocephalic and Brachycephalic types. His three main propositions are briefly these:—1. All cranial measurements must be projected in a line that is parallel or vertical to the horizontal base of the cranium. 2. The most important maximum and minimum dimensions should be obtained *per se*, and without reference to distances from definite anatomical points. 3. For all parts not in the medial plane, the percentage of lengths and heights must be given at the points where such parts intersect these diameters. Dr. Jhering thinks that it is time finally to set aside the theory transmitted from Blumenbach, and through Retzius to the present day, that every race possesses at once a special language, and a special type of cranium. According to his view it is never possible to determine with certainty from the form of the skull the precise race from which an individual has sprung, and in his opinion the problems which ought to engage the attention of future students of craniology are the determination of the *mean* cranial type of each race; and the definition of the limits within which each special type varies among different races. Finally the author wishes to show that craniology is not competent to determine questions of race, but is merely to be accepted as an auxiliary science to anthropology. The learned missionary, Th. Jellinghaus, to whom we are already indebted for many valuable contributions to our knowledge of the languages spoken by the outlying tribes of our vast empire in India, gives in this number a short account of the language of the Munda Kohls of Chota Nagpore. The peculiarities of their tongue seem to be a distinct dual for all three persons: the formation of the plural and dual by the addition of an abbreviated form of the third personal pronoun; the insertion of the letter *p* with the vocal accent for the formation of the plural and dual of certain nouns and adjectives; the interpellation of the letter *n* in the root-syllable of the verb to form the abstract noun. The units of the Munda Kohls' numeral system are 10 and 20. The author describes these people as kind and simple in their social relations with one another. Herr Virchow draws attention to a specimen of a synostolic cranium as the form has been figured and described by J. B. Davis in his work on "Synostolic Crania among Aboriginal Races of Man" (Haarlem, 1865). As this skull belonged to a rachitic child, and similar skulls, in which the calvaria was entirely obliterated, and the cranial bones were thickened outwardly, are preserved in the Berlin and other Pathologico-Anatomical collections, and were taken from rachitic subjects, Herr Virchow considers that such forms must be held to be quite independent of ethnological peculiarities, and that their occurrence amongst savage or aboriginal races must be ascribed to the frequent presence amonst them of rachitism—a fact to which Pruner-Bey has already drawn attention. We cannot close our notice of the contents of this number without mentioning an interesting communication by Dr. Brehm in regard to his experience—based on an eight years' acquaintance—of the habits of the Chimpanzee under confinement. The last individual which fell under his notice, and which died at the age of four from pulmonary disease, showed, in many respects, an aptitude of comprehension, a docility and a capability of practising the ordinary usages of daily life which made the animal an interesting and wholly unobjectionable inmate of Dr. Virchow's house, where he ran about with little more surveillance than would have been awarded to a human child of the same age. The result of the learned author's experience of this, and other individuals of the race is, that although not human, there is *very much* of the element of humanity in the Chimpanzee.

Poggendorff's Annalen der Physik und Chemie, No. 9, 1873.—This number commences with a theoretical examination, by the editor, of the action of Holtz's electrical machines of the "second" kind, those being meant which have two discs rotating in opposite directions, whereas in the "first," and more common kind, one disc rotates while the other is stationary. The author's view is, not that there is suction, by the conductors, of the electricities expanded in the insulators, as commonly supposed, but conversely, that electricities separated in the conductors, through induction, stream over to the insulators. In this way, both modes of excitation, by induction and by inflow (*Einstromung*), are explained on one principle. The same holds good for machines of the first kind.—M. Julius Thomsen continues his "Thermo-chemical Researches," investigating here the action of four agents of reduction, and seven of oxidation.—Dr. Muller describes a new tangent galvanometer and rheostat, free from the disadvantages of not being equally available for currents of all degrees of intensity, and of waste of time in use. The galvanometer differs from ordinary ones in the arrangements for reading and deadening; and, in the rheocord, to neutralise heating effects with strong currents, the wires are surrounded by distilled water.—There are four papers referring to the "horizontal pendulum;" in two of which M. Zollner describes the instrument as he constructs and uses it, giving several observations made with it, which indicate its great sensitiveness. In a third paper he represents that the idea was first conceived by Lorenz Hengler, a writer in "Dingler's Polytechnisches Journal" in 1832; while in a fourth note on the subject, Prof. Safarik produces evidence of the same fact, and also shows that the bold idea of demonstrating the variations of gravity and of cosmic attractions by terrestrial observations in one place, had already been expressed and experimented on by Gruithuisen, some fifty-two years before Zollner, viz., in 1817.—M. von Bezold communicates the first part of a valuable paper on the law of colour mixtures, and the physiological primary colours; and Prof. Clausius discusses a new mechanical proposition with reference to stationary motions.—In a note translated from the Italian, the question is considered by Prof. Roiti, Is the electric current an ether current? He argued that if this were the case, then the velocity of propagation of light in a body traversed by a galvanic current must be altered by the direction of this current. In his experiments he caused rays from two parallel slits to pass through two cell-divisions, respectively, of a rectangular glass vessel containing sulphate of zinc solution (the thickness of the dividing wall being equal to the interval between the slits). Interference fringes were obtained at the exit of the rays. Four electrodes being inserted, so that a current passed in opposite directions in the two cells, this had no effect in displacement of the fringes. M. Roiti concludes that if the galvanic current were an ether current, it must have a very small velocity, less than 200 metres per second, which does not agree with the phenomena of galvanic electricity.—Prof. Mach's paper on the stroboscopic determination of the pitch of tones, deserves the attention of musicians and others.

Der Naturforscher, Nov. 1873.—Among the botanical notes in this number is one on the age and mode of growth of woody plants in Greenland. M. Kraus finds that these plants often attain great age (150 years, *e.g.* in the case of some willows), but that the annual increase of thickness is extremely small, 1·5 mm. at the maximum.—Some experiments described by M. Godlewski prove that formation of starch in chlorophyll granules is not possible without access of CO_2; that the liberation of starch from these granules may occur in bright light; that we cannot, from absence of starch, infer there is no process of assimilation; and that the cause of change of form in etiolated plants does not lie in the suppression of the assimilative process. Mr. Sorby's observations on the colour of plants are also given.—In physics and chemistry, we have an account of experiments by M. Meyer as to the influence of access of air on alcoholic fermentation, MM. Favre and Valson's researches on work done in saline solutions, those of M. Edlund on the electromotive and thermoelectric forces of metallic alloys with copper, those of MM. Mach and Fischer on reflection and refraction of sound, Prof. Maxwell's lecture on molecules, and other papers.—In a physiological paper, entitled "The internal mechanics of nerves," some additional light is thrown by M. Bernstein on the electrotonic state investigated by Pfluger, and a hypothesis is offered, to account for the phenomena. There are also biological papers on the apparatuses for production of sound in insects, and on the deep-sea fauna of the Swiss lakes.—We further note an interesting lecture by M. Sandberger, on a portion of the geological history of the *Oberrheinthal*; and a quantity of valuable information in the *Kleinere Mittheilungen*.

Journal of the Franklin Institute, Dec. 1873.—In this number are given two reports of the Committee on the mode of determining the horse-power of steam boilers. A division of opinion is indicated, a minority in the Committee holding, that the horse-power of boilers for stationary engines is properly defined as the capability to evaporate a cubic foot of water per hour from and at the temperature of 212° F. and there is no reason for modifying its normal value; while the majority (4 against 3), consider, that, in view of variations of capacity of the same boiler under varied conditions, the discontinuance of the term horse-power, as descriptive of the size and capacity of the boiler, is advisable; purchasers and makers should, instead, describe fully and in accurate terms, the evaporative capacity of boilers poposed, and the conditions under which they are worked and tested. Considerations are urged in support of each of these views.—Mr. Richards continues his "Principles of shop manipulation, for engineering apprentices;" this part treating chiefly of the various kinds of motive machinery.—A paper read by Mr. Ransome before the Franklin Institute gives an account of the improvements which he has introduced into the manufacture of artificial stone.—We also find notes on a new hydraulic railway car-brake, by Mr. Henderson, and on the stability of towers and chimneys, by Mr. Evans.—Among the "Items and Novelties" reference is made to some important results obtained by Prof. Thurston, from experiments at the Stevens Institute of Technology, as to the behaviour of metals under stress. The following deduction was repeatedly confirmed:—Metal strained so far as to take a permanent set, and left under the stress producing it, gains in power of resistance up to a limit of time, which in these experiments was about seventy-two hours, and to a limit of increase which has a value, in the best iron, of about 20 per cent., where the applied force is 80 per cent. of the ultimate breaking force.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, Jan. 8.—Dr. Hirst, F.R.S., president, in the chair.—Mr. J. W. L. Glaisher read a paper on the transformation of continued products into continued fractions. The paper had its origin in a remarkable continued fraction for π given by Prof. Sylvester in the Philosophical Transactions for 1869, viz.:—

$$\frac{\pi}{2} = 1 + \cfrac{1}{1 + \cfrac{1 \cdot 2}{1 + \cfrac{2 \cdot 3}{1 + \cfrac{3 \cdot 4}{1 + \cfrac{4 \cdot 5}{1 + \&c.}}}}}$$

which is there shown to be equivalent to Wallis's formulæ—

$$\frac{\pi}{2} = \frac{2 \cdot 2 \cdot 4 \cdot 4 \cdot 6 \cdot 6 \cdot 8 \cdot 8 \ldots}{1 \cdot 3 \cdot 3 \cdot 5 \cdot 5 \cdot 7 \cdot 7 \cdot 9 \ldots}$$

Prof. Sylvester arrived at his result by means of a complete solution he had previously found of the equation of finite differences

$$U_{x+1} = \frac{U_x}{x} + U_{x-1}$$

(a "shaving" as the discoverer remarked from his method of "reducible cyclodes") whereas the present paper arrives at the result by a direct transformation.—Prof. Clifford spoke upon the foundations of the differential calculus and of dynamics. The points dwelt upon were—all continuous quantity is expressed by lines, therefore every fluxion is really velocity or the method of fluxions and the beginning of kinematic is the same; he then dwelt upon Newton's definition of tangents and Hankel's remark, and pointed out the necessity of a modification of the definition, traced out certain analogies to the methods of Euclid and Archimedes (definition of fourth proportional and of area), and closed his remarks with a proof of the composition of velocities.—The secretary read a note from Prof. Crofton on a method of treating the kinematical question of the most general displacement of a solid in space.—Mr. Perigal made a few remarks on the subject of link trammels (in connection with Prof. Sylvester's recent paper), in the course of which he stated that Peaucellier's lozenge-trammel is a modified reproduction of Jopling's "double cranks" trammel, invented about 1822. The statement was impugned by Prof. Sylvester and Mr. S. Roberts.—The following papers were taken as read:—On Hamilton's characteristic function for a narrow beam of light, by Prof. J. Clerk-Maxwell.—Preliminary account of investigations on the free motion of a solid in elliptic space, by Prof. Clifford.

Geological Society, Jan. 7.—Prof. Ramsay, V.P.R.S., Vice-President, in the chair. The following communications were read:—1. "The Origin of some of the Lake-basins of Cumberland."—First Paper. By J. Clifton Ward, F.G.S. After referring to the fact that the question of the origin of lake-basins cannot be satisfactorily discussed unless the depth of the lakes and the heights of the mountains are brought before the mind's eye in their natural proportions, the author sketched out the physical geography of the lakes under discussion (Derwentwater, Bassenthwaite, Buttermere, Crummock, and Loweswater), and pointed out what must have been their original size and shape before they were filled up to the extent they now are. These lakes were not moraine-dammed, but true rock-basins. The belief that the present Lake-district scenery was the result of the sculpturing of atmospheric powers, such as we see now in operation, varied by climatal changes and changes in the height of the district above the sea, was enforced, and the opinion given that the work of elaboration of the lake-country scenery has been going on ever since Carboniferous or pre-Carboniferous times. When all the evidence was considered—the fact of the lake-hollows under examination being but long shallow troughs, the thickness of the ice which moved along the valleys in which the lakes now lie, the agreement of the deepest parts of the lakes with the points at which, from the confluence of several ice-streams and the narrowing of the valley, the onward pressure of the ice must have been greatest,—the conclusion was arrived at that Prof. Ramsay's theory was fully supported by these cases, and that the immediate cause of the present lake-basins was the onward movement of the old glaciers, ploughing up their beds to this slight depth.—2. "On the Traces of a Great Ice-sheet in the Southern part of the Lake-district and in North Wales." By D. Mackintosh, F.G.S. In this paper the author brought forward the evidence which seems to him to establish the existence in the southern part of the Lake-district of a "valley-ignoring and ridge-concealing ice-sheet." He gave a table of the direction of ice-marks observed by him in the Lake-district, and stated that about Windermere and Ambleside the general direction is nearly N.N.W., round Grasmere between N.W. and N.N.W., north-west and west of Grasmere in upland valleys and on high ridges about N. 30° W., south of Grasmere and in Great Langdale N. 35° W., and in the Coniston district a little W. of N. In many places he recognised an uphill march of the ice. The author also referred to the glaciation of North Wales, some of the marks of which, observed by him in a district south of Snowdon, seemed to him to indicate the southerly movement of a great ice-sheet capable of ignoring or crossing deep valleys.—3. Notes on some Lamellibranchs from the Budleigh-Salterton Pebbles. By Arthur Wyatt Edgell, F.G.S.

Linnean Society, Jan. 15.—Mr. George Bentham, F.R.S., in the chair.—Dr. Hooker, Pres. R.S., exhibited a very beautiful series of specimens of Fossil Copal, the product of *Trachylobium Hornemannianum*, some specimens of Recent Copal from the same plant, and some fruits of a *Momordica*, all fowarded from Zanzibar, by Dr. Kirk, F.L.S., for the Kew Museum.—Before proceeding to the regular business of the Society, the president again read, and explained the purport of, the alterations in the Bye-laws agreed to by the Council. The proposed alterations were adopted by the Society. The following papers were then read, viz. :—On some Species of Japanese Marine Shells and Fishes which inhabit also the North Atlantic, by J. Gwyn Jeffreys, F.R.S. The mollusca noticed by the author were procured by Captain St. John in H.M.S. *Sylvia*, during the years 1871 and 1872, on the coasts of North Japan. His dredgings varied betweeen 3 and 100 fathoms. After passing in review the works of naturalists who had described the marine shells of Japan, and especially the "Mollusca Japonica" by Dr. Lischke, with reference to those species which are common to Japan and Europe, Mr. Jeffreys proposed to record from Captain St. John's dredgings thirty-nine species, and to give the range of depth for such of them as he had obtained in the *Porcupine* expeditions of 1869 and 1870. He then offered an explanation of the occurrence of the same species in the Atlantic and Pacific Oceans, by suggesting that it was probably owing to involuntary transport by tides and currents, and not to voluntary migration. Very little is known about the direction and force of deep sea currents; but high northern species might be transported on the one side to Japan, and on the other to Europe, by a bifurcation of the great Arctic current, which has been traced as far south as the Straits of Gibraltar in the course of the *Porcupine* expeditions. The entry of northern species into the Mediterranean may be accounted for by the former existence of a wide channel, or rather an open sea between the lower part of the Bay of Biscay and the Gulf of Lyons, which has been satisfactorily proved, on geological grounds, to have been formed since the Tertiary epoch. A list of the mollusca referred to in the paper was given, with critical remarks, as well as a list of twenty-two species of fish which Dr. Gunther communicated as common to the Japanese Seas and the North Atlantic or Mediterranean.—Dr. Carpenter, F.R.S., made some general remarks on Ocean-currents, especially with reference to the zones of temperature in the North and South Atlantic. He stated that it has been ascertained that water of 40° F. comes nearer to the surface in the equatorial regions than in the north and south temperate zones. There are, he believes, zones of all temperatures in all deep seas, such as that of 33° F. observed by Capt. St. John between Socotra and the Seychelles. He hoped that Capt. St. John would, in his future expeditions, be able to obtain a very valuable series of observations of deep-sea temperatures. Dr. G. J. Allman, F.R.S., said the specimens all belonged to types hitherto considered extinct; and he entered into some description of one of the most remarkable forms.—Note on Japanese Brachiopoda, by Thomas Davidson, F.R.S., communicated by J. Gwyn Jeffreys, F.R.S.

Chemical Society, Jan. 15.—Prof. Odling, F.R.S., president, in the chair. Mr. W. C. Roberts handed in a table supplementary to his paper read at the last meeting, and containing complete analyses of all the standard trial plates still extant, dating from A.D. 1477, namely, 17 gold plates and 14 silver ones.—The first paper was "On the action of trichloracetyl chloride on Amines: I. Action on aniline," by Dr. D. Tommasi and Mr. R. Meldola. This reaction gives rise to a substance called *phenyl-triacetamide*, which crystallises in lustrous plates. It is acted on by nitric acid with production of *dinitro-phenyl-triacetamide* crystallising in yellow needles.—Dr. H. E. Armstrong read a note "On the action of sodic ethylate on ethylic oxalate and other ethereal salts."—"On the products of decomposition of castor oil: I. Sebacid acid," by Mr. E. Neison. An account of the preparation and properties of pure sebacic acid, and of many of its salts.

Geologists' Association, Jan. 2.—Henry Woodward, F.R.S., president, in the chair.—On the nature and formation of Flint and allied bodies, by Mr. M Hawkins Johnson. The object of the paper was to show the nature of several members of a large group of bodies occurring in sedimentary deposits of different ages, and which are generally known as nodules, and described as concretionary. Those specially alluded to were the septaria from the London and Kimmeridge clays, the flints from the chalk, the iron pyrites from the chalk, the phosphatic nodules from the gault, the clay ironstone nodules from the carboniferous series, and the ironstone from the Woolwich beds. By the gentle action of solvents the structure of these bodies is revealed so as to be easily examined by the microscope. They are thus found all to agree in possessing a silicified organic structure, which may be described as a net-work of fibres or a mass permeated in every direction by anastomosing canals. This structure was subsequently filled in with other material, such as carbonate of lime, silica, bisulphide of iron, phosphate of lime, carbonate of iron, &c., the particular substance thus filled in depending upon the relative abundance of the substance dissolved in the interstitial water of the surrounding matrix. The singular group of concentric siliceous circular bands seen upon many fossils, and known as orbicular silica, or Beekite markings, were also explained. The fossils on which they occur are imbedded in a matrix more porous than themselves and of irregular constitution, so that the evaporation, to which the consolidation of the dissolved silica in their pores was mainly due, occurred at a number of points on the surface of the fossil, at which points a deposit of silica took place forming the central tubercles. The cessation of evaporation was followed by a fresh saturation, with the solution to be again evaporated. But as the evaporating points were now plugged up by the previous deposits, the silica last consolidated was deposited around their margin and upon them internally appearing outwardly as a ring round the tubercles. Alternations of these conditions account for the numerous bands seen in some of the groups.

Anthropological Institute, January 13.—Prof. Busk, F.R.S., president, in the chair.—A paper, by Mr. S. E Peal, was read, on the "Nagas and neighbouring tribes." The tract of country occupied by the Nagas lies mainly between lat. 25° N. to 27° 30′ N., and long. 93° 30′ E. to 96° E. It is bounded on the east by the country of the Singphus or Tsingpos, a distinct race showing strongly-marked differences in language, physique, and customs; on the north by Assam; and on the west are various other tribes, while to the south the boundary is undefined. The inhabitants of the tract, although all termed Nagas, are divided and sub-divided to so great an extent that few parts of the world can present such a minute segregation of innumerable and independent tribes. A common and conspicuous feature of the Nagas, Garos, Kukis, Lushais, and other hill-races of that district, is their custom of taking human heads—either by regular warfare, raids, or casual surprises. Not only is the custom general among them, but it has obviously existed for a long period of time, and, in its present phase, is the true cause of the strongly-marked variations both in language and physique that exist among the Naga tribes, no two of whom are really alike. An almost necessary consequence of this mode of life is that they are a fine, hardy, active race, excelling in all that relates to forest lore and labour, while, on the other hand, they are conspicuously deficient in the arts of pottery, working in metals, and writing. The most singular feature is, perhaps, the almost total absence of agricultural implements; everything is done with the Daú, or P-shaped axe. The mental capacity of the Nagas is low, although they exhibit smartness or cunning in matters relating to ordinary life; no individual known to the author was able to count beyond ten.—Mr. C. B. Clarke contributed a paper on the "Stone Monuments of the Khasi Hills." The Khasi Hills form a plateau, at a mean elevation of 4,500 feet above the sea, between the plains of Assam on the north, and Sylhet on the south, and are inhabited by a people quite distinct from the neighbouring Hindus. The stones, which are profusely scattered over the range of hills are of three kinds: the funeral pyres, the kists containing the jars of ashes, and the monumental groups. One great feature of the Khasi sepulture is, that the ashes of the family are collected from time to time. At first the ashes of a man are kept in a small kist, then, after a few years, a great funeral ceremony is held, and the ashes of the various individuals of the family are collected from the smaller kists. The ashes of all the men are collected into one earthen jar, those of the women into another, and these two jars are placed in one large kist; the jar of the women's ashes is placed next the last stone closed, for the reason that among the Khasi the woman is always mistress of the house.

Riga

Society of Naturalists, Aug. 27—Dr. Buhse presented some growing specimens of *Elodea Canadensis*, the American aquatic weed which, since 1836, has appeared and spread over Middle and Northern Europe; and gave some account of it. Two different analyses of the weed, by Fischer and Liebig, show a large proportion of salts in its ashes, and also how widely the proportion of its constituents varies with the nature of the water in which the plant grows.

Sept. 17—M. Schweder directed attention to a fossil egg now in the St. Petersburg Museum. It was found in the Chersonese Government a few years ago, and has been secured for 1,000 roubles (say 166*l*.). It is 18 cm. length, and 15 cm. short diameter; its capacity is reckoned equal to that of 40 to 44 hen's eggs. It is thus larger than an ostrich egg, but much smaller than the egg of the Epiornis, which is equal to 148 hen's eggs.—Various plants and other specimens were presented.

VIENNA

Imperial Academy of Sciences, Nov. 6.—Prof. Mach presented a paper on physical experiments as to the sense of equilibrium in man. From experiments on himself, he is led to think that Flourens' turning phenomena, the orientation of equilibrium and of motion, the phenomena of giddiness, certain optical movements, &c., may be explained by supposing that the nerves of the ampullæ of the semicircular canals respond to every stimulus (which commonly involves a turning of the contents of the canal), with a sensation of turning.—Dr. Boué gave results of 33 years' observations on the circumstances attracting lightning strokes. He points out that the lightning often strikes low objects, though higher may be nearer; and he considers that constancy of course, in thunder clouds (from presence of mountain chains, &c.) and repeated discharges at particular points, may afford an explanation, in the superior attraction, viz. of subterranean masses of metal in certain regions.—Prof. Niemtschik made a communication on the construction of an ellipse inscribed in a circle, centre and tangent being given.

Nov. 13.—Prof. Pfaundler described three forms of apparatus he had devised for showing the composition of vibrations occurring at right angles to each other.—M. Stefan gave a paper on evaporation, examining theoretically the experiments lately described. From the formulæ of his dynamical theory of gases, he calculates the mean courses of the vapour-molecules of ether and sulphuretted hydrogen from one collision to the next; these are 23 and 32 respectively, the millionth part of a millimetre being taken as unit; also, from these, the diameter of the molecules; which are 0·9 and 0·7.

Nov. 20.—M. Puschl presented a paper on the co-motion (*Mitbewegung*) of light in moved media. He states the following conclusions: (1) Through participation of the ponderable atoms in propagation of light, the latter may, in various bodies, be more or less retarded, but in no case is it considerably accelerated. (2) The specific refractive power of a body is connected with the substance of its atoms, and independent of its density, so long as the internal nature of the atom substance remains the same. (3) The internal nature of atoms is modifiable through external pressure, crystallisation, solution, mixture, and especially chemical action.* (4) The ether waves sent out from substances themselves are not produced immediately through the motions of the atoms as a whole, but mediately, through corresponding disturbances and concussions of the atom substance, which vibrates in the periods natural to it, according to the specific elasticity and the dimensions of the atoms.—Prof. Luess read a paper on the earthquakes of Southern Italy. He specified some points, in Sicily, and neighbouring islands, from which shocks spread radially in various directions (Etna, however, not being one of these centres); in other cases the earthquakes seemed to take a quite irregular course.—Dr. Weiss made some observations tending to identify the comet lately discovered by Coggia and Winnecke with Comet 1818 I.—M. Payer presented some fossils brought by the Weyprecht expedition from Spitzbergen.

BOSTON, U.S.

Natural History Society, Nov. 19, 1873.—Mr. F. W. Putman gave an account of the anatomy of *Bdellostoma*, and compared it with that of *Myxine* (known as hag-fish), illustrating his remarks with series of dissections, showing the brain, skeleton, intestine, ovary, liver, heart, branchial sacs, &c. These two genera of fishes form the family of *Myxinidæ*, and have similar habits and a very close external resemblance, although they can be readily distinguished by the number and position of the branchial outlets, and by the position of the œsophageal duct. Mr. Putnam said that his dissections, though in great part repetitions of those of Muller, made over thirty years ago, showed conclusively the natural separation of the genera by their internal structure.—Mr. L. S. Burbank read a paper on the "Surface Geology of North Carolina," with especial reference to some phenomena of Northern drift. From the facts noted the following inferences may be drawn:—(1) The time which has elapsed since the close of the drift period must be very short compared with the previous ages, during which the solid ledges were disintegrated by chemical and atmospheric agencies. 2) Boulders of the drift do not, in general, owe their rounded forms to attrition by glacial action, but, while still in place, assumed these forms by disintegration and exfoliation. (3) Whatever the force or agency of the drift may have been, it did not produce the great mass of the drift material by mechanical action in wearing and grinding down the solid rocks, but has merely carried forward and commingled the materials already disintegrated.—The secretary read an extract from a letter dated St. John's, Newfoundland, Nov. 10, 1873, from Mr. Alexander Murray, the geologist of Newfoundland, to Professor Jules Marcou, giving an account of a remarkable marine monster, which recently made its appearance off the shores of that island, and of a severed arm or tentacle of the same in his possession. The tentacle measured on October 31, having then been several days in strong brine and shrunk in consequence, seventeen feet, but was said to have measured nineteen feet previously.

PARIS

Academy of Sciences, Jan. 12.—M. Bertrand in the chair.—The following papers were read:—Tables of Jupiter, by M. U. J. Leverrier. The author finds that the influence of all the small planets on Jupiter is inappreciable.—Third memoir on chemical dynamics, by M. Becquerel. This paper dealt with the action of water in chemical combinations and with the effects of water and other liquids acting as electrodes.—On the distribution of magnetism in soft iron, by M. Jamin.—On the heat set free by the combination of nitrogen with oxygen, by M. Berthelot.—On the osteology of the anterior limbs of the *Ornithorhynchus*, &c., as compared with that of the corresponding members of reptiles, birds, and mammalia, by M. Ch. Martins.—On the problem of three bodies, by M. F. Siacci.—Studies on diffraction, by M. A. Cornu. The author gave a method for the geometrical discussion of diffraction problems.—On the physiology of the flight of birds in relation to the action of the wing on the air, by M. Marey.—Organogenesis compared with androgenesis (*l'androcée*) in its relation with natural affinities (class of *Cruciferæ*), by M. Ad. Chatin.—On the transformation of the vibroscope into a tonometer, and on its use for determining the absolute number of vibrations, by M. A. Terquem.—On chloral and its combinations with albuminous substances, by M. J. Personne.—On an acoustic pyrometer, by M. J. Chautard. This instrument depends on the variation of wave-length of a sonorous wave when the vibrating air is heated.—On a re-agent paper for detecting urea, by M. Musculus.—On the formation of gum in fruit trees, by M. Ed. Prillieux.—Researches on the glands of *Rosa rubiginosa* and on their contents, by M. R. Guérin.—On the geometrical properties of rational fractions, by M. F. Lucas.—On theorems of indeterminate analysis, by Father Pepin.—On the action of definite ternary systems compounded of mannite, borax, and water on polarised light, by M. L. Vignon.—On the artificial production of crystals of calcic oxalate resembling those produced by plants, by M. Vesque. The method consisted in causing solutions of potassic oxalate and calcic chloride to mingle very slowly in a third neutral liquid by causing them to flow through strips of blotting paper, or one solution and the neutral liquid were mixed and the other introduced in the same way, or the solutions were diffused into each other through a dialyser.—Notes on the storms of the year 1869, by M. Fron.

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ERRATUM.—Vol. ix. p. 198, 1st col. line 35, for "18 degrees" read "180 degrees."

THURSDAY, JANUARY 29, 1874

THE DUTY OF ELECTORS

IN his address to his Greenwich constituents Mr. Gladstone has undoubtedly attacked a weak point of human nature, by his announcement of a large balance, the promise of the removal of the income-tax, and other reductions in taxation; but those who really have the welfare of their country at heart cannot help feeling that, by making one doubtful good all-prominent, he has placed too far in the background many of those points which are daily becoming of more and more importance to the national welfare. Our country depends for its high position among nations, not only on its resources in coal and iron, but also, and more securely, on the mental capacity of its people, whose peculiarity is that they have the power of always using the resources at their disposal to better advantage than any others. We in nearly all cases have taken the lead in invention. A discovery, for instance, is made by which the amount of coal required to produce a certain amount of useful work is diminished greatly; this is adopted by others of ourselves, and is gradually spread to other countries, not however before sufficient time has elapsed to place us on the way to another method, which will have as great advantages over the new one as that had over the one which preceded it. There are those amongst us who, with our national tendency to depreciate our own abilities and resources, carefully compile statistics show that our gradual decline and destruction are inevitable in a certain definite number of years. They, however, inevitably leave out of the question the potentiality, as it may be termed, of the British brain.

But how is it that we are able to maintain this high position of progressive discovery? This is a question which may be well asked. The answer is not difficult to find. It is on account of the thoroughness of the work done by some of the scientific members of the community, who but too often use their best efforts, involuntarily though it may be, to the working out of those inductions which lead to the discovery of new methods that prove so invaluable to their fellow-creatures, and so often unremunerative to themselves. There are many who must feel that it is not too early to make it a part of all Government legislation, that more stress be laid on and more direct pecuniary assistance be given to such unremunerative scientific work, and encouragement offered for the production of it in greater quantity, so as to secure more of its invaluable results.

It may be said, and it is said by some, that we have gone on very well as yet without any great encouragement in this direction; but this is the old argument over again; we are now in a very different position to what we were formerly. When Science was in its infancy, it was not so essential to the production of good work that those who made the greatest strides should know much of the investigations of those who preceded them, nor of the principles of the sciences, by the employment of which alone they can expect to advance. But the last half century has been so prolific in scientific method and detail, that any one ignorant of all its branches, however great his ability, can have little or no chance of making fresh improvements or discoveries. What was not essential formerly is essential now; and just as the standard of general education is much higher at the present time than it was some years ago, so must the scientific education be.

But there is only one method of improving scientific education satisfactorily, and that is by making scientific work more possible and lucrative. A young man does not commence physics or chemistry or biology until he has really begun the battle of life; his mind is scarcely fit for it before; he must therefore, when he takes them up, see clearly a livelihood ahead. Such a livelihood at present is little more than a phantom. The prospect of a post in any Government institution, such as the British Museum, for example, is, to say the most, to scarcely a pittance, and there are many of the best workers who would undergo many privations rather than have to devote the greater part of their lives to the drudgery of an educational appointment. Most scientific men do not expect to become rich on their avocation; the inherent pleasure of their subject compensates to a certain extent for the diminished income; but they must live, and living means more than obtaining an income which is insufficient to allow of their maintaining the social position to which they are born, or to which their education has brought them.

Such being the case, ought not the nation in a fresh Parliamentary election to lay some stress on the improvements that are indispensable for the healthy progress of scientific thought? Why does Mr. Gladstone's interest in the higher Education begin and end with Ireland—can it have any reference to party questions? Why is there all too slight a reference to the University question and no reference at all to the Report of the Royal Commission which has recently been issued—is it because Mr. Gladstone knows that there are many Conservatives much more liberal and large-minded than the Liberals themselves on this subject?

Is there no feeling throughout the country on the subject of Museums, or the ever-growing necessity for a Minister of Education? Mr. Gladstone may well be excused from referring to these topics in his "prolix" manifesto, but are all the Constituencies to neglect them? Is the Sectarian or the Licensed-Victualler to be the only man who shall require his candidate to render a reason—to state his views? Our point is, that every voter in the kingdom has now an opportunity of helping on the cause of Science and Education by insisting upon his representative having ideas—and right ideas—on these questions.

Why should there not, among the numerous influential scientific societies which are spread through the country, be formed organised committees whose duty shall be to use their influence in representing their requirements to the candidates for parliamentary election, and doing all in their power to get their wants respected and complied with? Again, why should not those bodies, like the University of London, with a large number of scientific voters, and a representative, do all in their power to return for their member one who has the interests of Science and the higher education at heart, and who will do all he can to put these interests in the best light? That such will not be done by the University of London at least, will be evident if Mr. Lowe is again returned as their member at the

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coming election; for his principles of action are understood; his views with regard to the Universities and the higher teaching generally are known; and his unwillingness, even to consider the desirability of raising the salaries of the scientific officers of the British Museum to the level of those of ordinary Government officials is before the world. There is no doubt that he has forfeited all claim, to the support either of the Scientific or the Medical Graduates of the important Corporation which he represents.

PHYSICAL GEOGRAPHY

Physical Geography, in its Relation to the Prevailing Winds and Currents. By John Knox Laughton, M.A., F.R.A.S., Mathematical and Naval Instructor at the Royal Naval College. Second Ed. (J. D. Potter, 1873.)

The Ocean: its Tides and Currents, and their Causes. By William Leighton Jordan. (Longmans, 1873.)

THE first part of Mr. Laughton's work consists of a comprehensive and valuable summary of the present state of our knowledge of the prevailing Winds in different parts of the globe; on the basis of which he proceeds to examine into the commonly-received theory of Atmospheric Circulation, and pronounces that "it describes the phenomena which do not exist, and misrepresents, or does not account for, phenomena which do." The second part treats of the Currents of the Ocean; and these (following Major Rennell) he attributes for the most part to the winds prevailing in the localities in which they originate, their effects being variously modified by coast-lines, the meeting of other currents, &c. We believe that he is quite justified in upholding this general doctrine, and in repudiating the notion of Captain Maury that differences of temperature, excess of evaporation, &c., can sustain the Gulf Stream or any great oceanic current. But he rides his hobby a great deal too hard, when he affirms that under no circumstances can these agencies produce currents; going so far as to attribute the in-current of the Strait of Gibraltar to the *vis-à-tergo* of the Gulf Stream. As well might he attribute to it the constant current which sets over the bar of the Karaboghaz or Black Gulf on the eastern side of the Caspian, and carries (according to the computation of Von Bar) 350,000 tons of salt a day into this great natural salt-pan, the water (which the natives fancy must have some subterranean outlet) being all got rid of by evaporation. According to Sir John Herschel's computations, the excess of evaporation from the Mediterranean area, over the return of water by rain, would require *twelve Niles* to supply it; and as there is only *one* Nile, and as Captain Wharton's recent researches in the Dardanelles show that the Black Sea sends very little of its river-water into the Mediterranean (the supply poured in by the Danube, the Don, the Dnieper, and the Dniester, being very little more than sufficient to make up for the evaporation of the Black Sea itself), it is obvious that an enormous deficiency must exist, after every allowance has been made for the Rhone, the Ebro, and the Po, which are the only considerable rivers, beside the Nile, that pour their waters direct into the Mediterranean basin.

Mr. Laughton does not seem to have made himself as well acquainted as a Government naval instructor might have been expected to be, with recent contributions to Oceanic Hydrography. Thus he repeats the statement of his first edition, that the Gulf Stream rushes through the Florida Channel at a rate varying from 80 to 120 miles a day; whereas the Admiralty Pilot Chart, based on the most trustworthy information, makes the annual average only 48 miles per day. He does not deign to notice the arguments adduced by Dr. Carpenter in his last report to the Royal Society, which have satisfied many eminent authorities that the amelioration of the climate of North Western Europe is due, not to the *true* Gulf Stream or Florida Current, but to a slow north-easterly movement of warm water sustained by thermal influences alone. He repeats (p. 200) the old fallacy that the cold of the ocean-bottom is "due to the great depth, to the impermeability of water by the sun's rays;" as if this had not been disproved by the fact, that the bottom-temperature of the Mediterranean, at depths ranging to 2,000 fathoms, is from 54° to 56°, whilst that of the Atlantic at similar depths and under the same parallel is *twenty degrees lower*. And in p. 250 he makes the astounding statement that "the gradual closing up of the channels [through which the Gulf Stream flows], by the ceaseless work of the polypes, has, *by diminishing the outlet, increased the force of the stream;*" which is tantamount to saying that the stream of water which issues from a fire-engine has a greater force than that which works its pumps! If we had only to narrow an outlet to create force, we need not be afraid of the exhaustion of our coal.

We recommend Mr. Laughton, before he issues another edition of his book, to dismiss from his mind, if he can, all prejudice in favour of his particular theory, and to open his mind more fully to the evidence of a *vertical* Oceanic circulation, which he already partly admits, and which is not in the least inconsistent with his fundamental principle (in which we entirely accord) of the maintenance of the *horizontal* circulation of the great Ocean-basins by the movements of the atmosphere.

The title of Mr. Jordan's book is very misleading; for, although professing to treat of the tides and currents of the Ocean, he devotes the greater part of his 344 octavo pages to an exposition of what may be called the Jordanian (in opposition to the Newtonian) system of Astronomy. This system is based on the doctrine of *inertion*, by which Mr. J. means the inherent tendency of all motion to come to an end. The only motor force he admits is that of gravitation; and he considers himself to have proved that the revolutions of the planets round the sun are due to the opposition between solar gravitation and astral gravitation, "so that, in their courses, they are borne smoothly along the lines of equilibrium lying between opposing forces of gravitation." He also maintains that "the rotation of a sphere tends to cause surrounding bodies to revolve around it;" and that, in this manner, the rotation of the earth from west to east tends to carry the moon in the same direction, its "lagging behind" beng due to "astral gravitation."

The application of Mr. Jordan's theory of inertion to the movements of the ocean is very obvious. Reasoning upon the fact that when a vessel containing water is made to rotate, "the water tends to maintain its position, and therefore has a relative motion over the surface of the vessel in the opposite direction to that in which the vessel is moved," Mr. Jordan supposes that this will *always* be so; and that the tendency of ocean-water to be left behind is the great source of tides and currents. But if he will try the experiment of *continued* rotation, especially with a vessel havng not a smooth but an irregular interior, he will find that *after a time* the water rotates as fast as the vessel itself, and partakes of its momentum. Were it otherwise in the case of the Earth, no rock could withstand the abrading power of the mass of water which would be constantly impelled against its eastern face—not only on the surface, as in the case of the trade-wind current, but at its greatest depths. That Ocean-water not changing its place northwards or southwards, *does* fully partake of the Earth's motion, and *does not* tend to lag behind, is proved by the exceptional cases in which a flow of water moving towards either Pole tends *eastwards* in virtue of its *excess* of easterly momentum, and in which a flow moving towards the Equator tends *westwards* in virtue of its *deficiency* in easterly momentum. The *Challenger* temperature-sections of the Atlantic show this to be the case with the cold-stratum beneath the Gulf Stream, which comes to the surface along the Atlantic sea-board of the United States; a similar "cold wall" has been found by our Naval Surveyor, Capt. St. John, to intervene between the Kuro Siwo (which is the counterpart of the Gulf Stream in the Pacific) and the eastern coast of Japan; and the recent researches of Dr. Meyer have shown that even in the North Sea a like upward movement of the colder under-stratum is distinctly traceable along the eastern shores of Britain, and still more on the eastern slope of the Dogger Bank.

It would be quite useless to either follow Mr. Jordan through his detailed application of a theory which is so completely baseless, or to examine into the validity of his criticisms of the views of others. He is obviously a man possessed, like the notorious upholder of the earth's flatness, by a "dominant idea" which nothing will dispel; and all we can do is to warn our readers that his book is good for nothing, except as a warning example of misdirected ingenuity.

ANIMAL MECHANICS

Principles of Animal Mechanics. By the Rev. S. Haughton, F.R.S. (Longmans, Green and Co.)

THIS formidable volume has four languages on the very title-page, and bristles throughout with numerical calculations, analytical formulæ, and geometrical constructions. When, in addition, we record that it contains anatomical details, teleological postulates, hints on the best mode of hanging, &c., it will be obvious that no one man can be expected to be able to pronounce upon its value from more than a few of the possible points of view.

We are told in the Preface that the object of the work is to show "the mutual advantages obtainable by anatomists and geometers from a combination of the sciences which they cultivate. Anatomists will gain by the increased precision which numerical statements must give to their observations, and geometers will find in anatomy a new field of problems opened out to their investigation." Surely there is nothing new in this statement! Every anatomist worthy of the name strives after the greatest attainable precision in those observations in which it is requisite, and many able mathematicians have treated of anatomical problems. But passing this over, we are obliged to say that Dr. Haughton's mathematics are barely such as are calculated to attract the anatomist. When writing for a class of persons who, at the best, rarely know more than the merest elements of mathematics—surely it would be well to use the simplest processes which will suffice. This is not Dr. Haughton's method; he rather acts on the principle of making an investigation as showy as possible by the introduction of an immense quantity of quite superfluous analysis. This is, no doubt, calculated to impress the majority of readers with an idea of the author's profundity; and, though even very ordinary mathematicians will find no reason to share this impression, we cannot understand the necessity for putting such a threatening barrier in the way of the poor anatomist who wishes to understand the reasons here assigned why muscles have the particular forms which it is part of his business to examine, describe, and classify.

Excellent instances of this peculiarity of the work may be given in great numbers, but one must suffice. Take the investigation in p. 239, which is given to prove that no work is done by a quadrilateral muscle when one of the bones acted on revolves about a certain given point. The result given in the text follows instantly from the most elementary geometry, if a single additional line be inserted in the woodcut; always, however, providing that the reader is prepared to allow the following postulates, which may, perhaps, not be very readily assented to, but which are as necessary for the elementary geometry as for the pompous analysis. The first is, that when muscular fibres are extended, as much *negative* work is done by them as there is done of positive work when they contract by the same amount! The second is, that in a plane quadrilateral muscle the fibres run in lines which, if produced, would all meet in the intersection of the lines joining the ends of their places of attachment to the bones, and that they are *uniformly* distributed radially from this point. Postulates of this kind are, indeed, very common throughout the work.

The three great features of novelty in the work, so far as we can perceive—in addition to the very numerous, and obviously careful, determinations of the weights, &c., of corresponding muscles in various beasts, birds, and fishes—are the *Law of Fatigue*, a grand teleological *Postulate*, and the *Principle of Least Action*.

These are enunciated as follows, the third as applied to the heart:—

> "When the same muscle (or group of muscles) is kept in constant action until fatigue sets in, the total work done, multiplied by the rate of work, is constant."
>
> "The Framer of the Universe (Δημιουργός τοῦ κοσμοῦ) has constructed all muscles upon the principle that each shall perform the maximum of Work possible for it under the given external conditions."
>
> "The arrangement of the fibres of the heart must be such as to allow each fibre to contract to the fullest extent required by the law of muscular contraction."

As a simple comment on the first of these, rendered very instructive by the insight it affords us into the general cogency of the author's reasoning, take the following:—

"If any man wishes for a simple proof of the inferiority of the endurance of his muscles as compared with those of a woman, let him carry a child on his arm for the same time that his wife or nurse can do (*sic*) with ease, and he will find himself much fatigued."

In this striking passage, for "a woman" read "another man;" for "wife or nurse" read "coalheaver;" for "child" read "sack of coals;" and for "arm" "back." It is still obviously true! Many other, perhaps even more remarkable, forms of this statement will present themselves to the intelligent reader.

We had marked for comment or quotation a great number of passages, but considering the characteristic qualities of the specimens we have given, we think the reader who wishes more may safely be left to seek them in the work itself. Perhaps the most curious point we have not yet alluded to is the author's calculation of the force which can be exerted by the abdominal muscles. In his first publication of this astonishing result he adopted *seriously* a quotation from Sterne which Duncan, writing on the forces employed in parturition, had used (in its original intention) as a mere joke; and in the work before us, in spite of all that has been done, especially by Duncan and Schatz, since that first publication of his, Dr. Haughton still gravely writes:—

"Thus, we see that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner."

It is only necessary to explain that this is assigned not as the whole pressure on the *surface* of the child, but merely as the component in the direction of its motion!

POLAR EXPLORATION

The Gateway to the Polynia: A Voyage to Spitzbergen. From the Journal of John C. Wells, R.N. With numerous Illustrations. (London, 1873. 8vo, pp. 355.)

EVERY fresh book on the Arctic Regions helps to awaken the dormant interest of the public in the question of Polar Exploration, and from that point of view this volume commands our attention. From no other, however, can we recommend it. The "rapid sketch of Arctic voyages" contained in the introductory chapter is rapid indeed—we might also add vapid—and it is followed by a disquisition on things in general in which some of the statements are true and a few of them new, though the new and the true do not seem to be always successfully combined. Of course it was not to be expected that the masterly summary of the progress of northern discovery given by Richardson in his well-known "Polar Regions" should be excelled or equalled, but we had a right, we conceive, to look for a few more details than we get of the American, German, and especially of the Swedish expeditions executed since Sir John's work appeared. But even letting that pass, we should have been contented with a plain narrative of Capt. Wells's own "Voyage," whereas we have nothing of the sort. We are told, it is true, that he sailed in the yacht *Sampson*, that he left this country in May 1872, and, after reaching lat. 80½°, returned in the following September—facts which any of our readers may find if they take the trouble of looking back into our columns; but of the incidents and results of the voyage we are afforded only the most vague outline, drawn in a confused and book-making way. One remarkable and suggestive fact is to be noticed. The name of the owner of the yacht never appears in regard to this voyage! Little bits of what may once have been written in a journal pieced together with stories more or less (and rather less than more) connected with the subject, such as that of the building of Scalloway Castle (imperfectly told by the way)—yarns spun by old whalers and sailors—scraps of zoology, botany, geography, and meteorology (some of them incorrect)—long extracts from Parry's well-known "Voyage"—the whole jumbled into one chaotic mass, from which it is difficult to derive any clear knowledge of what belongs to the writer of the "Journal," and what has been drawn from other authors. We are treated to certain woodcuts, the like of which were the wonder of our childhood, such as that of the Right Whale (p. 64); but whether Captain Wells saw a Right Whale, or knows one when he sees it, we don't profess to say, and this particular portrait is simply named "Whale." The cut representing "Whales' Food" (p. 82) is altogether wrong; whales would fare badly if they only swallowed such nourishment as the *Hydrozoa* there figured, and the author might have learnt better from old Friderich Martens, two hundred years ago. Shetlanders are said (p. 71) to be a "branch of the Celtic family." The Reindeer figured (to face p. 223) were certainly not drawn from Spitzbergen examples, and most parts of the book indeed might almost just as well have been written by a man who had never been to that country.

But perhaps all this may be looked upon as trifling. Capt. Wells's great object is to urge the claims of the Spitzbergen over the Smith's Sound route for future Arctic discovery. On this question much has been written and spoken; and though the opinion of experts is overwhelmingly strong in favour of the latter, the former is not to be dismissed in the off-hand way in which it frequently is. Impossible as it may seem, we wish to reconcile the adherents of either creed. Capt. Wells, we think, is not the ablest of advocates. He omits putting the point as strongly as it ought to be put, indeed his theory is utterly opposed to it. In his map all the space encircled by the 85th parallel is marked "Polynia," and an arrow-head obligingly informs us that "the gateway to the Pole" lies in long. 10° E. To force this gateway by steamer would seem to be his advice. Now we cannot agree with him here, for the idea of a great extent of perpetually open water, which is the essence of the notion of a *Polynia*, is a mere assumption, against which much seems to militate. Now there are two entirely different things for the Arctic discoverer to do. If his object be merely to reach the Pole by the cheapest and easiest means, our belief is that there is no way better than the Spitzbergen route, but one cannot expect to do it by water. The expedition should winter in the north of Spitzbergen, or on one of the outlying islets, and sledge-parties should be sent in early spring over the ice to reach the goal, and return with all possible speed. But if the object be to make a really satisfactory exploration, then the almost perfect agreement of Arctic authorities declares for Smith's Sound. It is possible that the Spitzbergen route might be accomplished by private enterprise, but for the other a Government expedition is essential. On parting with Captain Wells, we are glad to find we agree with him on one point :—"We want a new motive to rouse up the spirit of the nation and Government ; and what higher and nobler one can be found than the search for truth and the advancement of Science? This is the duty of a Government, to promote the national welfare ; and one of the surest ways in which this can be done is by encouraging scientific efforts. . . . There are few ways in which this spirit can be better fostered than by Polar exploration ; and so popular is such service amongst our sailors, more especially Arctic sailors, that hundreds of them volunteer to go when any project of this kind is afloat. From this point of view, the exploration of the higher latitudes is a matter for Government, and not for private enterprise" (pp. 2, 3).

OUR BOOK SHELF

Perils in the Polar Seas. True Stories of Arctic Adventure and Discovery : A Book for the Young. By Mrs. Chisholm, authoress of "Rana; or, the Story of a Frog," &c. (London : John Murray, 1874.)

THIS is one of the best books of the kind we have met with. It is written for the young, but Mrs. Chisholm has wisely made no attempt to "write down" to the supposed mean capacity of the little folks ; she tells her intensely interesting story in simple, unaffected, clear, forcible English. Indeed, were it not for the occasional interruptive questions and remarks of the group of youngsters to whom the authoress is supposed to be telling her story, one would naturally fancy that the book, like "Gulliver's Travels" and "Robinson Crusoe" had been written for all who can understand plain English. Mrs. Chisholm, in her opening chapter, "Life with the Esquimaux," gives many details concerning the habits of that people, taken mainly from the late unfortunate Captain Hall's account of his residence among them. After another brief chapter on "North-East Voyages," she enters upon the history of Arctic discovery on the American side, and with the greatest care and clearness, tells what the principal explorers, from Frobisher down to Hall, have done to make known to us the outline of the lands and seas of those mysterious northern regions. In doing so the authoress's object is something more than merely to fascinate and thrill her readers by a narrative of strange adventures by flood and field ; while there is no apparent attempt at making the story a vehicle for conveying useful information, yet Mrs. Chisholm manages to convey, in an impressive manner, a great amount of knowledge of the geography, natural history, and meteorology of the Polar Regions. Indeed it would be difficult to devise a better method than is here followed, with the assistance of two excellent maps, of teaching the geography of Arctic America. As might be expected, the greater part of the book is occupied with modern voyages, mainly those of Parry, the Rosses, Franklin, and the Franklin Search parties. "Uncle George" gives a good deal of information concerning the whale-fishery, and also an account of Parry's boat voyage to the north of Spitzbergen. Besides the two maps already referred to, the volume contains many beautiful illustrations. Perhaps it was scarcely necessary to make the children interrupt the story-teller so frequently with their questions ; indeed the story is so attractively told that such diversions are sometimes irritating. But this is a small matter ; the work as a whole is capitally done, thoroughly interesting, healthy, and full of information.

Historische Fragen mit Hulfe der Naturwissenschaften beantwortet, von Dr. Karl Ernst v. Baer.

Studien aus dem Gebiete der Naturwissenschaften, von Dr. K. E. v. Baer, Part II., Sec. 1. (St. Petersburg, 1873.)

THE "Historic Questions," just published by this eminent naturalist, aim at solving by evidence from natural history certain disputed traditions which have puzzled historical critics. The first subject remarked on is the "swan's song," which seems so fanciful a myth to western nations accustomed only to the songless swan, which the Russians call *shipûn*, the "hisser," but not to the other swan, which they name *klikûn*, the "caller," whose melancholy notes are so often heard by travellers in North-East Europe and North Asia ; it is stated on no less authority than that of Pallas, that the swans utter these tones when mortally wounded. Next follows an examination of the voyages of Odysseus, made with the view of ascertaining how much of ancient geography is embodied in the Homeric narrative. According to Dr. v. Baer's map, several localities of the ideal voyage are to be traced in the Black Sea, at whose entrance are Skylla and Charybdis and the Symplegades, while the Læstrygonians dwelt in the Krimea, and Kimmerian darkness began at the opening into the Sea of Azof. Lastly, the locality of the Biblical Ophir is discussed ; Dr. v. Baer finds it in the Peninsula of Malacca.

In the collected "Studies" we find a German version of a paper dating from 1848, on the Influence of External Nature on the Social Relations of Races. The next is dated Berlin, 1866, on Purpose in the Processes of Nature, in which he gives the name of *teleophoby* to the fear he observes among some naturalists of recognising an object or purpose in Nature. Dr. v. Baer's doctrine is summed up in a passage reproduced, with slight alteration, from his own writings 33 years ago : "Thus the earth is but the seed-bed in which the spiritual inheritance of man increases, and the history of Nature is but the history of continuous victory of Spirit over Matter. This is the fundamental idea of Creation, for the satisfaction or rather for the attainment of which individuals and series of generations must disappear, that the future may be built on the framework of an immeasurable past." The concluding paper is on Rivers and their Action, a contribution to physical geography in which arguments as to the antiquity of man founded on the presence of human relics in river-beds or deltas are treated as of little account.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Prof. Barrett and Sensitive Flames

THOUGH my memory fails to recall the fact, I cannot, with Prof. Barrett's letter before me, refuse to believe that he sent me the paper to which he refers.

Perhaps I ought to have known what Mr. Barrett had been doing before large audiences, but I regret to say that I did not. My excellent assistant, Mr. Cottrell, first heard of Mr. Barrett's experiments from one of my own audience, and steps had been taken to do Mr. Barrett justice before his letter appeared. That act he has anticipated by very ably and very modestly doing justice to himself

J. TYNDALL

Remarkable Fossils

ONE of the most remarkable collections of Wealden fossils ever seen, was lately on loan for a few days to the exhibition then open at Horsham, and is one that is not to be equalled by any at our public museums in the country. So remarkable is it that I am induced to give you a short description. As you enter the room to the left, the first thing to attract the attention of the palæontologist was the collection contained in a case of about 12 ft, long by 3 ft. wide, filled to repletion with the fossil bones of the "Great Horsham Iguanodon" and the "Tower Hill Iguanodon," and various other bones. There were the fibula, scapula, and caracoid of Iguanodon in juxtaposition with the humerus belonging to the same specimen, the jaw of the young Iguanodon and the caudal vertebræ, all figured and described in the monographs of the Palæontographical Society. Also the

Hawksbourne, femur, and tibia with metatarsals and a distal phalanx, and various other vertebræ, teeth and phalanges. The jaw of a very young Megalosaurus which evidently perished very shortly after its escape from the egg. The tibia, supposed scapula, and various other bones and teeth of Megalosaurus, the ribs, vertebræ, and teeth of Hylæosaurus. The jaw and other remains of a young *Suchosaurus cultridens* not long escaped from the shell, and teeth of *Suchosaurus*, a fine vertebræ of Streptospondylian type found with the "Great Horsham Iguanodon," and a femur of a young crocodile. The muzzle and portions of jaws, teeth, vertebræ, scutes, and various other bones of *Goniopholis crassidens*. This specimen shows the succession of three teeth. This specimen was borrowed in 1842 by a well-known palæontologist for the purpose of illustration and description. Three artists were employed, who executed five quarto plates of the various portions, but they have never yet been published. Seventeen specimens have not been returned. A younger and very beautiful specimen of *Goniopholis crassidens* in its matrix of stone is missing from this collection. It was borrowed shortly after the above specimen and lithographed at once. It has unfortunately made its escape from custody. It is clear from the specimens shown that the armour of Goniopholis was far more perfect than that of any other living or extinct crocodilian. The toothed and imbricated scutes were in connection with others of a hexagonal or pentagonal shape, which were suturally united. The abdominal scutes overlapped each other on one side. Besides these there are several bones of Pterodactyl, the vertebræ, ribs and teeth of Plesiosaurus, a fine jaw of a beaver, various pubic and tympanic bones, and the pubic bone of a saurian described by Dr. Mantell, bones of turtles and many other bones, too numerous to mention, and some of most gigantic size, and in a wonderful state of preservation. This collection is the property of Mr. Holmes, who is also the discoverer of them.

Many of the bones are altogether unknown, and their inspection may throw some light on the kind of animals to which they belong. THOMAS WM. COWAN

Horsham, Jan. 5

Earthquake in Argyllshire

I BEG to forward to you a letter from the principal lightkeeper at Dhu Heartach Lighthouse, addressed to Mr. Cuningham, Secretary to the Board of Northern Lighthouses.

The Dhu Heartach is a trap rock about fifteen miles to the W.S.W. of Iona, in Argyllshire, which is the nearest land. It is 220 feet long and about 30 feet high, the tower, which is of granite, being raised to the height of 130 feet above the sea. The rock is everywhere surrounded by deep water, and is of an elliptical form. During the erection of the tower fourteen stones, each of two tons, which had been fixed in the tower by joggles and portland cement at the level of 37 feet above high water, were torn out and swept off the rock into deep water.

Although the tower is much subject to impact from the waves, in spite of its height above the sea, yet neither my brother nor I have any doubt that the light-keepers are right in tracing the shock to an earthquake. Perhaps some of your readers may have experienced the shock in other places.

Edinburgh, Jan. 16 THOMAS STEVENSON

"Dhu Heartach Lighthouse, Jan. 7, 1874

"Sir,—I beg leave to inform you of the following rare occurrence:—On the evening of the 6th inst. at 8.13 p.m. (local time), Mr. Leith and I were sitting in the kitchen, when we heard a rumbling noise, followed by a tremulous motion, which lasted about two seconds. On going to the light-room, Mr. McAllister (who was on watch at the time) states that the noise resembled the booming of a cannon, and the tremulous motion was very apparent. A fresh gale from W.S.W. was blowing at the time, but there was no sea striking the rock to cause the concussion; in fact there was less sea than had been for some days previous. When a heavy sea strikes the tower, it has quite a different effect, and cannot be mistaken for anything else. There was neither thunder nor lightning at the time; barometer steady at 29°·96; thermometer 46°; weather hazy.

"I can offer no suggestion as to the cause, unless it proceeded from a slight shock of earthquake: the rumbling noise and tremulous motion indicated such. One thing we are all confident of, it was not from a sea striking the rock. I have no wish to be at all sensational, but I have thought it right to send you the above details, as the same may have been felt in other parts of the country, and this may tend to corroborate it

(Signed) "JAMES EWING

"To the Secretary, Northern Lighthouse Office, Edinburgh."

Telegraphing Extraordinary

THERE appears to have been a misprint in your article "Telegraphing Extraordinary (Jan. 15).

It is there stated that the speed of the automatic instrument is but 200 letters a minute. This speed can be reached by *hand-signalling*, a very usual speed being 170 letters; and perhaps the writer intended to say that 200 letters, or 40 words, was the utmost limit of *un*-automatic service, which would be correct.

Post Office, Jan. 10 R. S. CULLEY

[In contrasting the work obtained out of the Wheatstone "high speed" automatic service in use by the General Post Office in this country with that of the new American instrument, by a slip of the pen the word "letters" was substituted for "words;" but in giving 200 words as the speed over a circuit of similar length to that between Washington and New York, a maximum under most favourable circumstances of insulation of the wires was recorded.

Practically the average working speed obtained on a circuit of from 300 to 400 miles in length, by the Wheatstone, does not exceed 90 words or about 450 letters per minute, and with the Morse about 25 words, or 130 letters. On circuits between 200 and 300 miles the Wheatstone Automatic Service may be considered practically to average 120 words, or about 600 letters per minute. The American instrument transmits from 1,200 to 2,500 words a minute over a 300 mile circuit.—ED.]

Echo at Maidenhead

THERE is a railway-bridge over the Thames at Maidenhead which is said to be of a wider span than any other in England. While standing beneath this arch, we hear the echo of a sound repeated fourteen or fifteen times with tolerable distinctness. From the first to the fourteenth echo occupies about five seconds. The sounds become, of course, less and less loud, but, at the same time, the *pitch* of the note is raised, and has at last risen three quarters of a tone as indicated by a delicate instrument which gives quarter-tones. As I have not seen a similar fact noticed in any work on Sound, I shall be glad if any of your readers can give an explanation.

I may add, that this echo repeats distinctly the sound of the letter *s*, which is not usually the case. J. P.

Belmont, Dartmouth

Flight of Birds

DURING the hurricane of October 6, 1873, I was residing on the west shore of Biscayne Bay, South Florida. In the early part of the gale, and while it was approaching its height, I noticed overhead innumerable "man-of-war hawks." They seemed to be "laying-to" (to use a nautical phrase), with but little motion of their wings; their heads were towards the wind, but instead of moving backwards they seemed to drift off in a line calculated to take them directly away from the storm-centre.

A short time ago I communicated these facts to the secretary of the Smithsonian Institution, who immediately informed me that what I had observed was new to him, and probably to the scientific world, and he advised me to send a copy of my letter to you. The learned secretary also wrote a flattering approval of my suggestion that the behaviour of the birds under consideration might be explained on the theory of "natural selection." I have forgotten the exact wording of my letter, but the idea embodied in it was that during a cyclone the "man-of-war hawk" profits by the experience of its ancestors, an experience which has become organised in the race, and which enables them to make the best possible adjustment to the circumstances which surround them.

Kasson, Minn., Dec. 28, 1873 HORACE B. PORTER

Vivisection

ASSUREDLY "the worthy and humane Huxley" stands in no pressing need of the testimonial of Mr. G. W. Cooke (NATURE, vol. ix. p. 202) to his worth and humanity. (By the way, I thought at first that the gratuity came from the generosity of Mr. E. W. Cooke, whose amusing vivisections, in his "Grotesque Animals," could offend nobody.) Still less does the practice of vivisection stand in need of such encouragement as is given to it in the leading article in NATURE, vol. ix. p. 177. With such a champion as Mr. Ray Lankester, there is fear of physiologists losing sight of the duty of vivisection, not merely for the discovery of truth, but for its demonstration to students of physiology.

Meanwhile I (for one) who, not being an expert in any branch of physiological science, have been educated to set the highest value on its conquests, cannot concede to the physiologists the principle which has been somewhat arrogantly put forth in recent discussions, that research for the purpose of acquiring new facts in physiology necessitates and justifies vivisection. On the contrary, I cannot admit that to ascertain the order of Nature is so high an end in itself as to render superfluous or irrelevant the preliminary question, Whether the means to be employed for that object are right or wrong? We have no need to discuss the rights of the lower orders of sentient beings; it is sufficient that we should recognise the fact that they have been endowed with organisms of exquisite sensibility, not for the purpose of affording man a ready means of experiment, but for the fulfilment of their own functions. To overlook this, to exercise the law of the strong over the weak, and to accustom ourselves to the conscious and deliberate infliction of pain on those beings, with no other object than to satisfy a rational curiosity, must recoil on the operator, and do violence to his moral nature.

When I see acts of wanton cruelty I am revolted, but I have hope; for I trust to the ameliorating effect of education to eradicate the propensity to cruelty. But when I learn that acts of deliberate cruelty are done by "worthy and humane" men, I am revolted without hope. Convince me that the cultivation of physical science culminates in making men so "worthy and humane" that they can practise the vivisection of an animal (to quote Isaac Walton's words) "as if he loved him," and you convince me of the mischievous tendency of such an education.

One word more: if there were a race of intelligents as much superior to man as man is to the dog, and certain investigators of that race were to capture men and women, and subject them to vivisection, in order to advance a knowledge which is beyond the faculties of man, what should we do? Submit, of course; but should we bow with resignation to our lot, and think our pains well spent, if our wretched tortured bodies did thereby add one jot to the scientific capital of our captors? Or, should we not protest to the God of Heaven (if we happened to believe in Him) against the monstrous and enormous injustice of which we should be the victims? Surely there is the same injustice in the abuse of animal organisms (*i.e.*, the use of them against their nature) for the purpose of scientific exploration.

Valentine House, Ilford, Jan. 18 C. M. INGLEBY

Instinct of Monkeys

HAVING read the letters of Dr. Gulliver and G. J. R. in NATURE, vol. viii. pp. 103 and 163, in which the affection of monkeys for their dead is discussed, I think that I may perhaps be permitted to record my experience in regard to a certain class of monkeys that I have peculiar facilities for observing, which is not in accordance with the observation of Mr. Forbes or G. J. R.

I keep, in my garden, a number of Gibbon apes (*Hylobates agilis*); they live quite free from all restraint in the trees, merely coming when called to be fed. One of these, a young male, on one occasion fell from a tree and dislocated its wrist; it received the greatest attention from the others, especially from an old female, who, however, was no relation; she used, before eating her own plaintains, to take up the first that were offered to her every day and give them to the cripple, who was living in the eaves of a wooden house; and I have frequently noticed that a cry of fright, pain, or distress from one would bring all the others at once to the complainer, and they would then condole with him and fold him in their arms.

But one morning one of the flock was found hanging dead in the fork of a tree, his comrades took no notice whatever of him, and were playing and singing their peculiar song as usual close to him; on the body being removed they took no notice whatever.

A neighbour of mine who keeps a pair of these apes, informs me that the male lately came home after an absence of two days very sick; the female, who had theretofore been very affectionate, carefully avoided him, and on his death a few days after showed the most thorough indifference. Very possibly the alleged affection for their dead may exist among some families of monkeys, and not among others. Though my apes live in complete freedom, they have never shown any disposition to breed, though I have had some of them over two and a half years.

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VIVISECTION

AS public attention has again been directed to this question, we think it convenient to reproduce the report of a Committee of the British Association on the subject.

The committee consisted of ten individuals, appointed at the meeting of the British Association, held at Liverpool in the year 1870, to consider the subject of Physiological Experimentation, in accordance with a resolution of the General Committee hereto annexed. The following report was drawn up and signed by seven members of the Committee:—

i. No experiment which can be performed under the influence of an anæsthetic ought to be done without it.

ii. No painful experiment is justifiable for the mere purpose of illustrating a law or fact already demonstrated; in other words, experimentation without the employment of anæsthetics is not a fitting exhibition for teaching purposes.

iii. Whenever, for the investigation of new truth, it is necessary to make a painful experiment, every effort should be made to ensure success, in order that the suffering inflicted may not be wasted. For this reason, no painful experiment ought to be performed by an unskilled person with insufficient instruments and assistance, or in places not suitable to the purpose, that is to say, anywhere except in physiological and pathological laboratories, under proper regulations.

iv. In the scientific preparation for veterinary practice, operations ought not to be performed upon living animals for the mere purpose of obtaining greater operative dexterity.

Signed by:—M. A. LAWSON, Oxford. G. M. HUMPHRY, Cambridge. JOHN H BALFOUR, ARTHUR GAMGEE, Edinburgh. WILLIAM FLOWER, Royal College of Surgeons, London. J. BURDON SANDERSON, London. GEORGE ROLLESTON, *Secretary*, Oxford.

Resolutions referred to in the Report.

That the Committee of Section D (Biology) be requested to draw up a statement of their views upon Physiological Experiments in their various bearings, and that this document be circulated among the Members of the Association.

That the said Committee be further requested to consider from time to time whether any steps can be taken by them, or by the Association, which will tend to reduce to its minimum the suffering entailed by legitimate physiological inquiries; or any which will have the effect of employing the influence of this Association in the discouragement of experiments which are not clearly legitimate on live animals.

The following resolution, subsequently passed by the Committee of Section D (Biology), was adopted by the General Committee:—

> "That the following gentlemen be appointed a Committee for the purpose of carrying out the suggestion on the question of Physiological Experiments made by the General Committee:—Prof. Rolleston, Prof. Lawson, Prof. Balfour, Dr. Gamgee, Prof. M. Foster, Prof. Humphry, Prof. W. H. Flower, Prof. Sanderson, Prof. Macalister, and Prof. Redfern; that Prof. Rolleston be the Secretary, and that they be requested to report to the General Committee."

AMERICAN SCIENTIFIC ENTERPRISE

THE magnificent Free Museum and Menagerie already established in the Central Park, New York, will ever stand as noble monuments of their founder's munificence, and it is now proposed to add to these a third source of benefit to science, and of recreation and instruction to the commonwealth at large. The scheme now in contemplation is the erection in the same Park of a Marine and Fresh-water Aquarium on the most approved system, and of greater magnitude than anything of the kind hitherto attempted. Following a similar principle, it is likewise intended to raise the funds requisite for establishing this aquarium through appeals to the public spirit, and proverbial liberality of New York's more wealthy citizens, as also hereafter to endow the institution, and throw the same freely open to all comers.

The credit of starting this praiseworthy enterprise is due to the Messrs. Appleton, the proprietors of *Appleton's Journal*, a house well-known for their zeal and energy in the promotion of the interests of science, and for the educational benefits that have been conferred through their agency on all branches of the American community. Learning some time since from a notice in NATURE that Mr. Saville-Kent was about to resign his late curatorship of the Brighton Aquarium, Messrs. Appleton at once placed themselves in communication with that gentleman with the view of securing his aid in their scheme. As a site, New York offers remarkable inducements for the establishment of a marine and fresh-water aquarium on the magnificent proportions intended, the sea and the Croton river being equally available for the supply of the two descriptions of water required, while as a position for the acquisition of specimens to stock its tanks its advantages cannot be over-estimated. The art of transporting fish from one distant locality to another has been already practised under the auspices of the "American Fisheries Commission," on a larger scale and with more momentous results than have been obtained on this side of the Atlantic, through means of special cars fitted up with tanks. These last appliances will prove of eminent service and importance for the ordinary transfer of aquarium specimens, while a slight modification of the same might be adapted for accommodation on ship-board, and for the conveyance of fish from distant seas. In fact, starting with this proposed aquarium in the Central Park, the future aim of high-class aquaria should and will doubtless be, to as perfectly represent in its tanks the marine fauna of every quarter of the globe as Menageries and Zoological Gardens do at present the terrestrial inhabitants. The most solid and important advantages, however, likely to arise from an institution founded on the basis of the New York scheme, are associated with the pre-endowed system on which it is to be established; this of itself constitutes a guarantee for the attention to, and accomplishment of, scientific results unattainable in connection with any similar undertaking set up as a mere commercial speculation, and necessarily weighted with the many antagonistic interests upon which its financial profits are dependent. The time again could not be more ripe than the present for projecting the proposed scheme, a sufficient number of aquaria having been established in this country and on the Continent to illustrate the advantages or defects attendant upon the several principles of construction which have been hitherto attempted, as also to indicate the special modifications yet required to make them thoroughly efficient for biological research.

It is to be hoped that the meritorious example set by America will not be lost on this country. England, with her great resources, richly indented coast-line, and innumerable populous centres scattered along the latter, offers remarkable facilities for the establishment of a large zoological station and aquarium, and which, conducted under the auspices of a body of scientific men, with a trained naturalist to superintend it, could not fail to yield the most valuable results. In the absence of sufficient funds forthcoming from private sources for free endowment, the self-supporting system initiated by Dr. Anton Dohrn at Naples offers singular advantages. His scheme of letting out laboratory tables to various universities, governments, and scientific bodies is particularly worthy of notice. The fact that Cambridge has consented to take a share in one of these tables, while testifying to the praiseworthy spirit of that University, carries with it at the same time a severe censure upon the insufficiency of the means provided for scientific investigation nearer home. A well-appointed marine aquarium with suitable laboratories established at Torquay, Plymouth, or such other desirable locality, could not fail to command the support of our leading English universities, and it might be anticipated that also of many others in France, Belgium, Denmark, and other countries of Northern Europe, too far removed to profit fully by the advantages of the Naples station. Through the supply of specimens for class demonstration, such an institution might also derive a considerable income. One of the great disadvantages under which science courses are at present conducted throughout this country arises from the difficulty of obtaining for dissection typical examples of the commonest representatives of our marine fauna and flora, all of which might be furnished regularly and at a low rate through the medium of a large seaside aquarium, towards which is constantly flowing from every side an amount and variety of material more than sufficient for its own requirements.

TUBES FOR SILENT ELECTRICAL DISCHARGES *

RUHMKORFF'S induction coil is now a classical instrument found in every laboratory. It is constantly employed to obtain the sparks intended to combine gases in eudiometric analyses, but its use is not limited merely to effecting combinations, it effects also decompositions, another property utilised in chemistry, particularly to show that at the moment of its decomposition into nitrogen and hydrogen, ammonia gas doubles its original volume. We never obtain, however, in this experiment, a perfectly accurate result, for the induction spark which separates the ammonia gas into its elements is also capable of determining anew their combination to reform the original gas. It exercises thus two actions of a perfectly opposite kind, one of which seems due to true electric action, and the other to the heat which accompanies the passage of the spark.

It would certainly be advantageous to separate these two actions, since they are capable of acting in opposite directions, and it is especially in the preparation of ozone that this separation would be valuable, since ozone, which is easily formed under the influence of the spark, is destroyed by the action of heat. For the purpose of more easily obtaining ozone M. Houzeau has recently constructed an apparatus worked by a Ruhmkorff coil, in which there are no longer sparks, but only dark discharges—*effluvia*—far more efficacious in the production of modified oxygen.

It is known that at the end of last century, Van Marum noticed a peculiar odour in the vicinity of an electric machine giving large sparks, and that he attributed this odour to electricity. In 1840 Schœnbein showed that oxygen disengaged by electrolysis from water has this same odour, and preserves it after being kept in well-stoppered phials; he gave to the substance characterised by this odour the name of ozone.

There remained, however, some doubts as to the real nature of this substance, until the investigations of M. Marignac and of De la Rive in Switzerland, and MM. Fremy and E. Becquerel in France. They succeeded in demonstrating with precision that it was merely pure oxygen which assumed, under the electric influence, a new form. Researches on this modified oxygen soon accumulated, and chemists investigated it with the greater ardour, thinking that in studying this particular form of oxygen, they were touching that important question of simple bodies which at present remains the "great unknown" of chemistry.

So far as research has gone, ozone appears to be a strongly oxidising gaseous body, of one and a half times the density of oxygen, and possessing affinities infinitely more energetic than the latter. Thus it can oxidise cold silver, which so strongly resists the action of ordinary oxygen, it can inflame pure phosphuretted hydrogen, can burn ammonia, transforming it into nitric acid, and can displace the iodine of iodide of potassium. All these properties have been observed in the traces of ozone contained in oxygen submitted to suitable treatment, and the difficulty of obtaining appreciable quantities of ozone was not one of the least obstacles which stood in the way of continuous researches. Thus chemists and physicists have eagerly sought to discover a regular process of preparation, or at least a method of obtaining appreciable quantities of ozone. M. Houzeau, who has devoted much of his time and talents to the study of ozone, has recently devised an apparatus which is spreading rapidly among the laboratories, and which has already yielded very remarkable results, of which the following is a brief *résumé*.

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* Translated from an article in *La Nature*, No. 29.

The apparatus of M. Houzeau consists of two concentric tubes, the middle one enclosing a metallic wire, fixed to one extremity of a Ruhmkorff coil; the other wire, attached to the second pole of the coil, is rolled spirally round the exterior tube; finally, the gas circulates in the annular space comprised between the interior and exterior tubes, and, consequently, is not directly in contact with either of the two wires. The two metallic wires, along which the electricity flows, play the part of a Leyden jar, and the gas which circulates in the space traversed by the dark effluvia, by means of which the two different electricities shot along the wires are re-united, is essentially modified. If it be oxygen, it is charged with a notable quantity of ozone, whose odour rapidly spreads around the apparatus.

M. Houzeau's method produces oxygen much more charged with ozone than any other process; thus it has enabled some new properties of the gas to be discovered. Let the gas issuing from the effluvia-tubes come into contact with olefiant gas and the latter will be immediately set on fire with a loud explosion. M. Houzeau has devised a beautiful experiment, by introducing gradually into a somewhat large tube, a current of bicarburetted hydrogen, obtained by the reaction of sulphuric acid on alcohol; then by means of another narrower tube, penetrating about a centimetre into the tube filled with ethylene, he directs very gently a current of ozone, condensed as much as possible; the ozone which is introduced causes detonation.

When ozone is made to act on benzine a product is obtained, which, according to M. Houzeau, is essentially detonating; this ozo-benzine under concussion or pressure disengages suddenly a considerable quantity of gas, as do nitro-glycerine or the picrates, whose fulminating properties are well known. A few decigrammes of ozo-benzine produce a detonation so violent that the windows of the laboratory are invariably broken, and thus only the very smallest quantities should be used in experiments; 3 to 5 milligrammes suffice to establish the eminently explosive properties of this dangerous substance.

M. Houzeau has also been able to show by means of his apparatus, the remarkable decolourising properties of ozone. If a solution of indigo is thrown into a bottle containing oxygen mixed with ozone, it is as easily deprived of its colour as if it were in contact with chlorine. It is known, moreover, that dyed stuffs are bleached by being simply exposed to the air, and as it is now proved that our atmosphere contains ozone, it appears very probable that it is this gas which is the active agent in the old process of bleaching on the grass.

Such are the new properties which M. Houzeau has been able to establish by employing ozone in a state of condensation infinitely greater than that which is presented when it was prepared by the old methods; and these results are certainly not the only ones which may be looked for.

M. Houzeau is not the only one who has made use of the tubes whose structure he has made known, and soon we may expect to see them modified so as to make them much more durable. M. Boillot, a writer well known to the readers of the *Moniteur*, proposes to substitute for the wire of M. Houzeau's tube, whalebone charcoal contained in the interior tube and in the space comprised between the gas-holding tube and a thin tube concentric with the first two; and M. A. Thénard has brought to bear on the construction of the tubes a further modification which makes them still more efficacious.

As is shown in Fig. 1. M. A. Thénard's apparatus is composed of three tubes of unequal length, welded together. The central tube *aa′* is filled with chloride of antimony in solution with hydrochloric acid; the negative pole B of the coil dips in the liquid which descends to the bottom of the tube at *a′*; the same solution of chloride of antimony is placed in the exterior tube E; it receives the positive wire of the coil at A. The liquid E E is then positively electrified, the liquid *aa′* negatively, and the gas which enters at C and issues at D, after having passed across the annular space between the two tubes, is submitted to the electric effluvia determined by the two opposite electricities of the two liquids.

Into the tubes thus arranged M. A. Thénard directs the gases on which he wishes the electric effluvia to act. One of those which he first submitted to this treatment was carbonic acid, which is decomposed in oxygen and carbonic oxide, with increase of volume. The experiment is perfectly clear, and such as to show the complete difference between the action of the effluvia and that of the spark. While carbonic acid submitted to the decomposing power of dark discharges contains about one-fourth of its volume of the mixture of oxygen and oxide of carbon, which proceeds from its decomposition, carbonic acid decomposed by the luminous sparks of a coil never yields more than 7·5 per cent.; for the latter act not only by their decomposing power, but also by their heat, which determines the combination of the gases as first separated, up to the moment when carbonic-acid, oxygen, and carbonic oxide, are formed in such a state of equilibrium, the spark produces no further effect, the decomposition being equal to the combination. This equilibrium is reached when the mixture contains precisely 7·5 per cent. of carbonic oxide.

This experiment is not, however, the most curious of those which have been published during the course of last year by MM. Paul and A. Thénard working together in that laboratory in the place Saint-Sulpice, which is so liberally opened to all who wish to study.

M. Paul Thénard has noticed that marsh-gas sometimes contains equal volumes of carbonic acid and protocarburetted hydrogen, *i.e.*, it constitutes a mixture in which the carbon, the hydrogen, and the oxygen are found in equal quantities, as when they are combined in a very largely diffused organic matter—glucose. Has the effluvium the power of determining the union of these different elements, so as to reconstitute an organic substance? Such was the idea which MM. P. and A. Thénard wished to verify by making a mixture in equal quantities of formic acid and carbonic acid in one of their effluvia tubes, so arranged that the changes of volume which the gases may undergo are easily determined.

After ten minutes, the condensation of the gases was already sensible; it increased in time, and soon there was seen to appear upon the sides of the tubes a liquid possessing a strong refracting power, viscous, yellowish, which was found to be an organic substance of a somewhat high order, burning readily. Its nature has not been determined, but it is sufficient to prove the importance of MM. Thénard's experiment, that its formation has been established.

The synthesis of organic matters from the elements has always been one of the problems which profitably engage the attention of chemists; and vegetation, indeed, enables us to witness their formation by a series of reactions which we cannot reproduce in the laboratory. Is it not surprising, for example, that under the influence of light a leaf can decompose carbonic acid and water, both extremely stable substances, and which we can only reduce to their elements by means of the most elevated temperatures which we can produce? But this work which is accomplished in the leaf of a plant, the effluvium performs equally well; it decomposes water into oxygen and hydrogen. It can reduce carbonic acid to oxygen and oxide of carbon, just as happens in the green parts of plants under the rays of the sun.

As we learn by experiment that for one volume of carbonic acid decomposed by the green parts of plants, one volume of oxygen is given off, *i.e.* one volume of oxygen exactly equal to that of the carbonic acid, the decomposition of the latter being only partial, it is necessary that the water be forcibly decomposed in the same time as the carbonic acid, and that it yield us the half volume of oxygen necessary to complete that which appears at the moment of insulation, so that the decomposition is represented as follows :—

1 vol. carbonic acid = 1 vol. carbonic oxide + $\frac{1}{2}$ vol. oxygen.
1 vol. vapour of water = 1 vol. hydrogen + $\frac{1}{2}$ vol. oxygen.

The disengaged oxygen presents then a volume equal to that of the decomposed carbonic acid, and leaves instead carbonic oxide and hydrogen in equal volumes, which, on uniting, furnish in végétables one of the products that are met with in young plants, glucose, which exactly represent the carbonic oxide and hydrogen, or, again, the carbon and the water. But this product has never been directly prepared; it has been impossible, so far, to obtain it by synthesis, and all the attempts to unite the carbonic oxide to the hydrogen have been futile. There is, however, a problem of the same order which has been solved by MM. Thénard, and, in our opinion, is one of the most important points of their recent labours. They have not obtained, it is true, the organic matter, yet without a name, which was condensed upon their tube by directly combining hydrogen and the carbonic oxide, but by employing carbonic acid and formic acid, in which the elements are met with in the same proportions, in fact, instead of having

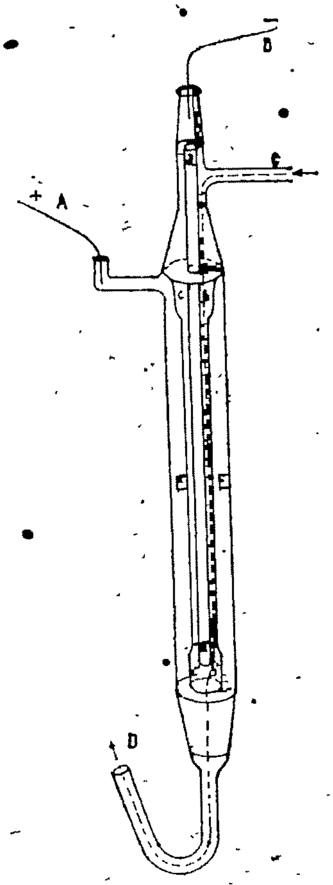


FIG. 1.—M. A. Thénard's Effluvia Tubes.

2 vols. carbonic oxide containing	1 vol. oxygen, 1 vol. carbon vapour.
2 vols. hydrogen.	

they have employed

4 vol. carbonic acid, containing	4 vols. oxygen, 2 vols. carbon vapour.
4 vol. carburetted hydrogen containing	2 vols. carbon vapour, 8 vols. hydrogen.

in which the oxygen and carbon, as in the first case, are in equal volumes, and the hydrogen in double volume. We may then regard the experiment of M. Thénard as opening a new way to the synthesis of organic substances, already so brilliantly studied by M. Berthelot.

The first apparatus employed by MM. Thénard presented a drawback; the gases circulated with considerable difficulty; and their union was not so complete as could be desired; they could not easily be renewed. MM. Thénard have got rid of this difficulty by means of the apparatus represented in Fig. 2. It will be seen that the electricity from the coil is distributed in the two tubes by cups filled with chloride of antimony, one forming the external tube, the other the interior, between which circulate the gases. These are kept in continuous motion by means of a very ingenious employment of mercury. By examining the figure it will be seen that the mercury placed in the large vessel, firmly fixed above the apparatus, can be let out drop by drop into the vertical tube on the right and carry along a certain quantity of gas imprisoned between two consecutive drops. The excess of mercury falls back into the vat into which the horizontal tube goes, while the moving gas received into a funnel which dips into the mercury, is brought into the annular space where it is subjected to the effluvia.

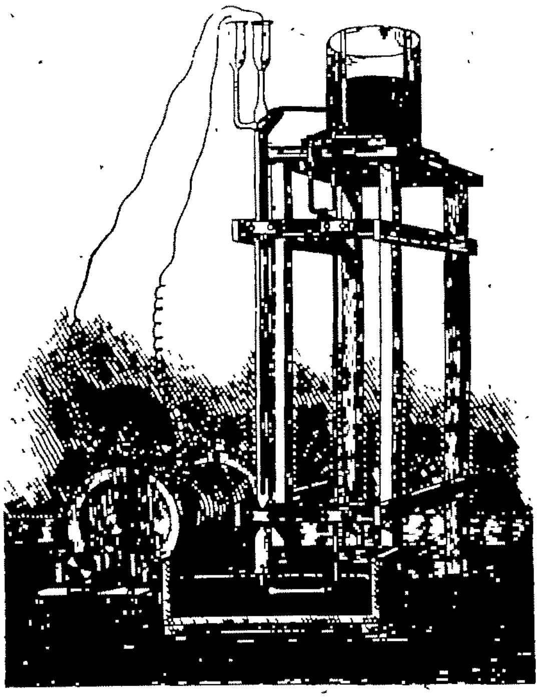


FIG. 2.—Apparatus employed by MM. Thénard to make gases circulate in effluvia tubes.

It is thought that if the gases in coalescing yield a liquid or solid substance, which can only happen by a great diminution of volume, it may be possible to introduce through the funnel placed under the mercury, a new proportion of the gases which, under the influence of the effluvium, will react upon each other.

HAECKEL ON INFUSORIA

IN this communication* Prof. Haeckel discusses the different views which have been entertained as to the structure of the Infusoria, and adopts that of Prof. Siebold, that they are unicellular. This constitutes in his opinion a fundamental distinction between them and the rest of the animal kingdom, although, strictly speaking, some species, as for instance, *Loxodes rostrum*, and *Enchelys gigas*, have more than one nucleus, and must, therefore, be regarded as physiologically consisting of more than one cell. Prof. Haeckel, however, does not attach much importance to these exceptional cases, because the multiplication of the nuclei involves little change of organisation in other respects.

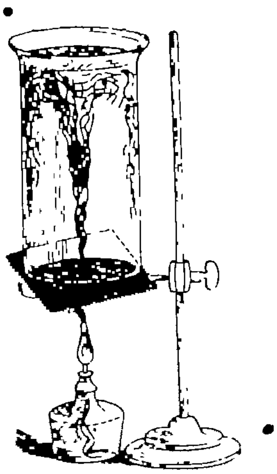
The difficulty of conceiving a single cell with such complex properties becomes lessened, if we remember the nerve-cells of the higher animals, the thread-cells of many Acalephæ.

Considering, then, that the true Infusoria are unicellular, as first maintained by Prof. Siebold in 1845, Prof. Haeckel denies that they have any near connection with either Cœlenterata or the worms. In all the higher groups of the animal kingdom the organism is multicellular, and develops itself from the original egg-cell by the characteristic process of segmentation, and the cellular mass thus formed differentiates itself into two epithelial layers, from the inner one of which the digestive canal, with all its appendages, develops itself; while from the outer layer are formed the skin, nervous system, &c. In his monograph of the Calcareous Sponges, Prof. Haeckel has developed his views of the relations of these two primary layers in the principal groups of animals, and from this fundamental homology has enunciated the theory of a common original form, which he proposed to call "Gastraea," and from which all the higher forms of animals are derived. This theory, which he calls the Gastræa theory, is based upon the consideration that all the six higher animal classes, from the sponges to the lowest vertebrates, pass through a similar stage of development, which he proposes to call the Gastrula stage, and which he considers to be the most important and instructive embryonal form of the animal kingdom. In the calcareous sponges, for instance, this Gastrula law forms a simple generally egg-shaped body, surrounding an ample hollow, the primitive stomach, or digestive cavity, and with an orifice at one end, the primitive mouth. The wall of the digestive cavity consists of two layers, the entoderm, and the ectoderm, which, as Prof. Huxley was the first to point out, are homologous with the outer and inner layers of the vertebrate embryo. Similar larvæ occur in other sponges, and in many zoophytes, while as examples of embryonal forms in other groups he refers to the researches of Kowalevsky in Phoronis, Sagitta, Euaxes, Ascidia, &c.; and of Ray Lankester in Mollusca. He considers that the larval forms of Arthropods can be reduced to the same type; and finally that the researches of Kowalevsky have shown that the same is the case with the lowest vertebrata (Amphioxus). The Infusoria, on the contrary, have no yolk-segmentation, no blastoderm, and consequently nothing which corresponds to the Gastrula stage, nor any homologue of the digestive cavity of other animals. The resemblance of many ciliated larvæ to the Infusoria is therefore merely superficial, the latter being unicellular, the latter multicellular. He regards this difference as so fundamental that he proposes to divide the animal kingdom into two great groups, the Protozoa, and the Metazoa, Blastozoa, or Gastrozoa. The Metazoa, to use his first name, he again divides into two; the Zoophytes, or Cœlenterata on one side, and the Worms, from which again the Molluscs, Echinoderms, Arthropods, and Vertebrates have spring, on the other.

A LECTURE EXPERIMENT

THE ordinary experiment described in books for demonstrating the heating of a body of fluid by convection currents consists in throwing bran into a vessel of water, to the bottom of which a source of heat is afterwards applied. Mr. Clowes's experiment, given in NATURE, vol. ix. p. 162, is no doubt more effective. I have, however, found that the ordinary experiment admits of being made quite satisfactory for the purpose of clear demonstration, and the hint may be useful to those to whom it has not already occurred.

Take a large beaker filled with water, and introduce down to the bottom the end of a burette filled with a strong indigo solution and closed at the top by the finger. If necessary, the solution may be driven out by the application of the mouth to the other end, and gently blowing. The burette must be carefully withdrawn without producing upward currents; this can be easily managed with a little care. The dark fluid now lies at the bottom of the clear water, with which, during a time sufficient for the experiment, it does not appreciably mix. But when a spirit lamp is applied it rises in slender streams, which can be rendered very visible by placing a sheet of white paper behind the beaker.



W. T. T. D.

A SCIENCE LECTURE AT THE CHARTER-HOUSE

A LECTURE on one who was once a Brother of the Charterhouse, and who laid the foundations of scientific electricity, could not fail to be of interest when delivered within the walls of that building, where indeed many of the experiments of the original investigator in question were conducted. This pleasant duty devolved on Dr. Richardson on Thursday, January 22, when he gave to the brethren of the Charterhouse, and to many eminent friends, an experimental demonstration of the work of the early electrician, Stephen Gray.

The lecturer opened his discourse with an exposition of the personal history of Mr. Gray; of this, he said, he could gather little. He discovered Gray first at Canterbury, in 1692, making an observation of a mock sun, in the afternoon of February 6. At this time Gray was evidently engaged on physical and astronomical research. In 1696 he was busy constructing a water microscope; in 1698 he was engaged making a microscope with a micrometer for measuring the height of mercury in the barometer more exactly; in 1699, on April 7, between 4 P.M. and 5 P.M., he was observing an unusual parhelion and a halo; in 1701 he was studying the fossils of Reculver

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* Zur Morphologie der Infusorien. Sep. Abdruck aus der Jenaischen Zeitscrift. Bd. vii,

Cliff and inventing a method for drawing the meridian line by the Pole star and finding the hour by the same; in 1703, on June 15, 16, and 18, he was making some observations on spots on the sun; and in 1706, on May 12, in conjunction with Flamstead at Greenwich, Captain Stannyan at Berne, and Mr. Sharp at Bradford, he was taking observations of the great solar eclipse of that day.

From his various reports on these subjects it is clear that Mr. Gray, while at Canterbury, had a good observatory; he had three telescopes, one of which was of 16 ft., an astronomical table, a theodolite, a pendulum clock, and various other instruments, with the use of which he was quite familiar; but what his occupation was, otherwise, there is no record.

We now lose all sight of Gray until 1717, when we find him being recommended to the Charterhouse by Prince George to become a pensioner there. The letter of recommendation is signed by the Prince, but says no more than that the applicant is a proper person to receive the advantage of residence. In 1719 he entered the building as a pensioner and remained there until his death, seventeen years later.

With his entrance into the Charterhouse a new career of scientific research seemed to have opened itself to Mr. Gray. He became an electrician, and, said Dr. Richardson, his experiments led to such extraordinary results that, but for them, electrical science might have waited for centuries, or for ever, in the state in which he found it. That the audience might know upon what pre-existing data Gray proceeded, Dr. Richardson traced back the origin of experimental electricity to the reign of Queen Elizabeth and to her physician, William Gilbert. He reviewed from this source, briefly and succinctly, the labours of Boyle, Otto de Guericke, Wall, Newton, and Hawksbee, introducing a model of Hawksbee's revolving cylinder, and Sir Isaac's simple experiment of making light bodies move between an excited plate of glass and a table.

In 1720 the first electrical work of Mr. Gray saw the light in a paper entitled "An account of some new electrical experiments," which appeared in that year in the Philosophical Transactions. In this paper the communicability from one electrified substance to other substances not previously electrified is described.

From this point in Mr. Gray's career Dr. Richardson traced him step by step through his experimental researches, making each of his (Gray's) experiments a matter of direct demonstration to the audience, and using only the simple kind of instruments the original investigator himself had at command. Thus were demonstrated the experiments of the cork and the excited tube, the ivory ball on the wooden rod, and the pack-thread experiments, by which Gray discovered that electricity could be conducted long distances. Next were demonstrate the famous loop experiments and those with bridges of pack-thread, silk, and wire, by which silk was discovered to be an insulator, and the new fact of insulation was recorded. The audience, at this point, were carried, by description, to the Mansion of Mr. Granville Wheeler, Otterden House, near Faversham, and were shown by a beautifully simple diagram, drawn for the occasion by the distinguished George Cruikshank—how Mr. Gray, putting up poles in Mr. Wheeler's grounds, insulated a pack-thread line on silk supports, and on July 14, 1729, sent by the line a communication through a distance of 650 ft.

Another series of experiments showed how Mr. Gray discovered induction, the conducting power of water and of metals; the fact that electricity arranges itself upon the surfaces of bodies; that attraction will take place *in vacuo*; and that an insulated, pointed iron rod, when electrified by induction, will yield a brush at its extreme point, will charge another insulated conductor, will give a spark to the knuckle when tha is brought near, and will pass through a chain of animal bodies, if they be insulated.

A beautiful experiment with a soap-bubble, showing how, when insulated and charged, it will attract, closed the experimental part of the lecture. The experiments throughout were highly successful, and were so rendered as to be distinctly visible to all the observers.

A few more points in the personal history of Gray were introduced. It was told that he gained the first Copley Medal of the Royal Society in 1731, and the second in 1732, and that he was admitted a Fellow of the Society on March 15 of the latter year. A graphic description was given of a meeting of the Royal Society on November 25, 1731. At this meeting Prince George was present with the Duke of Lorraine, and the Duke was admitted a Fellow. Afterwards a model of a fire-engine, used at York, was exhibited; then Dr. Frobenius lectured on phlogiston, and on the transmutation of phosphorus, using several pounds' worth of that now common element. Finally, the company ascended to the library, where Mr. Gray showed some experiments, proving how electricity travels along conductors, and succeeded well, notwithstanding the largeness of the company.

Two remaining subjects relating to Gray were briefly touched upon. One was his prediction that what he was doing *in minimis* would some day be so extended, that electrical phenomena would be made to resemble those of thunder and lightning; and the other, his belief that he had invented what he called a Planetarium, that is, a method of making a pith-ball suspended by silk move in circles or ellipses round a metallic centre set in a cake of resin, while the resin was excited by friction of the hand. The first of these observations of Gray had been fulfilled; the latter had appeared as an error of the last days of this wonderful man, and might well be forgiven.

The death of Stephen Gray afforded the lecturer an opportunity for a touching description of a man of science struggling to the last with his labours. On February 14, 1735-36, he was visited by Dr. Cromwell Mortimer, the secretary of the Royal Society, who took from his lips the account of the Planetarium by which, "if God spared his life," the electrical philosopher would create, he thought, much astonishment: but the following day, experiment, speculation, and hope, lay alike low in death.

NOTES

THE report which reached England a few days ago of the death of Livingstone, and which Dr. Kirk was able to characterise as possibly unfounded, as it closely resembled a discredited one current in Zanzibar before he left, received important confirmation yesterday morning. We are enabled, however, to state that a letter seems to have come from Lieut. Cameron at Unyanyembe, reporting that a man named Chumas, who was with Livingstone, had arrived there with a circumstantial story of his death, which Lieut. Cameron, with his slight knowledge of Suahili, had to turn into English. It now depends upon the veracity of Chumas, of which at present there is no means of judging. The circumstantiality is nothing, for the tale of the lying Johanna man was quite as detailed. There is, however, we are bound to confess, much reason to fear that we have lost one of the most unselfish, noble, and devoted investigators the century has produced.

THE Council of the Geological Society has awarded the Wollaston Medal for the present year to Prof. Oswald Heer of Zurich, and the balance of the Proceeds of the Wollaston Donation Fund to M. Henri Nyst of Brussels. The Murchison Geological Medal was awarded by the Council to Dr. Bigsby, F.G.S., and the balance of the Proceeds of the Murchison

Geological Fund to Mr. Alfred Bell and Mr. Ralph Tate, F.G.S., between whom it will be divided.

WE are glad to note that the Emperor of Brazil has conferred upon Dr. Huggins, F.R.S., the honourable distinction of Commander of the Order of the Rose.

WE are informed there is a scheme in contemplation for the erection of an aquarium at Margate. The building will commence at Cold Harbour and pass round Fort Point to the flagstaff point on the Fort Promenade, and will be carried out by a limited liability company, with a capital of 15,000*l.* In all likelihood the work will be commenced early in the spring and will take about nine months to complete. According to present plans the aquarium will be 250 ft. long by 100 ft. broad, and will be connected with a large hall suitable for concerts and balls.

THE Royal Irish Academy have granted to Messrs. Draper and Moss the sum of 30*l.* towards their researches on Selenium, and 35*l.* to G. J. Stoney, F.R.S., towards the construction of the Academy's spectroscope.

ON the 19th inst. Prof. Corfield delivered a lecture on Smallpox and Vaccination, in connection with the Laws of Health Class of the Birmingham and Midland Institute.

A COURSE of "Science Lectures for the People" has been arranged by the Council of the Crewe Mechanics' Institution, to be delivered in their hall. The following is the programme for the next two months:—February 5th and 12th, two lectures on "Mechanics," by Sedley Taylor, M.A., late Fellow of Trinity College, Cambridge; February 19th and 26th, two lectures on "Waves," by G. W. Hicks, B.A., scholar of St. John's College, Cambridge; March 5th and 12th, two lectures on "Light," by William Garnett, B.A., scholar of St. John's College, Cambridge. These two last will treat of spectrum analysis, and its application to the Bessemer flame. H. N. Read, B.A., of St. John's College, Cambridge, will give the two concluding lectures on "Chemistry," on March 19th and 26th. Each lecture will be illustrated by experiments.

WE have received two more of the penny reprints of the Science Lectures for the people delivered at Manchester, namely:—"Muscle and Nerve," by Prof. Gamgee, M.D., F.R.S., and "The Time that has elapsed since the Era of the Cave Men of Devonshire," by William Pengelly, F.R.S. They both seem admirably adapted for the purpose for which they were given, the subjects being treated clearly and familiarly without that sacrifice of scientific accuracy which is often the bane of popular lectures delivered before mixed audiences.

WE have received the thirteenth annual report of the Manchester Scientific Students' Association, containing an account of the various soirées, excursions, and papers for the past year. We are pleased to see that the Committee speak very favourably of the position and prospects of the Society. The total number of members is 177. During 1873 two soirées were held, seventeen lectures delivered on various branches of science, and eleven excursions made to places of scientific and antiquarian interest in the locality.

A NEW society has been formed at Londonderry under the name of the "Londonderry Scientific Association" to promote the study of physical, natural, and historical science. Courses of lectures will be delivered on scientific subjects, single lectures on special subjects by eminent Lecturers will also be provided as occasion may serve, and excursions made during the summer for the field-study of Geology, Zoology, and Botany. The first meeting was held on January 14, when Mr. W. E. Hart, M.A., President of the Society, occupied the chair and opened the proceedings by reading a paper on "Local Scientific Societies; their aims and objects;" in which he pointed out the importance of the study of natural science, both as in itself a valuable branch of education, and as a means of intellectual discipline. This was followed by a discussion on the "Relations of Physico-Geographical Conditions to Civilisation." The "Londonderry Scientific Society" is chiefly composed of ex-members of the "Londonderry Natural History and Philosophical Society," which ceased to exist some two years ago.

WE learn from Mr. Gerard Krefft, F.L.S., Curator of the Australian Museum at Sydney, that the museum, which is the oldest and richest in the Australian colonies, was visited last year by nearly 250,000 persons, who were admitted free. We understand that Mr. Krefft will be glad to receive specimens of all kinds from any individuals interested in the progress of science in New South Wales.

MR. HENRY SOLLY writes to the *Times* with reference to the address issued by the Trades Guild of Learning noticed in NATURE. He says that the Guild originated with himself, and was first proposed at a meeting he called last March to a number of leading working men, when Lord Lyttelton was in the chair, and when Mr. James Stuart, of Trinity College, Cambridge, the originator of the University Extension Scheme, was present at his invitation. A Provisional Committee was then formed, consisting of most of the working men present, with the addition of Mr. Stuart, Mr. Webster, Q.C., Mr. Hodgson Pratt, Mr. Edward Hall, himself, and a few other friends of the movement. That committee resigned its trust, after doing a good deal of work, to a conference held in June at the Hall of the Society of Arts, when the Guild was formally founded, and a Council was appointed on which nearly the whole of the Provisional Committee was placed.

DURING several days in December, says the *Levant Times*, consternation prevailed in the town of Adramytti, in Asia Minor, in consequence of certain ominous noises which seemed to proceed from a considerable depth below the earth's crust. The sound which was heard at intervals and resembled the report of distant cannon, was accompanied and followed by shocks of earthquake, which added to the terror of the inhabitants. At a short distance from the town and in the surrounding villages there was no such cause for alarm, the earth maintaining its normal condition of harmless repose. These details are, no doubt, trustworthy, as they are taken from the report sent in by the Governor of Adramytti to the Governor-General of the Vice-Royalty of Smyrna.

ON December 1, at 10.25 A.M., a violent shock of earthquake was felt at Sofia, in European Turkey. The shock was accompanied by a loud subterranean noise.

THERE were two shocks of earthquake at 8 P.M. on Dec. 26 at Salonika, in European Turkey.

AT the Berlin Medico-Psychological Society in November last, says the *Medical Times and Gazette*, Dr. Hitzig, the author of the method of examination of the brain by electricity, made some remarks on Dr. Ferrier's well-known experiments on the localised functions of the brain, especially with regard to the discrepancies between his own and the latter's results. He considers that the chief of these is that while he and Fritsch have found only one part of the convexity of the hemispheres capable of electrical excitation, Ferrier extends this property to nearly the whole of it. This Hitzig explains by saying that Ferrier has in his experiments used two strong currents (the secondary coil of Stohrer's battery being pushed into eight and even four centimetres), and has thus excited the ganglia at the base of the brain, so that it is to them, and not to centres localised in the cortex, that the movements noted must be referred. Another reason why Hitzig doubts some of the effects of irritation in Dr. Ferrier's cases is because, although there is such a remarkable similarity between the brains of the dog and the cat, the latter found that electrisation of the spot on the cat's brain corresponding to the centre of movement for the tail in the dog gave no result. Hitzig *has* repeated several of the experiments in which Ferrier's results differed from his own, and declares that his own views are re-confirmed. He will shortly publish a detailed account of all his work in Du Bois-Reymond's *Archiv.*

Dr. Peters of Berlin, in the 1873 *Festschrift* of the *Gesellschaft Naturforschen der Freunde zu Berlin*, has described a very interesting new genus of Rodent animals, named by him *Dinomys branickii.* The specimen on which the memoir is based is a skin and skeleton, which were placed in his hands by Mr. L. Taczanowski, Conservator of the Zoological Museum at Warsaw, the latter naturalist having obtained it from Mr. Constantin Jelski, who found it in the high lands of Peru.

We are glad to know that a Microscopical Society has been founded in Victoria quite recently, this being the first of such institutions established in Australia. The first meeting, held at Melbourne, was under the presidency of Mr. W. H. Archer, the Registrar-General of the Colony, who gave an interesting introductory address, in which he showed the great field there is for fresh work in that comparatively unexplored country.

We noticed in Nature last week the announcement of a work by Sir Bartle Frere, G.C.B., G.C.S.I., called "The Impending Famine in Bengal; how it will be met, and how to prevent future Famines in India," with maps; to be shortly published by Mr. John Murray. Amongst Messrs. H. S. King and Co.'s forthcoming books we find the following:—"The Threatened Famine in Bengal; how it may be met, and the recurrence of Famines in India prevented," by Sir H. Bartle Frere, G.C.B., G.C.S.I., &c., with three maps. Is it not somewhat strange that two publishing firms should announce separate works by the same author, with titles that are so nearly synonymous?

Messrs. H. S. King & Co. will shortly publish—"Longevity: the Means of prolonging Life after Middle Age," by Dr. J. Gardner, author of "Household Medicine." "The Principles of Mental Physiology, with their applications to the training and discipline of the Mind, and the Study of its Morbid Conditions," by W. B. Carpenter, M.D., LL.D., F.R.S. "Physiology for Practical Use," by various eminent writers. Edited by James Hinton. 2 vols., with 50 illustrations. "The History of Creation:" a popular account of the development of the earth and its inhabitants, according to the theories of Kant, Laplace, Lamarck, and Darwin. With coloured plates and genealogical trees of the various groups of both plants and animals, by Prof, Ernst Haeckel of Jena. "The New Chemistry," by Prof. Josiah P. Cooke, of Harvard University, with numerous illustrations.

Messrs. Wm. Blackwood and Sons have in the press the following works relating to natural science:—An "Advanced Text-book of Botany for the Use of Students," by Robert Brown, F.R.G.S., Lecturer on Botany under the Science and Art Department of the Committee of the Privy Council on Education and author of the "Races of Mankind," just published by Messrs. Cassell, Petter, and Galpin; "Domestic Horticulture, Window Gardening and Floral Decorations," by F. W. Bu bidge; and "Economic Geology, or Geology in its Relation to the Arts and Manufactures," by David Page, F.G.S., Professor of Geology in the Durham University College of Physical Science, Newcastle.

Mr. William Topley's interesting paper, with maps and sections, "On the Relation of the Parish Boundaries in the South-east of England to Great Physical Features, particularly to the Chalk Escarpment," has been reprinted in a separate form from the *Journal of the Anthropological Institute.*

In *Le Tour du Monde*, is appearing a French translation of the account of the voyage of the German Arctic Expedition of 1869—70, in the ships *Germania* and *Hansa.* The illustrations are plentiful and beautiful.

We have received the following reprints of papers by Mr. F. W. Putnam, from the *Bulletin of the Essex Institute* (U.S.):—"Description of a Stone Knife found at Kingston, N.H.," "Description of a Carved Stone representing a Cetacean, found at Seabrook, N.H.," and "Descriptions of Stone Knives found in Essex County, Massachusetts."

The success of Professor G. W. Hough, of the Dudley Observatory, in constructing self-recording barometers and thermometers, lends additional interest to his announcement of the successful construction of an automatic evapometer and rain-gauge. The apparatus consists of a vessel two feet square and one foot deep, suspended on levers, and held in equilibrium by a small spring, the amount of change in the weight of the mass, either from rainfall or evaporation, being indicated on the scales of a delicate balance. In order to secure the mechanical record of the hourly variations in the weight of the vessel and of its contents, the professor causes the lever to vibrate between two platinum points so placed that whenever a change in the weight of the vessel by a given amount (say ten grains) takes place, a magnetic circuit will be established passing through an electro-magnet. A micrometer screw will then be operated by means of clock-work, thereby tracing a curve on a revolving drum, precisely as in the case of the self-recording barometer and thermometer.

The principal articles in the last number of the *Canadian Journal of Science, Literature, and History* are—on "An Ancient Carved Stone, found at Chesterholm, Northumberland, England," by the Rev. Dr. M'Caul; an article by Dr. Daniel Wilson, on the work done by Alexander Gordon, the Scottish antiquary, author of the *Itinerarium Septentrionale*, and a paper, also published separately, by Prof. H. A. Nicholson, on "The Species of Favosites of the Devonian Rocks of Western Ontario." Appended are meteorological tables for Toronto for the half-year May to October 1873.

The addition to the Zoological Society's Gardens, during the past week, include an Ocelot (*Felis pardalis*) from America, presented by Mr. J. Ryde; a White-headed Sea Eagle (*Haliaetus leucocephalus*) from Nova Scotia, presented by Mr. H. Walpole; two Grey-breasted Parrakeets (*Bolborhynchus monachus*) from Monte Video, presented by Mrs. C. Dawkins; a Bernicle Goose (*Bernicla leucopsis*), European, presented by Mr. T. P. Tyndale; two Sclater's Curassows (*Crax sclateri*) from Maranham, and a Prince Albert's Curassow (*C. alberti*) from Columbia; a Capybara (*Hydrochaerus capybara*) from South America; a Rhea (*Rhea americana*) from Rosaria, and a Chimachima Milvago (*Milvago chimachima*) from Brazil, purchased or deposited, the last-named bird being new to the collection.

THE ACOUSTIC TRANSPARENCY AND OPACITY OF THE ATMOSPHERE[*]

THE cloud produced by the puff of a locomotive can obliterate the noonday sun; it is not therefore surprising that in dense fogs our most powerful coast lights, including even the electric light, become useless to the mariner.

A disastrous loss of life and property is the consequence. During the last ten years, for example, the number of total wrecks on the coasts of the United Kingdom, which were reported to have been caused by fog and thick weather, amounted, I am informed, to 273 vessels.

Of late years various efforts have been made, both on our own coasts and on the American seaboard, where trade is more eager and fogs more frequent than they are here, to furnish warning and guidance to ships by means of sound signals of great power established along the coast. Regarding the performance of such signals, the most conflicting evidence exists; and no investigation has been hitherto instituted sufficiently exhaustive to remove the uncertainty.

The problem has occupied for some time the attention of the Elder Brethren of the Trinity House; and soon after my return from America they requested me, as their official adviser in scientific matters, to superintend an investigation of the entire subject. They had appointed a committee under whose auspices two stations had been established at the South Foreland. I entered upon the inquiry with such ardour as I could derive from a sense of duty, rather than from the pleasure of hope, for I knew it would be long and difficult, and that I was at the mercy of a medium, the earth's atmosphere, which could not be put into the witness-box and cross-examined scientifically. The experimenter can usually impose his own conditions upon Nature, and force her to reply. In the present case we were forced to accept the conditions which Nature imposed.

Nevertheless, if the student only holds on faithfully to any natural problem, intending his mind upon it, and not falling into hasty despair, he is sure to be rewarded in the end; and after a time results, important not only in a practical but in a purely scientific point of view, appeared to grow out of the investigation. I mentioned this to the Deputy Master of the Trinity House, saying that I thought such results might, without impropriety, be communicated to the Royal Society and the Royal Institution. His response was prompt and cordial, and he was seconded by his colleagues in this response. They gave not only the requested permission (which on various pleas they might have withheld), but they have aided me in every way in the preparation of this discourse.

I would add that the Elder Brethren themselves have had a large share in the executive portion of this investigation, and whatever success has attended the inquiry is in a great measure due to the cheerful promptness and thoroughness with which my wishes and suggestions were carried out by the gentlemen with whom I had the honour to act. It is not necessary to mention names when all have been so sympathetic and so helpful, but I should like to refer to a few gentlemen on the working staff of the Trinity House, who have aided me with all assiduity and all zeal. They are the able Trinity House engineer, Mr. Douglass, his assistant engineer, Mr. Ayres, and Mr. Price Edwards, the private secretary of the Deputy Master of the Trinity House.

On Monday, May 19, the experiments began. The instruments employed had been previously mounted at the top and bottom of the South Foreland Cliff. They were two brass trumpets, or horns, 11 ft. 2 in. long, 2 in. in diameter at the mouth-piece, opening out at the other end to a diameter of 22 in. They were provided with vibrating steel reeds, 9 in. long, 2 in. wide, and ¼ in. thick, and were sounded by air of 18 lbs. pressure. They were mounted vertically on the reservoir of compressed air; but within about 2 ft. of their extremities they were bent at a right angle, so as to present their mouths to the sea. These horns were constructed by Mr. Holmes. There were also two whistles shaped like those in use on locomotives, one 6 in. in diameter, sounded by air of 18 lbs. pressure; the other constructed by Mr. Baily of Manchester, 12 in. in diameter and sounded by steam of 64 lbs. pressure.

We embarked on the steamer *Irene*, and placed ourselves abreast of the signal-station, halting at a distance of half a mile from it. The wind was strong, the sea rough. The superiority of the trumpets to the whistles was very marked, and I may say continued marked throughout. Their sound was exceedingly fine and powerful. At 1 mile's distance their sound was clear and strong; at 2 miles they were heard distinctly, though not loudly. The whistles were also heard, but as fog-signals they had become useless. At 3 miles the horns became also useless. It required great attention to hear them distinctly. At a distance of 4 miles, with the paddles stopped, we listened long and attentively, but heard nothing.

On May 20, at 3 miles' distance, the steam whistle was not at all heard, the horns but faintly. At 4 miles' distance, the air being very light, the sea calm, and the circumstances generally to all appearances highly favourable, we halted and listened. The horns were so heard as to render it unmistakeable that a sound was there. At 4·8 miles the sounds were faintly heard; at 5 miles an occasional murmur reached us. At 6 miles the faint hum of a horn was wafted to us at intervals. A little farther out, though local noises were absent, and though we listened with stretched attention, we heard nothing.

This position, clearly beyond the range of whistles and trumpets, was chosen with the view of making a decisive comparative experiment between horns and guns as instruments for fog-signalling. Through the courtesy of General Sir A. Horsford we were enabled to carry out this comparison. At 12·30 precisely the puff of an 18-pounder, with a 3-lb charge, was seen at Dover Castle, which was about a mile farther off than the South Foreland. Thirty-six seconds afterwards the loud report of the gun was heard, its complete superiority over the trumpets being thus to all appearance demonstrated.

We clinched this observation by steaming out to a distance of 8¼ miles, where the report of a second gun was well heard. At 10 miles the report of the gun was heard by some and not by others. At 9·7 miles a fourth report was heard by all observers.

There was nothing, so far as I am aware of, in our knowledge of the transmission of sound through the atmosphere, to invalidate the founding upon these experiments of the general conclusion that, as a fog-signal, the gun possessed a clear mastery over the horns. No observation, to my knowledge, had ever been made to show that a sound once predominant would not always be predominant; or that the atmosphere on different days would show preferences to different sounds. A complete reversal of the foregoing conclusion was therefore not to be anticipated; still, on many subsequent occasions, it was completely reversed.

On June 2 the maximum range, at first only 3 miles, afterwards ran up to about 6 miles.

Optically June 3 was not at all a promising day; the clouds were dark and threatening; and the air filled with a faint haze, nevertheless the horns were fairly audible at 9 miles. An exceedingly heavy rain-shower approached us at a galloping speed.

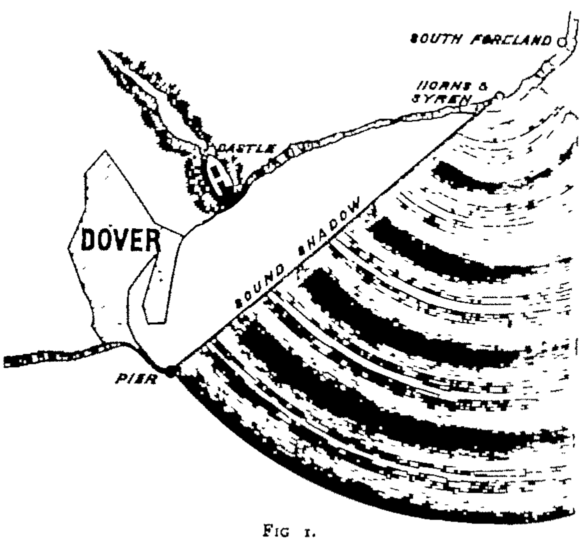

FIG. 1.

The sounds were not sensibly impaired during the continuance of the rain. This state of the atmosphere, according to hitherto expressed opinions, should have deadened the sound. It rather aided the sound, and this added to my perplexity.

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[*] Royal Institution, Friday evening Discourse by Prof. Tyndall, D.C.L., LL.D., F.R.S. Jan. 16.

On June 10 the maximum range was 9 miles. An extraordinary sinking of the sound was, however, noticed on the Dover side of the Foreland. At a mile's distance from the station the sounds rapidly fell. Surprised at the suddenness of the effect, and thinking it might be due to some peculiarity of the horns, at 2 miles' distance I signalled for the guns. With a 3 lb. charge not one of them was heard.

On June 11 we steamed towards the South Sound Head lightship. At the distance of 2¼ miles, and even at 2 miles and less from the station, the sounds were not so strong as at 3¾ miles. We steamed abreast of the station and on to the line joining the South Foreland to the end of the Admiralty Pier. At three-quarters of a mile from the station the sound fell, and a little farther on was scarcely audible. This weakening of the sound between the pier and the Foreland was invariable. This needs a word of explanation. The fall of the sound is not caused directly by an acoustic shadow, for it occurs when the instruments are in view, but the limit of an acoustic shadow is close at hand. A little within the line joining the Foreland and the pier end, the instruments are cut off by a projection of the cliff near the station; all the sea space between this limit and the cliff under Dover Castle is in the shadow. Into this, however, the direct waves diverge, and lose intensity by their divergence, the portion of the wave nearest the shadow suffering most. To this must be added the effect of interference.

On June 25 the range was 5½ miles. On June 26 the range was 10 miles. The former day the wind was in the direction of the sound; on the latter the wind was opposed. Plainly there must be something besides the wind which determines the sound-range. This something was now the object of search.

Is it the clearness of the atmosphere? All previous writers have extolled a clear atmosphere as best for sound; but on July 18 we steamed out to a distance of 10 miles and heard sounds, the white cliffs of the Foreland being at the same time entirely hidden in thick haze. Nay, more: we spoke the *Triton* tender on its way from the *Varne* lightship, and took the master of the *Varne* on board. He reported that the sounds had been heard at the lightship, though it is 12¾ miles from the Foreland. It was, moreover, dead to windward of the Foreland, so that both haze and wind were then in opposition; still the sound ranged at least twice as far as it had done on days when neither haze nor wind was there to interfere with the sound.

On July 2, a sudden acoustic darkness, if I may use the term, settled upon the atmosphere. The range was only 4 miles. The magnitude of the fluctuations, from 3¼ to 12¾ miles, observed up to this date, was striking: but I was unable to fix upon any meteorological element that could be held accountable for them. The wind, the clearness of the air, the barometer, the thermometer, the hygrometer, gave me no help. All was perplexity. I longed for light, but saw little prospect of obtaining it.

July 3 was a lovely morning: the sky was of a stainless blue, the air calm, and the sea smooth. I thought we should be able to hear a long way off. We steamed beyond the pier end and listened. The steam clouds were there, showing the whistles to be active; the smoke puffs were there, attesting the activity of the guns. Nothing was heard. We went nearer; but at two miles horns and whistles and guns were equally inaudible. This however being near the limit of the sound shadow, I thought that might have something to do with the effect, so we steamed right in front of the station, and halted at 3¾ miles from it. Not a ripple nor a breath of air disturbed the stillness on board, but we heard nothing. There were the steam-puffs from the whistles, and we knew that between every two puffs the horn sounds were embraced, but we heard nothing. We signalled for the guns: there were the smoke puffs apparently close at hand, but not the slightest sound. It was mere dumb show on the Foreland. We steamed in to 3 miles, halted, and listened with all attention. Neither the horns nor the whistles sent us the slightest hint of a sound. The guns were again signalled for; five of them were fired, some elevated, some fired point blank at us. Not one of them was heard. We steamed in to two miles, and had the guns again fired: the howitzer and mortar with 3 lb. charges yielded the faintest thud; and the 18-pounder was quite unheard.

In the presence of these facts I stood amazed and confounded, for it had been assumed and affirmed by distinguished men who had given special attention to this subject, that a clear, calm atmosphere was the best vehicle of sound: optical clearness and acoustic clearness were supposed to go hand in hand: indeed, it had been proposed to make the one a measure of the other. But here was a day perfectly optically clear, proving itself to be a day of acoustic darkness almost impenetrable. I was driven slowly to the conclusion that all I had read upon this subject was wrong, and that for 165 years, namely since 1708, when Dr. Derham published his celebrated paper on this subject, succeeding generations of scientific men had gone on repeating the same errors. This knowledge, however, did not help me much. The problem was still there challenging solution.

I ventured, two or three years ago, to say something regarding the function of the Imagination in Science, and notwithstanding the care that I took to define and illustrate its real province, many persons, amongst whom were one or two able men, deemed me loose and illogical; in fact, merely poetic, when I referred to the imagination. The history of science, however, numbers many men of strong poetic temperament, who, in the presence of a scientific problem, became as cold and clear as the light of stars. Look at these two pieces of polished steel. Have you a sense, or the rudiment of a sense, to distinguish the inner condition of the one from that of the other? And yet they differ materially, for one is a magnet, the other not. What enabled that noble philosopher, and pure and elevated character, Ampére, to surround the atoms of such a magnet with channels in which electric currents ceaselessly run, and to deduce from these pictured currents all the phenomena of ordinary magnetism? What enabled Faraday to visualise his lines of force, to follow them through magnets and through space until his mental picture became a guide to discoveries which have rendered this place immortal? What but imagination? I have reason to know but too well the fantastic, and even scandalous use that is made of the faculty when it is divorced from the disciplined understanding and handed over to the undisciplined passions and emotions. But this is not the scientific use of the imagination.

And now to return. Figure yourself on the deck of the *Irene*, with the invisible air stretching between you and the South Foreland, knowing that it contained something which stifled the sound, but not knowing what that something is. Your senses are not of the least use to you; you are unable to see, or hear, or feel, or taste, or smell the object of your search; nor could all the philosophical instruments in the world, as it now is, render you the least assistance. You cannot take a single step towards the solution without the formation of a mental image, in other words, without the exercise of the imagination. Let me unfold my own exact course of thought and action.

Sulphur in homogeneous crystals is exceedingly transparent to radiant heat, whereas the ordinary brimstone of commerce is highly impervious to it. Why? Because the brimstone of commerce does not possess the molecular continuity of the crystal, but is a mere aggregate of minute grains not in pefect optical contact with each other. When this is the case, a portion of the heat is always reflected on entering and quitting a grain. Hence when the grains are minute and numerous, this reflection is so often repeated that the heat is entirely wasted before it can plunge to any depth in the substance. A snowball is opaque to light for the same reason. It is not optically continuous ice, but an aggregate of grains of ice, and the light which falls upon the snow being reflected at the limiting surfaces of the snow granules, fails to penetrate the snow to any depth. Thus by the mixture of air and ice, two transparent substances, we produce a substance as impervious to light as a really opaque one. The same remark applies to foam, to clouds, to common salt, indeed to all transparent substances in powder. They are all impervious to light, not through the real absorption or extinction of the light, but through internal reflection.

Humboldt, in his observations at the Falls of the Orinoco, is known to have applied these principles. He found the noise of the Falls three times louder by night than by day, though in that region the night, through beasts and insects, is far noisier than the day. The plain between him and the Falls consisted of spaces of grass and rock intermingled. In the heat of the day he found the temperature of the rock to be 30° higher than that of the grass. Over every heated rock, he concluded, rose a column of air rarefied by the heat, and he ascribed the deadening of the sound to the reflections which it endured at the limiting surfaces of the rarer and denser air. This philosophical explanation made it generally known that a non-homogeneous atmosphere is unfavourable to the transmission of sound.

But what on July 3, over a calm sea, where neither rocks nor grass existed, could so destroy the homogeneity of the atmosphere as to enable it to quench, in so short a distance, so vast a body of sound? As I stood upon the deck of the *Irene*, pondering this question, I became conscious of the exceeding power of the sun beating against my back and heating the objects near me. Beams of equal power were falling on the sea, and must have produced copious evaporation. That the vapour generated should so rise and mingle with the air as to form an absolutely homogeneous mixture, I considered in the highest degree improbable. It would be sure, I thought, to streak and mottle the atmosphere with spaces, in which the air would be in different degrees saturated, or it might be displaced, by the vapour. At the limiting surfaces of these spaces, though invisible, we should have the conditions necessary to the production of partial echoes, and the consequent waste of sound.

Curiously enough, the conditions necessary for the testing of this explanation immediately set in. At 3.15 P.M. a cloud threw itself athwart the sun, and shaded the entire space between us and the South Foreland. The production of vapour was checked by the interposition of this screen, that already in the air being at the same time allowed to mix with it more perfectly; hence the probability of improved transmission. To test this inference the steamer was turned and urged back to our last position of inaudibility. The sounds, as I expected, were distinctly though faintly heard. This was at 3 miles' distance. At 3¾ miles we had the guns fired, both point blank and elevated. The faintest thud was all that we heard, but we did hear a thud, whereas we had previously heard nothing, either here or three-quarters of a mile nearer. We steamed out to 4¼ miles, when the sounds were for a moment faintly heard, but they fell away as we waited; and though the greatest quietness reigned on board, and though the sea was without a ripple, we could hear nothing. We could plainly see the steam-puffs which announced the beginning and the end of a series of trumpet-blasts, but the blasts themselves were quite inaudible.

It was now 4 P.M., and my intention at first was to halt at this distance, which was beyond the sound range, but not far beyond it, and see whether the lowering of the sun would not restore the power of the atmosphere to transmit the sound. But after waiting a little, the anchoring of a boat was suggested; and though loth to lose the anticipated revival of the sounds myself, I agreed to this arrangement. Two men were placed in the boat, and requested to give all attention so as to hear the sound if possible. With perfect stillness around them, they heard nothing. They were then instructed to hoist a signal if they should hear the sounds, and to keep it hoisted as long as the sounds continued.

At 4.45 we quitted them and steamed towards the South Sand Head lightship. Precisely fifteen minutes after we had separated from them the flag was hoisted. The sound, as anticipated, had at length succeeded in piercing the body of air between the boat and the shore.

On returning to our anchored boat we learned that when the flag was hoisted the horn sounds were heard, that they were succeeded after a little time by the whistle sounds, and that both increased in intensity as the evening advanced. On our arrival of course we heard the sounds ourselves.

The conjectured explanation of the stoppage of the sounds appeared to be thus reduced to demonstration, but we pushed the proof still further by steaming farther out. At 5¼ miles we halted and heard the sounds. At 6 miles we heard them distinctly, but so feebly that we thought we had reached the limit of the sound range. But while we waited the sound rose in power. We steamed to the Varne buoy, which is 7¼ miles from the signal station, and heard the sounds there better than at 6 miles distance.

Steaming on to the Varne lightship, which is situated at the other end of the Varne shoal, we hailed the master, and were informed by him that up to 5 P.M. nothing had been heard. At that hour the sounds began to be audible. He described one of them as "very gross, resembling the bellowing of a bull," which very accurately characterises the sound of the large American steam whistle. At the Varne lightship, therefore, the sounds had been heard towards the close of the day, though it is 12¾ miles from the signal station.

What is the full meaning of this result? Imagine a man in an anchored boat at 2 P.M. at a distance of 2 miles from the Foreland, and suppose him possessed of instruments which would enable him to measure the growing intensity of the sound. Applying the law of inverse squares, to carry the sound to six times the distance, its intensity at 2 miles would have to be augmented 36 times. But the Varne lightship is more than 6 times 2 miles from the Foreland. Supposing no absorption or partial reflection to occur, the observer would have found that by the lowering of the sun the sound at his position had at 6 P.M. risen to more than forty-fold the intensity which it possessed at 2 P.M. In reality the augmentation was still greater.

(*To be continued.*)

BIRMINGHAM NATURAL HISTORY AND MICROSCOPICAL SOCIETY

AT the annual *soirée*, held on Tuesday, December 16, 1873, celebrating the sixteenth year of the existence of the society, Mr. W. R. Hughes, F.L.S., the president, gave, at the request of the committee, an address on "The recent Marine Excursion made by the Society to Teignmouth." After alluding to the apt and graceful remarks of his predecessor in office (Rev. H. W. Crosskey, F.G.S.) twelve months ago, on the advantages of the study of Natural History, and then describing the preliminary arrangements in connection with the excursion which have already been detailed in NATURE, vol. viii. pp. 334 and 469, Mr. Hughes stated that upwards of 20 members of the society, including several ladies, proceeded from Birmingham and assembled at the headquarters at Teignmouth on Monday, September 1, on which day the dredging operations commenced on board the yacht *Ruby* with satisfactory results. These were carried on during the week, and have already been described in NATURE; the principal feature being the capture of the pedunculate form of *Antedon rosaceus* (*Comatula rosacea*), the rosy feather star, including representatives of 12 genera of Echinodermata and about 40 species of Hydrozoa and Polyzoa, the last of which had been mounted and presented to the society by his friend and colleague, Mr. A. W. Wills, to whom the Society were also indebted for life-like drawings of the *Antedon* in various stages of development.

Mr. Hughes then proceeded to speak of the moral of the marine excursion. So far as he was informed it was the first of its kind that had been undertaken by any society carrying on its operations in an inland district like Birmingham, far removed from the sea, and that point was in itself noteworthy, and might contribute to raise the status of the society and cause its example to be followed by others of a kindred nature. He thought it was pretty well agreed among the members that the excursion was attempted properly, and on the whole carried out successfully. The members who took part in it had been stimulated and encouraged in their project by the hearty and unanimous way in which it was adopted by others whose studies lay in different directions, by praise from NATURE, that most cultivated of scientific serials, and by "good words" from the local press. The results might not have satisfied all. Circumstances rendered the absence of many old supporters of the society unavoidable. It was planned a little too late in the season, and many of the microscopic animals they dredged had played their part in the great problem of life, and empty cells alone remained where many a delicate and beautiful organism had spread its feathery plumes "in the dark unfathomed caves of ocean." Too much time was devoted to the dredging, and not sufficient for subsequent investigation of the proceeds. Still the members had enjoyed the rare opportunity of examining many beautiful marine animals under the microscope which they could not have hoped for at home. And the excursion had done much to promote exchange of thought and friendliness among those taking part in it. Doubtless if a similar one were planned in 1874 the members would profit by the experience of the late one, and Mr. Hughes commended such to the consideration of the committee, and suggested that the members should make it the subject of their annual holiday, especially as ladies were now for the first time admissible as members. The President stated he could not close his remarks to an assembly composed of naturalists and those who had evinced a taste in their pursuits, without alluding to the fact that must have impressed most of them, viz.: that the study of marine zoology had in these days attained an interest second to that of no other branch of natural history, and that the existence and habits of the denizens of "the great and wide sea" were discussed as familiarly in the newspapers of the day as the events of social and political life. As further evidence, Mr. Hughes alluded to the record, almost surpassing any story in the "Thousand and One Nights" contained in that most charming of books "The Depths of the Sea," of the researches in deep-sea dredging, by Prof. Wyville Thomson, F.R.S., and Dr.

Carpenter, F.R.S., refuting the old theories of the non-existence of animal life at great depths, and bringing to light (with others) animals such as *Pentacrinus wyville-thomsoni* and *Bathycrinus gracilis*, pedunculate star-fishes allied to the *Comatula*, from 2,435 fathoms, whose very existence was unsuspected, and who were supposed "in the lapse of ages to have been worsted in the struggle for life." And following these investigations came the exploring expedition of H.M.S. *Challenger*, the most important mission of its kind that Great Britain had ever engaged in, from whence we now and then got stray tidings of yet more remarkable animals,—animals of comparatively high organisation, allied to the lobster, dredged up from 2,000 fathoms: in one individual, a gigantic amphipodal crustacean, "the eyes of the creature extended in two great facettic lobes over the whole of the anterior part of the cephalo-thorax, like the eyes *Æglina* among Trilobites" (NATURE, vol. vii. p. 388). In another, *Deidamia leptodactyle*, there were no traces whatever of eyes of sight or their pedicels (NATURE, vol. viii. p. 52). Further, from the enormous depth of 3,000 fathoms, or nearly 3¼ statute miles, "a depth which does not appear to be greatly exceeded in any part of the ocean," have been taken a tube-building annelid, its tube formed of the gritty matter which occurs but sparingly in the clay at the bottom, affording, in fact, as Prof. Wyville Thomson remarks, "conclusive proof that the conditions of the bottom of the sea to all depths are not only such as to admit of the existence of animal life, but are such as to allow of the unlimited extension of the distribution of animals high in the zoological series, and closely in relation with the characteristic faunæ of shallower zones (NATURE, vol. viii. p. 53).

Contemporaneously with these expeditions and what would appear to be not an inappropriate *sequitur*, marine aquaria of extensive size, and constructed on scientific principles, had been established in some of our principal towns, thus affording a new source of enjoyment and an intellectual gratification to the people—that of the examination of living marine animals as they exist "in the depths of the sea"—and combining with this in some instances a source of pecuniary benefit to the promoters. The public interest in these establishments seemed so great that the arrival of the octopus had attracted almost as much attention as the visit of a foreign emperor, and the death of the porpoise was mourned as a national calamity.

In conclusion the president said he hoped he had said something suggested by the recent marine excursion to interest the members in marine zoology. For his own part he could say that the little attention he had been able to devote to it had been a most acceptable relief to official duties (as Treasurer of the Borough), always laborious and responsible, and at the same time it had brought him in contact with fellow-workers in natural history from whose friendship and kindly intercourse he had derived the most lasting pleasure.

Mr. Hughes ventured to express his opinion that a Marine Aquarium, if constructed properly and managed efficiently—for instance like that beautiful one at the Crystal Palace under the direction of his friend, Mr. W. Alford Lloyd, who had done more for the advancement of public aquaria than any man living—would be most acceptable to Birmingham, where the great Priestley carried on his scientific researches, and in 1773 obtained the Copley medal of the Royal Society for the discovery of the mutual dependence of plants and animals on each other—the grand principle of all aquaria. It occurred to him after much thought, and as a successful student of marine aquaria for many years, that no greater attraction or means of intellectual recreation for the working classes and their neighbours in the mining districts could be devised, because it would be so utterly different from any other exhibition now existing, and so suitable as a relief and mental refreshment for those in crowded courts to whom the sea was but a name. It was indisputable that "the sea and its living wonders" had irresistible fascination to us far away from it. The peculiar central inland position of Birmingham would be highly advantageous by allowing ready and rapid modes of conveyance of rare animals from almost any part of the coast. He commended the project to the earnest consideration of the many wealthy and intellectual men in the town, and could not help believing and hoping—although his views as a naturalist might be sanguine—that Birmingham would sooner or later possess a marine aquarium worthy of it, and follow the example of London, Brighton, Liverpool, Manchester, Plymouth, and other large towns, which, besides others on the Continent, had already taken the matter up.

We understand that Mr. Hughes' suggestion has been acted upon, and that an influential committee has since been appointed to make inquiries with the view to the promotion of an efficient marine aquarium for Birmingham.

SCIENTIFIC SERIALS

Astronomische Nachrichten, No. 1,969, Jan. 9.—This number contains a paper by Prof. Spoerer on M. Faye's theory of solar spots. He refers to M. Faye's statement that spots are below the surface of the sun, and to his reliance on his treatment of Carrington's observations; for if a spot be observed for two or more revolutions its distance from the limb can be calculated on the assumption that the spot is on the surface of the sun. Should, however, the observed plan of the spot not agree with the calculated position, the assumption will be that the spot is below or above the surface of the sun. Prof. Spoerer informs us that, on the whole, his observations show that the observed distance from the limb of the sun are too great; this he ascribes to the effect of refraction altering the position of the sun's limb to a greater degree than that of the spot.—On the identity of Coggia's Comet of Nov. 10, with Comet 1818 I. by Prof. Weiss. In this paper the author gives the elements of Coggia's comet as lately determined, together with the recorded observations of Comet 1818 I. from which he considers the two comets to be identical.—In a second paper by Prof. Weiss he gives elements calculated on a parabolic orbit and on two elliptic orbits of 55·82 and 6·9775 years respectively, being on the assumption in the first case that one revolution has happened since 1818, and in the other that eight have taken place.—Observations on variable stars, by G. T. G. Schmidt. From observations up to the end of 1873, given at length in his paper, we extract the epochs of maxima and minima of the following stars:—

		Max.		Min.	
Mira Ceti	about	May 25		Jan. 30·5,	1873, mag. 9·5
S Scorpii	,,	Sept. 13, mag. 10		—	,,
R Scorpii	,,	Aug. 27, mag. 10·8		—	,,
R Bootis	,,	Sept. 15,	—	—	,,
R Virginis	,,	July 27,	—	May 13	,,
S Coronæ	,,	July 6,	—	—	Period, 363 days.

	Max.	Min.	
R Leonis	May 18	—	
R Leporis	—	Jan. 29	
χ Bygni	Oct. 5	—	Period 404·7 days.
RScuti ...	May 13	June 16	
	July 9	Aug. 21	
	Sept. 20	Oct. 13	
	Nov. 4	—	

Wilhelm Schur gives an opposition ephemeris for Arethusa. The opposition happening Jan. 21, the R. A. being then

8 . 2 . 52.78 and Dec. + 4 . 1 . 54

The fourth line of the spectrum of the nebula in Orion, by D'Arrest. The author refers to a note on this line by Dr. Vogel in Astron. Nach., No. 1963, mentioning the fact that the fourth line of the nebula coincided with Hγ, and goes on to mention that this line was known to Huggins in 1864, and by Capt. Herschell in 1868, and was brought to the notice of the Royal Society in 1872 by Huggins. Its wave-length he gives as 4343. The author also mentions a peculiarity in the spectrum of B. Durchm + 22° 4203.—Mr. J. Birmingham states in a note that he believes that 252 Schjellerup has disappeared, and he thinks this star a remarkable variable.

Medizinische Jahrbucher: k.k. Gesellsch. d. Aerzte: Vienna, 1873, Heft 3 and 4.—The last two issues of this quarterly journal for 1873 contain the following papers of scientific interest:—Researches into the minute structure of the Tendon, by Arnold Spina, with an illustration; the Nerves of the Knee-joint in the Rabbit, by Dr. C. Nicoladoni (with a figure); contributions to the Anatomy of the Human Bladder, by Dr. Gustav Jurie; Quarantine in Cholera, a report to the International Medical Congress, by Dr. Oser.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, Jan. 20.—Prof. Newton, F.R.S., vice-president, in the chair.—Mr. Sclater exhibited two skulls of Baird's Tapir (*Tapirus bairdi*) received from Mr. Constantine

Rickards, of Oaxaca, Mexico. The receipt of these specimens proved that this Tapir extended from Panama through Central America into Southern Mexico, and was probably the only species of this genus to be met with in America, north of the Panamanic Isthmus.—Mr. Sclater also exhibited and made remarks on skulls of *Ovis arkar*, from the Altai Mountains, and the stuffed skin of a specimen of the Wild Ibex of Crete.—Mr. E. Ward exhibited two feet of a Fawn, the mother of which had had double hind feet, and had for several years brought forth fawns having the same malformation.—A communication was read from Dr. O. Finsch containing a description of an apparently new species of Parrot from Western Peru, which was proposed to be called *Psittacula andicola*.—A second paper by Dr. Finsch contained the description of a new species of Fruit Pigeon from the Pacific Island of Rapa or Opara. This unique specimen had been sent to the author by Mr. F. W. Hutton, of Otago, New Zealand, after whom it was proposed to name the bird *Ptilonopus huttoni*.—A note was read by Major O. B. C. St. John, on the locality of the Beatrix Antelope (*Oryx beatrix*), which was believed to be the south of Muscat.—Mr. Edward R. Alston read the description of a new Bat of the genus *Pteropus*, which had been sent to him from Samoa for identification by the Rev. S. J. Whitmee. Mr. Alton proposed to call this species *Pteropus whitmeei*.—A communication was read from Mr. A. G. Butler, containing a list of the species of *Fulgora*, with descriptions of three new species in the collection of the British Museum.—A communication was read from Mr. Herbert Druce, containing an account of the Lepidopterous Insects collected by Mr. E. Layard, at Chentaboon and Mahconchaisee, Siam, with descriptions of new species.

Meteorological Society, Jan. 21.—Dr. R. J. Mann, president, in the chair.—The date of the annual meeting having been altered in June last to January, the report of the Council was shorter than usual. The Council have been making efforts to render the operations of the Society more extended. They took advantage of the presence of their foreign secretary, Mr. Scott, as one of the delegates from this country, at the Meteorological Congress at Vienna, to request him to represent the Society. The congress was duly held from September 1st to 16th, when Mr. Scott presented a report on the replies received in answer to a series of questions which the Council issued to the Fellows on several important points in connection with the hours of observation, instruments, &c., and which has been printed in the report of the Congress. The report concluded by stating that the Council have to mark, with some measure of satisfaction, the maintenance of the numbers of the Society during a somewhat critical and transitional period in its history, when changes of detail have been entered upon with a view to increased energy of action, and when the beneficial results of the alterations have not yet had time to be practically felt. The president then delivered his address. After alluding to the loss which the Society had recently sustained in the death of Mr. Beardmore, and marking the place that gentleman had filled as president at the transition era of the Society's history, the president drew attention to a misconception that is largely entertained of the primary aims of meteorological science, and pointed out that desirable as a comprehensive and reliable theory is, the immediate object of observational work is none the less certainly the determination of climate in different regions of the earth, and the investigation of the method by which the action of the great natural forces that determine temperature, direction and force of wind, and rainfall, is influenced by physical conditions. This argument was supported by evidence of the valuable practical results that are secured in these particulars by the labours of meteorologists. The address then proceeded to note briefly the chief landmarks that had marked the yearly progress of meteorological science since the period of Mr. Beardmore's presidency, when the Society, in its remodelled form, had just reached the half-way stage of its history. From this review it appeared that the photographic method of record has been largely extended; that the discussion of the Greenwich observations from 1848 to 1868 is being steadily pursued; that the influence of meteorological conditions upon the public health is carefully investigated in the metropolitan district; that telegraphic intercommunication of meteorological aspects is now regularly made throughout the United States of America; and from the Meteorological Office of London through England, and through France to the shores of the North Sea and Baltic in one direction, and to Corunna in the other; and that storm-warnings are displayed and fishermen's barometers maintained at 129 coast stations. The methodical investigation of the connection of sun-spot periods with atmospheric phenomena, such as rainfall, auroræ, and magnetic storms and earth-currents was also alluded to. Among other topics of special interest connected with the recent progress of meteorological science, the president dwelt with special favour and affection upon the discovery and establishment of Buys Ballot's law and Mr. T. Stevenson's barometric gradient; the extension of the influence which indicates this law to the great vertical circulation of the oceans, traced out by Dr. Carpenter and Prof. Wyville Thomson; the marine charts, and especially the mapping out of the mid-Atlantic area of the Doldrum calms, by Capt. Toynbee; Mr. Meldrum's Mauritius investigations of the movements of the cyclones of the Indian Ocean; the daily weather charts of the Meteorological Office; the deleterious physiological influence of the recent heavy fog in London; Mr. Symond's examination of the rainfall of the British Islands, with a volunteer staff of 1,700 observers systematically distributed; Mr. Draper's deductions as to the invariability of the climate of the United States, and to the orderly progress of storms across the entire breadth of the Atlantic; the establishment and work of International Meteorological conferences; and the barometric compensation of clock-rates for altering pressure and resistance of the atmosphere.—The following gentlemen were elected officers and council for the ensuing year:—President—R. J. Mann, M.D., F.R.A.S. Vice-Presidents—C. Brooke, M.A., F.R.S.; G. Dines; H. S. Eaton, M.A.; Lieut.-Col. A. Strange, F.R.S. Treasurer—H. Perigal, F.R.A.S. Trustees—Sir Antonio Brady, F.G.S.; S. W. Silver, F.R.G.S. Secretaries—G. J. Symons; J. W. Tripe, M.D. Foreign Secretary—Robert H. Scott, F.R.S. Council—Percy Bicknell; A. Brewin, F.R.A.S.; C. O. F. Cator, M.A.; R. Field, B.A., Assoc. Inst. C.E.; F. Gaster; J. K. Laughton, F.R.A.S.; R. J. Lecky, F.R.A.S.; W. C. Nash; Rev. S. J. Perry, F.R.A.S.; Capt. H. Toynbee, F.R.A.S.; C. V. Walker, F.R.S.; E. O. Wildman Whitehouse, Assoc. Inst. C.E.

Entomological Society, Jan. 5.—Prof. Westwood, president, in the chair.—Mr. Meldola exhibited some photographs of minute insects taken with the camera-obscura and microscope.—Mr. McLachlan called attention to a paper in the last part of the "Annales de la Soc. Ent. de France," by M. Bar and Dr. Laboulbène on a species of moth belonging to the *Bombycidæ*, described and figured by M. Bar as *Palustra laboulbenei*, and of very extraordinary habits, the larva being aquatic, living in the canals of the sugar plantations in Cayenne, and feeding upon an aquatic plant. The hairy larva breathed by means of small spiracles, a supply of air being apparently entangled in its hairs.—Mr. Butler remarked that Mr. J. V. Riley, in the *Journal* of the St. Louis Academy of Sciences, had alluded to *Apatura lycaon* Fab. and *A. hyrse* Fab. as distinct species, whereas he believed them to be identical with the *A. alicia* Edwards.—A letter from M. Ernest Olivier stated that he had recently come into possession of a portion of the collection of his grandfather, the celebrated French coleopterist, and that he would be happy to show it to any entomologist who might desire to examine the types.—Mr. Smith communicated a paper on the hymenopterous genus *Xylocopa*; and Mr. D. Sharp a paper on the *Pselaphidæ* and *Scydmænidæ* of Japan, from the collections of Mr. George Lewis.

Edinburgh

Royal Society, Jan. 19.—Principal Sir Alex. Grant, vice-president, in the chair.—The following communications were read:—Additional remarks on the fossil trees of Craigleith quarry, by Sir Robert Christison, Bart.—On a method of demonstrating the relations of the convolutions of the brain to the surface of the head, by Prof. Turner.—On some peculiarities in the embryogeny of *Tropæolum speciosum*, Endl. and Poepp., and *T. peregrinum*, L., by Prof. Alex. Dickson.—Notes on Mr. Sang's communication of April 7, 1873, on a singular property possessed by the fluid enclosed in crystal cavities. (1) By Prof. Tait; (2) By Prof. Swan.—Preliminary note on the sense of rotation, and the function of the semicircular canals of the internal ear, by Prof. Crum-Brown.

Dublin

Royal Zoological Society of Ireland, Jan. 13.—His Excellency, Earl Spencer, president, in the chair.—The report was read by the Rev. Prof. Haughton, M.D., secretary, who referred, among other matters, to the loss by death of an old pelican (*Pelecanus onocrotalus*) "who had been domiciled in the Gardens for forty-two years. He was supposed to have been eight years of age upon his admission, so that he was a bird of over fifty at the time of his death.—Every effort was made to prolong his valuable existence by feeding him on live eels and whisky punch; but old age prevailed, and he died peaceably on the approach of the cold weather. He drank the punch with great relish; in fact he had resided so long in Dublin that it must have come naturally to him, and this and the live eels prolonged his life for at least a fortnight." We are sorry to see the funds of the society are not in a very thriving condition.

Vienna

I. R. Geological Institute, Nov. 18.—The director, F. v. Hauer, stated that towards the end of the international exhibition he had asked almost all Austrian and a great many of the foreign exhibitors of ores, coals, or other useful minerals, to present these objects to the museum of the Institute. This request was very successful, more than a hundred exhibitors have offered the whole or parts of their expositions to the Institute, and the number of the donators is increasing still every day. Out of the objects obtained in this way will be formed a particular collection of useful minerals from Austria and from abroad, embracing ores, coals, salts, building-stones, all sorts of useful clays, limestones, &c., minerals used for colours, for dung, &c. This collection, which will contain generally specimens of large size, will form quite a new and, as he hopes, very interesting branch of the museum.—Dr. R. v. Drasche: Geological observations on a journey to the west coast of Spitzbergen during the summer of 1873. The journey was made in a schooner chartered especially for the purpose. Dr. Drasche left Tromsoe on June 30, went to the north till Amsterdamo in 79° 45′ N. lat., and returned to Hammerfest on August 27. Many very interesting observations and large collections of rocks and fossils are the fruit of the expedition. Here we will give only a few particulars: On the flat land which forms the eastern part of Dansko and Amsterdamo, Dr. Drasche found very large masses of erratic boulders, which consist partly of certain varieties of granites, syenites, and gneiss, unobserved till now on the shores of Spitzbergen. Probably they are brought down by glaciers out of the interior of the island. The Hekla Hook formation (Nordenskiöld), which is probably Devonian, is formed in the Belsund by black limestones and chloritic slates, which resemble very much the Taunus-slates. The mountain limestone formation is developed in large masses and with many fossils in the Belsund and on the island of Axelo. On Cape Staratschin the mountain limestone alternates with very fine Hyperith. The Triassic formation was accurately studied on Cape Thordsen; it contains here many Ceratites, Nautilus, Halobia, &c., besides which were found the remains of a saurian. The Jurassic and the Tertiary formation are formed by marly beds in the Ice-fiord, and can scarcely be separated from each other whenever they do not contain fossils. On the Goose Island in the Ice-fiord Dr. Drasche found proofs of a very recent levation of the ground. From 8 ft. to 10 ft. above the highest level of the sea the ground is covered with shells of *Mytilus edulis*, which have preserved perfectly well their bluish colour.—M. Niedzwiedzki has examined the microscopical structure of a large number of the eruptive rocks of the Banat which by Prof. Cotta had been united under the name "Banatites." He found that the mineral which mostly prevails is a plagioklastic feldspar out of the Andesin series. He concludes therefore that the rocks from Dognacska, Oravitza, and Csiklova, which hitherto generally had been called Syenites, are rather to be considered as quartz-bearing Diorites. The basalt, which transverses in small veins the "Banatite" from Moldova contains, besides a vitreous ground-mass, only Augite, Olivine, Biotite, and Magnetite, and therefore cannot be united with any one of the great divisions of the basalt rocks.

Philadelphia

Academy of Natural Sciences, Sept. 23.—"Exceptional Conditions in the Vegetation of Forest Seed," by Mr. Thomas Meehan.—Mr. T. Meehan also presented some specimens of a malformed clover, *Trifolium pratense*.—Mr. Gentry made the following remarks regarding the nest of *Vireo solitarius* (Vieill). Audubon, in describing the nest of *Vireo solitarius* (Vieill.) affirms it "is prettily constructed and fixed in a partially pensile manner between two twigs of a low bush, on a branch running horizontally from the main stem, and formed externally of grey lichens, slightly put together, and lined with hair chiefly from the deer and racoon." My experience has been quite different. I have five nests of this species, four of which are perfectly similar in structure; the remaining one formed of the culms of a species of *Aira*, constituting an exceptional case, and the only one that has ever fallen under my notice. They are all shallow, loose in texture, scarcely surviving the season for which they were designed, and placed between two twigs of a cedar or a maple tree at a considerable elevation from the ground, on a branch nearly horizontal to the main axis. They are built entirely of clusters of male flowers of *Quercus palustris*, which, having performed their allotted function, don their brownish hue at the very period when they can be utilised. Here is evidently a change within a moderately short period, rendered necessary by external causes. This necessity may have grown out of inability to procure the favourite materials, or a desire for self-preservation. I am satisfied, however, that the former has not been the leading one, but that self-preservation has operated in this case for individual and family good.

Paris

Academy of Sciences, Jan. 19.—M. Bertrand in the chair.—The following papers were read:—On the theory of shocks, by M. H. Resal.—Memoir on the temperatures observed by means of electric thermometers, at the Jardin des Plantes, from the surface of the ground to a depth of thirty-six metres during the meteorological year 1873, by M.M. Becquerel and E. Becquerel.—On the formation, in the gaseous state, of the oxides of nitrogen from their elements by means of heat, by M. Berthelot. The paper dealt with the thermal phenomena accompanying these formations.—On the discovery of a deposit of bismuth in France, by M. Ad. Carnot.—On organogenesis compared with androgenesis, &c., by M. Ad. Chatin.—On the geometrical properties of rational fractions, by M. F. Lucas.—On the vibratory movement of an elastic wire fastened to a tuning-fork, by M. E. Gripon.—On the measurement of the magnetic movement in very small magnetised needles, by M. E. Bouty.—On the modes of forming black phosphorus, by M. E. Ritter. The author stated that certain samples of phosphorus refuse to blacken when heated to 70°, while others show that property. The latter contain a trace of arsenic, and to arsenide of phosphorus the author attributed the blackening. He gave analysis of the arsenide which agree with the formula As_2P.—On the existence of two isomeric modifications of anhydrous sodic sulphate, by M. L. C. de Coppet.—On the solubility of succinic acid in water, by M. E. Bourgoin.—On a new cause of spontaneous gangrene accompanied by obliteration of the capillary arteries, by M. L. Tripier.—On the pathological development of the eye in the so-called telescope fish, by M. G. Camuset.—During the meeting, the places of MM. Petit and Valz, in the Astronomical section, were filled up. For the first, Dr. Huggins obtained 38 votes, M. Stéphan 2, and Mr. Newcomb 1; for the second, Mr. Newcomb obtained 46 votes, and M. Stéphan, 1; Messrs. Huggins and Newcomb were accordingly elected.

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THURSDAY, FEBRUARY 5, 1874

SCIENTIFIC WORTHIES

II.—THOMAS HENRY HUXLEY

THOMAS HENRY HUXLEY was born at Ealing, on May 4, 1825. With the exception of two and-a-half years spent at the semi-public school at Ealing, of which his father was one of the masters, his education was carried on at home, and in his later boyhood, was chiefly the result of his own efforts. In 1842 he entered the medical school attached to Charing Cross Hospital, where, at that time, Mr. Wharton Jones, distinguished alike as a physiologist and oculist, was lecturing on Physiology. In 1845 Mr. Huxley passed the first M.B. examination at the University of London, and was placed second in the list of honours for Anatomy and Physiology, the first place being given to Dr. Ransome, now of Nottingham. After some experience of the duties of his profession among the poor of London, in 1846 he joined the medical service of the Royal Navy, and proceeded to Haslar Hospital. From thence he was selected, through the influence of the distinguished Arctic traveller and naturalist, Sir John Richardson, to occupy the post of Assistant-Surgeon to H.M.S. *Rattlesnake*, then about to proceed on a surveying voyage in the Southern Seas. The *Rattlesnake*, commanded by Captain Owen Stanley, with Mr. MacGillivray as naturalist, sailed from England in the winter of 1846. She surveyed the Inner Route between the Barrier Reef and the East Coast of Australia and New Guinea, and after making a voyage of circumnavigation, returned to England in November 1850. During this period Mr. Huxley investigated with a success known to all naturalists, the fauna of the seas which he traversed, and sent home several communications, some of which were published in the "Philosophical Transactions" of the Royal Society. The first which so appeared, presented by the late Bishop of Norwich, and read June 21, 1849, bears the title "On the Anatomy and Affinities of the Family of the Medusæ." This was, however, not Mr. Huxley's first scientific effort. While yet a student at Charing Cross Hospital, he had sent a brief notice to the *Medical Times and Gazette*, of that layer in the root-sheath of hair which has since borne the name of Huxley's Layer. Shortly after his return he was (June 1851) elected a Fellow of the Royal Society.

In 1853 Mr. Huxley, after vainly endeavouring to obtain the publication by the Government of a part of the work done during his voyage, left the naval service, and in 1854, on the removal of Edward Forbes from the Government School of Mines to the chair of Natural History at Edinburgh, succeeded his distinguished friend as Professor of Natural History in that institution, a post which he has continued to hold up to the present day. Since that time Mr. Huxley has lived in London a life of continued and brilliant labour. From 1863 to 1869 he held the post of Hunterian Professor at the Royal College of Surgeons. He was twice chosen Fullerian Professor of Physiology at the Royal Institution of Great Britain. In 1869 and 1870 he was President of the Geological Society, having previously served as Secretary. During the same period he was President of the Ethnological Society. In 1870 he filled the office of President of the British Association for the Advancement of Science, and in 1872 was elected Secretary to the Royal Society. He has been elected a corresponding member of the Academies of Berlin, Munich, St. Petersburg, and of other foreign scientific societies, has received honorary degrees from the Universities of Breslau and Edinburgh, and last year was presented with the Order of the Northern Star by the King of Sweden. Since 1870 he has been one of the Members of the Royal Commission on Scientific Instruction and the Advancement of Science. From 1870 to 1872 he served on the London School Board as one of the members for Marylebone, and during that time was Chairman of the Education Committee which arranged the scheme of education adopted in the Board Schools. In 1872 he was elected Lord Rector of the University of Aberdeen.

In this skeleton narrative of the career of this distinguished naturalist we have purposely omitted any list or any critical estimate of his writings; but we have great pleasure in laying before our readers, as a token of what is thought of him by those who are labouring in the same field of Science, the following communication from one who ranks in his own country as well as among ourselves as one of the very first of German naturalists.

The more general, year by year, the interest taken by all educated people in the progress of Natural Science, and the wider, day by day, the field of Science, the more difficult is it for the man of science himself to keep pace with all the advances made—the smaller becomes the number of those who are able to take a bird's-eye view of the whole field of science, and in whose minds the higher interest of the philosophical importance of the whole is not lost amid a crowd of fascinating particulars. Indeed if at the present moment we run over the names distinguished in the several sciences into which Natural Knowledge may be divided —in Physics, in Chemistry, in Botany, in Zoology—we find but few investigators who can be said to have thoroughly mastered the whole range of any one of them. Among these few we must place Thomas Henry Huxley, the distinguished British investigator, who at the present time justly ranks as the first zoologist among his countrymen. When we say the first zoologist, we give the widest and fullest signification to the word "zoology" which the latest developments of this science demand. Zoology is, in this sense, the entire biology of animals; and we accordingly consider as essential parts of it the whole field of Animal Morphology and Physiology, including not only Comparative Anatomy and Embryology, but also Systematic Zoology, Palæontology and Zoological Philosophy. We look upon it as a special merit in Prof. Huxley that he has a thoroughly broad conception of the science in which he labours, and that, with a most careful empirical acquaintance with individual phenomena, he combines a clear philosophical appreciation of general relations.

When we consider the long series of distinguished memoirs with which, during the last quarter of a century, Prof. Huxley has enriched zoological literature, we find that in each of the larger divisions of the animal kingdom we are indebted to him for important discoveries.

From the lowest animals, he has gradually extended his investigations up to the highest, and even to man. His earlier labours were, for the most part, occupied with the lower marine animals, especially with the pelagic organisms swimming at the surface of the open sea. He availed himself of an excellent opportunity for the study of these, when on board H.M.S. *Rattlesnake* on a voyage of circumnavigation, which took him to many most interesting parts of tropical oceans little investigated, previously, by the zoologist; especially the coasts of Australia. Here he was able to observe, in their living state, a host of lower pelagic animals, some of which had not at all been studied, others but imperfectly. In the Protozoa, he was the first to lead us to satisfactory conclusions concerning the nature of the puzzling Thalassicollidæ and Sphærozoida. Our knowledge of Zoophytes has been greatly extended by his splendid work on "Oceanic Hydrozoa," in which, chiefly, the remarkable Siphonophora, with their largely developed polymorphism and the instructive division of labour in their individual organs, are described with very great accuracy.

Already in his first work "On the Anatomy and the Affinities of the Medusæ," 1849, he directed attention to the very important point, that the body of these animals is constructed of two cell layers—of the Ectoderm and the Endoderm—and that these, physiologically and morphologically, may be compared to the two germinal layers of the higher animals. He has made us better acquainted with several interesting members of the class Vermes, Sagitta, Lacinularia, some lower Annulosa, &c. He was the first to point out the affinities of Echinodermata with Vermes. In opposition to the old view, that the Echinodermata belong to the Radiata, and, on account of their radial type, are to be classed with corals, medusæ, &c., Huxley showed that the whole organisation of the former is essentially different from that of the latter, and that the Echinoderms are more nearly related, morphologically, to worms. Further he has essentially enlarged our knowledge of the important group of Tunicata by his researches on the Ascidians, Appendicularia, Pyrosoma, Doliolum, Salpa, &c.

Many important advances in the morphology of the Mollusca and Arthropoda are also due to him. Thus, *e.g.*, he has greatly elucidated the controverted subject of the homology of regions of the body in the various classes of Mollusca. He has considered the generation of vinefretters from quite a new point of view, based on his "genealogical conception of animal Individuality." But it is the comparative anatomy and classification of the Vertebrata which, during the last ten years, he has especially studied and advanced. His excellent "Lectures on the Elements of Comparative Anatomy" afford abundant proof of this, to say nothing of his numerous important monographs, especially those on living and extinct fish, amphibians, reptiles, birds, and mammals.

Huxley's works on the comparative anatomy of the Vertebrata are the only ones which can be compared with the otherwise incomparable investigations of Carl Gegenbaur. These two inquirers exhibit, particularly in their peculiar scientific development, many points of relationship. They both belong to that small circle of morphologists which is marked by the names of Caspar Friedrich Wolff, George Cuvier, Wolfgang Goethe, Johannes Müller, and Carl Ernst von Baer.

More important than any of the individual discoveries which are contained in Huxley's numerous less and greater researches on the most widely different animals are the profound and truly philosophical conceptions which have guided him in his inquiries, have always enabled him to distinguish the essential from the unessential, and to value special empirical facts, chiefly as a means of arriving at general ideas. Those views of the two germinal layers of animals which were published as early as 1849 belong to the most important generalisations of comparative anatomy; they already contain in germ, the idea of the "perfect homology of the two primary germinal layers through the whole series of animals (except protozoa)," which first found its complete expression, a short time since, in the "Gastræa theory;" also his researches on animal individuality, his treatment of the celebrated vertebral theory of the skull, in which he first opened out the right track, following which Carl Gegenbaur has recently solved in so brilliant a manner this important problem, and above all his exposition of the Theory of Descent and its consequences, belong to this class. After Charles Darwin had, in 1859, reconstructed this most important biological theory, and by his epoch-making theory of Natural Selection placed it on an entirely new foundation, Huxley was the first who extended it to man, and in 1863, in his celebrated three Lectures on "Man's Place in Nature," admirably worked out its most important developments. With luminous clearness, and convincing certainty, he has here established the fundamental law, that, in every respect, the anatomical differences between man and the highest apes are of less value than those between the highest and the lowest apes. Especially weighty is the evidence adduced, for this law, in the most important of all organs, the brain; and by this, the objections of Prof. Richard Owen are, at the same time, thoroughly refuted. Not only has the Evolution Theory received from Prof. Huxley a complete demonstration of its immense importance, not only has it been largely advanced by his valuable comparative researches, but its spread among the general public has been largely due to his well-known popular writings. In these he has accomplished the difficult task of rendering most fully and clearly intelligible, to an educated public of very various ranks, the highest problems of philosophical Biology. From the lowest to the highest organisms, from Bathybius up to man, he has elucidated the connecting law of development.

In these several ways he has, in the struggle for truth, rendered Science a service which must ever rank as one of the highest of his many and great scientific merits.

ERNST HAECKEL

ZOOLOGICAL NOMENCLATURE

The Object and Method of Zoological Nomenclature. By David Sharp. (E. W. Janson and Williams and Norgate, 1873.) Pp. 39.

ZOOLOGISTS and botanists universally adopt what is termed the binomial system of nomenclature invented by Linnæus. The essential principle of this system is, that every species of animal or plant is to have a name made up of two words, the second word—which is called the specific or trivial name, having exclusive reference to the species itself, the first word—which is called the generic name, indicating the genus, or small natural group, which comprises the species in question along with others. Thus the cat, the tiger, and the lion, belonging to one genus or small natural group of closely-allied animals, are called respectively, *Felis cattus*, *Felis tigris*, and *Felis leo*. The name of each species, therefore, shows us what group it belongs to, and thus gives us a clue to its affinities; and the system of nomenclature is to this extent classificatory. But, as the true natural grouping of species has not yet been agreed upon by naturalists, and genera have been in a state of incessant change from the time of Linnæus to the present hour (or for about a century), the names of an immense number of species have been repeatedly altered; and one of the first requisites of a good system of nomenclature—that the same object shall always be known by the same name—has been lost, in the attempt to make the name a guide to classification, while the classification itself has ever been fluctuating and still remains unsettled. As an example let us take the Snowy Owl. This has been placed by different ornithological authors in the genera Bubo, Strix, Noctua, Nyctea, Syrinium, and Surnia; and at the same time, owing to carelessness or error, a number of different specific or trival names have also been used, such as *scandiaca*, *artica*, *nivea*, *erminea*, *candida*, and *nyctea*; and the various combinations of these two sets of names have led to the use of about twenty distinct appellations for this single species of bird. This example is by no means a very extreme one; and it represents what occurs over and over again, in varying degrees, in every department of zoology and botany.

In order to determine in every case which of the names which are or have been in use is the right name, and so arrive at uniformity of nomenclature, certain rules have been pretty generally agreed upon, the most important of which is that of "priority." This means that the first name given to a species is to be the name used, even when it has never come into general use, but is now discovered in some scarce volume dated 80 or 100 years ago. But this absolute law of priority only applies to the specific or trivial name; in the case of the generic name no such absolute priority has been thought possible, because the genera of the old authors were very extensive groups, which have now been divided, in some cases into hundreds of genera. This process of division has, however, gone on step by step, one author dividing an old genus into three or four new ones, with new names; another dividing some of these still further, with more new names; another perhaps discovering that these genera were not natural, and grouping the species into genera on altogether different principles, and again giving new names. Genera have been thus subdivided to such an extent that the owls, for example, which Linnæus classed as one genus, now number more than fifty; and the ten British owls have to be placed in nine distinct genera.

In the very ingenious and careful essay which has led to these remarks, Mr. David Sharp, a well-known entomologist, advocates a mode of attaining the great desideratum of naturalists—a fixed and uniform nomenclature of species—which has not, so far as we are aware, been suggested before, although it is at once simple and logical. He proposes that, not merely one-half, but the entire name of every species once given, should be inviolable, until by general consent some permanent classificatory system of naming species, analogous to that used in chemistry, is arrived at. The insect named by Linnæus *Papilio dido* should, for example, retain that name, although it must find its classificatory place in the genus Colœnis and the family Nymphalidæ; while the glossy starling of the East should retain the name *Turdus cantor*, given to it by Gmelin, although it is no thrush, and belongs to the genus Calornis. The name would thus remain fixed, however the place of the species in our classifications might be changed; and the very errors of the original describers might help us to remember the object referred to by directing our attention to the cause of their error in classifying it. A beginner might, it is true, be misled, but the mistake once pointed out, the very inappropriateness of the name would serve us an aid to memory, as in the well-known "*lucus a non lucendo*." It is also pointed out that the value of the binomial nomenclature as a guide to the affinities of a species is now almost lost, owing to the minute subdivision of the old well-marked groups and the consequent multiplication of genera. No one can remember the names of all the genera of beetles now that they exceed ten thousand, unless he devotes his life to their study; and even then the fixity of the names of all the old and well-known species would be a great help in the study of new classifications, or the use of modern catalogues.

A great evil of the present system is, that while professing to keep the specific or trivial name inviolable, it often compels an entire change of name. This happens whenever, by a new arrangement, a species has to be placed in a genus which already contains the same trivial name. Two species thus come to have the same name, and one of these must be wholly changed. The evil of this system of perpetually changing names is not so much the trouble it gives us to find out what object a name really refers to (though that is serious) as the enormous waste of labour involved in the elaborate working out of synonomy, rendered many fold more difficult by the complication of changes in both the generic and specific names, from a variety of causes. These difficulties are much greater in the case of genera than in that of species; and this portion of synonomy would be almost got rid of if it were decided that the first binomial name given to a species should never be changed. We should then avoid the absurdity of having hundreds of familiar names abolished, because a mere compiler of an early catalogue, who had perhaps never seen the objects themselves, divided them up almost at random into a number of named groups, or because some modern student thinks it advisable to split up every large genus into dozens of smaller ones.

These appear to be weighty arguments in favour of Mr. Sharp's proposal, yet we are far from thinking that it will be adopted. For, after all, the changed names are but few in comparison with those which remain unchanged for considerable periods; and the charm of a nomenclature which is to a considerable extent classificatory is so great, that most naturalists will strongly object to giving it up. So long as the old name keeps within the bounds of the modern family (which is in most cases a stable and well-defined group) there might be little objection to retaining it; but when it leads to the use of a name indicating a distinct and often quite unrelated family—as *Silpha scabra* for one of the Lamellicornes, (*Trox scabra*) in the example given by Mr. Sharp—the system will, we apprehend, be almost unanimously rejected.

Many minor details of nomenclature are discussed in the essay before us, and on some of these the author's views are more likely to meet ultimately with general acceptance. He objects strongly, for example, to the common practice among classical purists of altering all names which they consider to be not properly spelt or not constructed on true classical principles. For, as he justly remarks, the emenders can give no guarantee that their alterations will be permanently accepted, since others may come after them who will have different views as to classical orthography and propriety of nomenclature. He points in particular to the inconvenience of placing an H before many names which were originally spelt with a vowel, thus altering their places in an alphabetical arrangement, and creating a synonym for no useful purpose whatever.

Although it appears to us pretty certain that the plan of returning to the first generic name given to a species will not be adopted, the proposal to do so may lead to a reconsideration of the practice of applying the law of priority to generic names, as all are agreed it must be applied to specific or trivial names. If the generic part of the name may be altered any number of times in accordance with altered views as to classification, the principle of priority in the mere name is so totally given up, that it seems absurd to use it for the purpose of resuscitating the obsolete appellations of early writers. When an author is admitted to have defined a natural genus, he should have full power to give a name to that genus, because it is really a new thing; and it is both illogical and inconvenient to reject his name because some former writer has given another name to a group, not the same, but which merely happened to contain some one or more of the same species. Again, we think Mr. Sharp's arguments suggest the advisability of opposing the splitting up of large genera into many smaller ones otherwise than provisionally; the old generic name continuing to be used till there is a concurrence of opinion as to the necessity of adopting the new ones. The older authors were often modest enough to do this; indicating natural divisions of large genera, but not naming them; whereas modern naturalists, as a rule, feel bound to give a new name to every fragment they can split off an established genus.

It appears, then, to the present writer, that the plan best adapted to lead speedily to a fixed nomenclature, and at the same time one that will least offend the prejudices of zoologists, is as follows:—

1. To adopt, absolutely and without exception, the principle of priority as regards specific or trivial names.

2. To adopt the same principle for genera only so long as the generic character or definition of the genus remains unaltered; but whenever an original investigator defines a genus more completely than has been done before, he is to be left free to name it as he pleases. Every consideration of utility and common sense will of course lead him to retain a name already in use when the new genus does not materially differ from an older one: but of that he is alone the judge, and it should be absolutely forbidden to any third party to say that a name so given must be changed.

3. Whenever genera which are widely recognised are split up into a number of proposed smaller ones, the old generic name should continue in use till further investigation determines whether the new groups are sufficiently well defined and natural to supplant the old one.

In conclusion, it may be suggested that if zoologists who have paid attention to this subject would, after a careful consideration of Mr. Sharp's paper, state their own conclusions in the form of short propositions, accompanied by their reasons for them, a notion might be obtained, not only as to which system is intrinsically the best, but, what is of equal or perhaps greater importance, which is most likely to command general assent.

ALFRED R. WALLACE

RESULTS OF THE FRENCH SCIENTIFIC MISSION TO MEXICO

Mission Scientifique au Mexique et dans l'Amerique Centrale. Recherches Zoologiques publiées sous la direction de M. Milne-Edwards. Livraisons 4. (Paris: 1870-72.)

THE ill-fated attempt of the Second Empire to establish Imperialism in Mexico has had at least one good result in the work now before us, in which the labours of a Scientific Mission originally sent out under the shadow of the French Army are given to the world. The materials accumulated by M. Bocourt and his Fellow-Naturalists, were deposited in the National Museum of the Jardin des Plantes, and the elaboration of them entrusted to special workers in the different branches of science. In 1870 three livraisons were issued, each forming the commencement of a separate section of the work, as planned out under the direction of M. Milne-Edwards. These relate to the terrestrial and fluviatile Molluscs, by MM. Fischer and Crosse; to the Orthopterous Insects and Myriapods, by M. Henri de Saussure; and to the Reptiles and Batrachians, by MM. Auguste Duméril and Bocourt. The fall of the Empire and German occupation stopped the immediate progress of the work, but we are glad to see it has now been resumed. A second livraison of the section devoted to the Myriapods, prepared by MM. H. de Saussure and Humbert, has been lately issued, and we believe it is fully intended to bring the work to a conclusion. It will be observed that authors engaged on the various sections are all well-known authorities on the subjects of which they treat, and that the figures and illustrations are of an elaborate character. We are the more glad to call the attention of our readers to the revival of this work, because it does not appear to be very generally known to naturalists, and because it has lately been the subject of a most unjustifiable attack in an English scientific periodical.* After a general condemnation of the work we are there informed that it is "a lamentable exhibition of the very backward state of zoological science in the French capital." As to the justice of this remark we need only appeal to the recent numbers of the "Annales des Sciences Naturelles" and the "Nouvelles Annales du Musée," which are replete with zoological memoirs of the highest interest, and to the great work on fossil birds, by Alphonse Milne-Edwards, recently completed, which is alone sufficient to refute such a sweeping accusation. That the spirit of scientific enterprise is still alive in France is, moreover, sufficiently manifest by the grand researches of Père David in Chinese Tibet, and of Grandidier in Madagascar, while there is certainly no lack of scientific experts to bring their discoveries before the public. A more baseless and unjust attack was certainly never penned against the savants of a sister nation.

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* Ann. Nat. Hist. for August 1873.

But when our English critic proceeds to suggest that either the general editor of the present work, Prof. Milne-Edwards, or the joint author of the part devoted to the Reptilia—the late Prof. Dumeril (for his remarks may be intended for either of these gentlemen)—has appropriated the funds devoted to its preparation and left the labour to be performed by some inferior subordinate, the matter becomes still more serious. It is, however, sufficient to reply that no sort of evidence is given to support these statements, and that the value of Dr. Gray's *ipse dixit* is not sufficiently appreciated among naturalists to induce them to accept such an impossible supposition.

OUR BOOK SHELF

Sahara and Lapland. Travels in the African Desert and the Polar World. By Count Goblet D'Alviella. Translated from the French by Mrs. Cashel Hoey. (London: Asher and Co., 1874.)

AT first sight it would seem that no two countries had less in common than the two about which this book is written; but Count D'Alviella ingeniously and correctly shows, in his thoughtful preface, that they, or rather the Lapps and Arabs, have many circumstances in common. These two peoples "lead the same vagabond existence; they live exclusively upon their herds, they carry with them all they have and that they possess, and they make analogous migrations at the changes of the seasons—the Lapps from the Swedish steppes to the Norwegian valleys, the Arabs from the plains of Sahara to the pastures of Tell. In this manner of life they have both acquired the same strength of constitution, or rather the same power of resisting such fatigue, privations, and weather as would kill the most robust European. . . . Both the Lapps and the Arabs—who are rather the slaves than the masters of Nature—owe their consciousness of isolation and powerlessness to the same superstitions, the same beliefs in spirits, to the 'evil eye,' in amulets, and in incantations. . . . Both races—restricted for centuries to a form of society unsuitable to any kind of progress—affect the same respect for the routine of their ancestors, and the same disdain for the arts of civilisation." The author concludes rightly, we think, that both peoples, incapable as they are of transformation or civilisation, are doomed to disappearance. Many attempts have been made by the Swedish and French Governments to get these nomads to settle down into civilised life, but invariably without success. The author, on the authority of M. Charles Martins, relates that the French Government gave to a number of the poorest Arabs of the Sahara some fertile fields with a ready-built village, and even a mosque in the middle of it. They reserved the houses for their flocks, and pitched their tents in the streets; until one day the nostalgia of the desert seized upon them, and they returned rejoicing to their wandering life.

Count D'Alviella tells the narrative of his travels in these two regions very pleasantly. He is a cheerful and observant and somewhat philosophic guide, and we can assure anyone who cares to buy this work, that he will get the value of his money in enjoyment and information. The narrative of the Lapland journey is especially interesting, and contains information about a people and a country that we believe many know but little about. Here will be found an account of the mode of life of a people that in many respects may be taken as the living type of the men who, ages ago, struggled for existence amid conditions very different from those which now obtain in Europe, and whose implements and remains come within the province, not of the historian, but of the geologist.

Mrs. Hoey deserves credit for her excellent translation. The volume contains a number of fairly executed illustrations.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

M. Barrande and Darwinism

IN the article in NATURE (vol. ix. p. 228) on M. Barrande's "Trilobites," published in 1871, several statements are made which require not only considerable modification as to the facts then known, but which are entirely misleading when made to appear to represent the state of our knowledge of these facts at the present time. M. Barrande is well known to be a determined opponent to the theory of evolution, and doubtless this strong bias has prevented him from seeing and accepting many facts which would otherwise, to so keen and careful an observer, have seemed inconsistent with such strong views. The list of fossils given by him from the Cambrian formation, and which is reproduced in NATURE, is most incomplete and inaccurate when made to refer to the Cambrian fauna of this country, as will be evident at once by referring to p. 249 of the same work, where a list of fossils discovered by me in the "Harlech or Superior Longmynd" group of Wales is given, and which includes several trilobites; and yet in the above-mentioned article it is stated "that no trace of a trilobite has been found in the Cambrian formation." Surely no English geologist will be bold enough to deny to the name Cambrian its right to these Harlech and Longmynd rocks, whatever else it may not be entitled to. Nor, indeed, did Sir R. Murchison and the Geological Survey ever attempt such a breach, and I cannot believe that M. Barrande has realised what such an assumption means, or what it would lead to; nor can I believe that it is possible for him to have followers in this country in such a "violation of historic truth," and, as observed by Prof. Sterry Hunt (in the *Canadian Naturalist*, vol. vi. p. 448), for no other reason than "that the primordial fauna has now been shown by Hicks to extend towards their base." Surely this country, which has not only given to scientific nomenclature the name Cambrian, but which has given to all other countries the groundwork upon which to build up theirs, should have a right to explain the succession in its own way, and especially when it is proved that its succession of these rocks is clearer and more natural than has been hitherto found to be the case in any other country. Indeed it is quite clear that M. Barrande has not yet succeeded, in Bohemia, in reaching this early fauna, and it is evident also that his first zone of life is only equal in order of appearance to the latter part of our *second zone*, and hence the mistake to attempt to correlate our fauna with his zone.

At St. David's in South Wales, the Cambrian of the Geological Survey, consisting of red, purple, and green rocks, attains a thickness of over five thousand feet of beds resting conformably, and of these beds over four thousand feet have yielded evidence, in the form of fossils, of life having existed in the seas in which they were deposited. The forms of life comprised annelids, brachiopods, pteropods, bivalve crustaceans, trilobites, and sponges, and I think it would be seen on examination that the picture offered by this early fauna is not one in discordance with Darwinism, as assumed in the article in question. But as M.

Barrande and the author of the article have restricted their remarks almost entirely to the trilobites, I will only ask to be allowed space to reply to the facts stated with regard to these forms of life. Trilobites have now been discovered as low down as 4,000 ft. in these red and green rocks at St. David's, that is, in the very earliest fauna known, and amongst them are forms hitherto not discovered in any other country; still if we are to believe with some that we are here near the beginning of life on the globe, or even of trilobitic life, we can expect but little evidence to support or to disprove Darwinism. For, considering the frequent changes in the sea bottom which must have taken place at this period, to produce at one time a shingle, then a sand or grit, and then a fine muddy deposit, and such beds frequently repeated, we cannot possibly expect that during all these physical changes an unbroken record of these forms should be preserved to us. No, rather we should expect to find that the necessary migrations would produce alterations in the forms, and that they should now and again return modified and altered in proportion to the time which had intervened and the circumstances which surrounded them. And this is really what we do find, and which is apparent at once to the palæontologist, who is prepared to allow and to recognise in these very marked physical changes a controlling influence capable of greatly affecting the life of the period. Again, can any one really believe, when thinking of the enormous time which must have elapsed during the accumulation of the great Laurentian series, and possibly of other series previous and succeeding, all antecedent to the time when the Cambrian fauna made its appearance, that the seas in which these were deposited were entirely barren of life? Surely not; therefore, why so readily jump at conclusions when there is so much room for doubt? Again, this Cambrian fauna is not without evidence in favour of evolution. Trilobites we know develope by increase of the body segments, and therefore M. Barrande says that the earliest trilobites should have the smallest number of segments in the thorax—"but that those of the primordial fauna are generally characterised by the opposite condition, while the number is less in those of the succeeding faunas." Now it does not seem to have occurred to M. Barrande that trilobites show every indication of having culminated at or about this period, that they had attained their maximum size and development, and that from this time they seem to have gradually diminished in size, and to have degenerated, doubtless much in the order in which they had previously progressed. This will explain also why the number of segments should, as he says, diminish in number in the genera of succeeding faunas. One of the very earliest trilobites we know of is the little *Agnostus*. It is also the simplest and apparently the most rudimentary of the group. It has no eyes, only two segments to the thorax, and usually an ill-defined glabella. In tracing a species of *Paradoxides* from the earliest stage upwards, I was struck with the very great resemblance which, at an early stage, it had to the little *Agnostus*. The glabella was indistinct, and much shorter in proportion to the length of the head than in the fully grown specimen, and the eye very faint, scarcely marked out, and the outline of the head more evenly rounded, with scarcely any indication of spines. Before the discovery of the Cambrian fauna at St. David's, no genus of trilobites had been found with four segments to the thorax, therefore we had to jump from one with two to one with six, as in *Trinucleus* or *Ampyx*. Now, however, since the discovery of *Microdiscus* with four segments, the gap has been filled up, and the genus, unfortunately for those holding M. Barrande's views, appears in our earliest fauna, and where the evolutionist would be most inclined to look for it. It is also a most interesting and instructive genus. It is somewhat larger than *Agnostus*, but like it, has no eyes. The *glabella* is better formed, more distinctly marked off from the cheeks, and instead of being irregularly grooved, as is usually the case with *Agnostus*, it is furrowed regularly as in an advanced stage in the development of *Paradoxides*. In the caudal portion the axis is partly divided into segments, and in one species the lateral lobes are slightly grooved as if into rudimentary pleuræ. It is very plentiful in the beds at St. David's, and since its discovery there, species have also been found in Canada and elsewhere.

From this stage forms have been found to represent every step in development as to the number of segments, and indeed often to show marked stages in other parts. *Anopolenus* is really a *Paradoxides* with enormous eyes, reaching to the hinder margin, and with several of the hinder pleuræ consolidated together to form a large spinous pygidium. Another *Paradoxides* has the eyes nearly as large as *Anopolenus*, but with a few more segments to the thorax, and a smaller pygidium. Other species show various gradations in the eyes and in the pygidium until we attain to *P. Davidis*, which has small eyes, a small pygidium, and the greatest number of thoracic segments. Indeed there are forms to represent almost every stage, and there can I think be no doubt that in the fauna of the Tremadoc group, which is separated from the earlier Cambrian by several thousand feet of deposits indicating a period of very shallow water in which large brachiopods and phyllopod crustaceans were the prevailing forms of life, we witness a return to very much the same conditions as existed in the earlier Cambrian periods, and with these conditions a fauna retaining a marked likeness to the earlier one, and in which the earlier types are almost reproduced, though of course greatly changed during their previous migrations. The *Niobe* (?) recently found in the Tremadoc rocks is truly a degraded *Paradoxides*, retaining the glabella and head spines, but with the rings of the thorax, excepting eight, consolidated together to form an enormous tail. Instead therefore of having here, as stated by M. Barrande, "a very important discord between Darwinism and facts," we find in these early faunas facts strongly favouring such a theory, and in support of evolution.

Hendon, Jan. 27 HENRY HICKS

ACCORDING to a notice in NATURE, vol. ix. p. 228, a distinguished continental naturalist finds an important discordance between Darwinism and certain facts connected with Trilobites and other fossil crustaceans. But his argument appears to be based on an assumption that we are acquainted with a "primordial fauna," that we are justified in dating the beginnings of life at or near some known geological period. This, however, the whole history of geology ought to make us less and less inclined to believe. It is one of those assumptions, essentially based on ignorance, on which so little dependence can rightly be placed. We have no right to call any fauna the *earliest*, merely because, as it happens, we know of none *earlier*.

A point is made of the fact that the earlier known Trilobites have more segments than the later, while individual Trilobites, as they develope, increase in number of their body segments. It may be granted at once that in this case the development of the individual is not an accurate picture of the past development of the species. But Frits Müller has long ago shown that we could not, on principles of Darwinism, expect it always to be so; and surely, if Trilobites have been gradually developed rather than abruptly created, there must have been Trilobites with *few* before there were Trilobites with *many* segments, so that after all, the development of the individual will carry us back to an early stage in the history of the family. It could scarcely be expected to give us all the alternations and complications which that history may have presented in its whole course.

Those who on other grounds accept the theory of evolution, far from finding any obstacle to it in the large number of genera of Silurian Trilobites, will consider the largeness of that number clear evidence that life in general, and Trilobite life in particular, must have flourished on the globe for a very long period prior to the Silurian age.

The argument that we do not find connecting links between different genera has little immediate force. It must await the verdict of time and further investigation. Of 252 species o Trilobites, 61 are assigned to England. The true reading of this piece of statistics must surely be that that which great research has done for a small area may be equalled, and far surpassed, when as close a scrutiny is applied to the whole available surface. If no gaps between species, and genera, and orders are filled by the results of such a search, then it will be time to say that we have "an important discord between Darwinism and facts."

Torquay, Jan, 27 THOMAS R. R. STEBBING

Perception in Lower Animals

I RELATE the following, as it has some bearing on a question lately ventilated in NATURE.

A friend and myself were watching on one occasion the actions of two half-bred Persian cats on seeing for the first time a freshly caught cobra, which had been placed in a wire-gauze covered box near the verandah. First of all one of the cats, a black one, stalked carefully up to the box in which the snake was keeping up a perpetual "swearing," with extended hood, and after a minute survey, crept away about 7 or 8 ft. off and sat down with its back to the snake. The other cat, a white one, now caught sight of the strange object, and, in a like stealthy manner, advanced to within a few inches of the gauze, and was in the act of examining the cobra, when my friend, to see the result of a sudden sound—for up to this time we had both been still as mice—moved his feet on the gravel. Had the effect been due to electricity, it would not have been more instantaneous, nor more startling. At the first grate of the pebbles the white cat flung himself backwards, tumbling—to use expressive terms—"heels over head" and "all of a heap" for about a couple of yards; whilst the black cat shot vertically upwards to somewhere near four feet in height, the impulse given by the spring of his hind legs being sufficient to throw these and his tail higher than his head.

Now both these cats are tame, and bold to such a degree that they reign supreme over all the dogs in the house, so that their great timidity on this occasion was evidently due to a perception of danger. I have since found, however, that all snakes are not equally feared by them. They will let the harmless green tree snake (*Passerita mycterizans*) twine round them without showing any signs of repugnance, and some other harmless snakes receive but little notice from them. Why is this? Is it that the hood of the cobra renders it so frightful an object, or have the cats in their nocturnal wanderings been struck at by cobras? Such is possible, for we know that in nine cases out of ten the strike is made without intention to exert the deadly power of the fangs. I believe indeed that unless irritated by an attacking enemy, or to secure active prey such as rats, &c., the cobra never strikes viciously. Experience of the ease with which its fangs are drawn and its helplessness without them would teach it to be careful of them.

Mangalore, Sept. 17 E. H. PRINGLE

Earthquake in New Guinea

WHEN crossing the main land of New Guinea, from the Geelvinks Bay in the north, to the south coast, I slept on the night of the 12th to the 13th of June, 1873, in the swamps of the MacCluer Gulf (famous for the murder of some of the crew and the ship's-doctor of H.M.S. *Panther* and *Endeavour*, Capt. MacCluer, in 1791, and by the attack on Signor Cerruti, the Italian traveller, several years ago). About 2 A.M. of the 13th I awoke, in consequence of a rattling noise like that of gun-shooting. I roused my six Malay companions, who slept around me in a small native prouw, seized my guns, and listened to what would follow. But nothing happened. It was unintelligible to me what had been the cause of this noise, the natives of these parts having no guns, so far as I knew, and even if they had intended an attack, would not announce their arrival by firing their guns, instead of approaching in silence. On the other hand, when sleeping in a virgin forest like that which bordered these swamps, crushing noises from falling trees and from animals breaking down rotten branches often occur, but never so many together.

Nothing more being heard we fell asleep again. At about 4 A.M. the same thing happened once more. I remained awake. At dawn the Papooas, whom I had brought with me from the north coast—ten men—came back to my resting-place; they had left me, to sleep apart, had heard the noise, but could not understand it either.

When on the 13th I came back to Papooan houses at the River Takasi, which falls into the MacCluer Gulf—a minute description of which will be published very soon in "Petermann's Mittheilungen"—I heard the account of a heavy earthquake, which had taken place the night before; this of course explaining the noises we had heard: many trees having broken down at the same moment in consequence of the movement of the ground. We did not feel the earthquake in our small boat, because it lay entirely in the swamp, which had not propagated the shock.

On the 18th I was back at my little schooner, which was at anchor in the Geelvinks Bay, near a place called Passim. The earthquake had been felt here at the same time, accompanied by heavy underground thunder, and I could make out that the direction had been N.W. to S.E.

After some days I came to a place just at the foot of the so much spoken of Arfak Mountains, called Audai; the earthquake had been heavy here, and even more shocks were felt on the following day. The direction had been W.E. Several native houses, built on very high poles near the slope of a hill, were destroyed, the Papooas (Arfaks) still frightened and of opinion that the earthquake had been "made" by their enemies, another tribe on the mountains.

But in the Bay of Dorey, which has so often been visited by expeditions to New Guinea and by naturalists, where I arrived a fortnight later, the shocks appeared to have been the heaviest. All the Papooas in the different settlements there were living on shore in small shelters or huts, hastily erected, whereas they are known always to live in those large houses on the water so often described. Several of these large houses had broken down, and the natives were still very much frightened; they would not remove into their houses on the water. On the island of Mansawari (Mansinam), in the Bay of Dorey, the seat of a missionary, the shocks had been from S.W. to N.E. I afterwards sought information about the extent of this earthquake, and made out that it was felt at Amberbaki, on the North coast of New Guinea, at Salwatti, the island in the North-west, and on the island of Toble, in the east. The centre had been undoubtedly on the Arfak Mountains. Light earthquakes sometimes occur in New Guinea, heavy ones seldom. The destruction by the last heavy one in 1864 could even be seen by me in 1873 along the seashore from Dorey to Wariab, and up the Arfak Mountains, in the south of the bay of Dorey. Volcanic eruptions in these parts are not known or recorded from earlier times. But one of the tops of these mountain chains bears in the native language the name of "Fire Mountain," and some of my hunters pretended to have seen on one of their excursions (some thousands of feet high) the ground split open quite fresh, in consequence of the earthquake, as they believed.

This earthquake has not been felt in Halmabeira and the Molukkos Islands, where shocks occurred some weeks afterwards, so that the convulsions, referred to above, appear to have been local ones in New Guinea. DR. A. B. MEYER

Sensitive Flames at the Crystal Palace Concerts

LAST Saturday, Jan. 31, at the Crystal Palace, while Mr. Vernon Rigby was singing Beethoven's "Adelaida," I heard what I thought was strangely out of place—an accompaniment to the song played on the highest notes of a violin, sometimes closely following the air note for note, at other times being one-third lower. I soon found that this proceeded from one or two sensitive gas jets, notwithstanding they were at the end of the winter concert-room farthest away from the orchestra. The very perfect manner in which they responded to every note, no matter how *piano*, was curious.

It happened that the gas pressure had just been increased. Had this occurred earlier the effect of Mdme. Norman-Neruda's fine performance of Mendelsohn's violin concerto would have been totally destroyed, as far as regards a large part of the audience. This shows that it is a matter of no small importance in a concert-room to have the size and number of the gas-burners properly proportioned to the gas supply.

King's College, Feb. 3 W. N. HARTLEY

THE PHOTOGRAPHIC SOCIETY

THE metropolitan photographic journals contain evidence that the Photographic Society of London is menaced with revolution or dissolution. If both were to befall it, the interests of Science would hardly suffer, since a more singularly inefficient organisation, under the guise of a scientific body, it would be difficult to find, or one whose results in the scientific world are so trivial.

It is difficult indeed to conceive that a society into whose hands, *faute de mieux*, the recognition and fostering of research in so important a branch of science as photography has fallen, should have done absolutely nothing for so many years but organise itself into a pocket borough in the direction of which no man of eminent scientific capacity takes part; which not only has no scientific reports or even investigations, but seems to care only to make of itself a weak mimicry of an art club, the chief objects of which are to prove that a photographer ought to have a chance for the Royal Academy, to discuss the most effective style of getting up portraits to revive the trade demand, and to discuss such questions as to whether portraits may be re-touched or not, and whether the printing of a photograph from a half-dozen negatives, more or less, is to be regarded as a work of design or not.

It is not sufficient to put the names of two or three well-known men of science on the council of a society if the society show no care for science; and if the Photographic Society can do nothing more to merit the nominal position which it holds (without filling it), it is time that it should retire and give place to another. Photography has now become one of the most important aids to research in many fields of Science; every new discovery which shall develop this assistance and make its efficiency more complete is of importance to the whole world—of an importance which makes it almost incredible that the Photographic Society should not only take no part in the investigations which would lead to discovery, but should never even take recognition of them even when made, while the petty jealousies of the dominant clique have driven out of the society most of the really capable and successful investigators who have ever been in it. If the efforts at reform now being made should lead to success and the society become what it should be, a scientific body, so much the better; but if not, it is time that some new organisation should be formed to take in hand seriously the exploration of the still untried fields of chemical research, and make Photography a real branch of Science, and not deal with it merely as an amusement or a trade.

ASTRONOMY IN THE ARGENTINE CONFEDERACY

DR. GOULD, the director of the new Observatory in the Argentine Confederacy, continues to send encouraging accounts of the progress of the great astronomical works that he has there undertaken. Having laboured to determine accurately the relative brightness of all the stars in the southern heavens visible to the naked eye, he announces that a few weeks will enable him to begin the preparation of this work for publication. Great care has been taken to make a thorough and accurate comparison of the results of the four assistants, and the rule has been to determine the brightness of all the stars down to the 7·3 magnitude, in order to make sure of losing none as bright as the seventh.

The labour of the Uranometry was undertaken before the arrival of the large meridian instrument, and as soon as the latter was established (namely, on Sept. 9, 1872), the observations of the zones of all stars as bright as the ninth magnitude were commenced in earnest. Each night three zones are observed whose lengths average about one hundred minutes, the entire observations for the year occupying at least eight hours. The weather is described as having been exceedingly unfavourable for astronomical work during the winter and early spring, until March, April, and May of the present year, when magnificent opportunities were enjoyed. Dr. Gould states that he has observed in all during the past year about fifty thousand stars, and considers that somewhat more than half of the work of observing is already finished.

Astronomers, however, know how great a labour of computation still awaits Dr. Gould and his assistants before his results can be put into that form which is most convenient for use. The photographic work undertaken by him at his own private expense has been prosecuted with all the success that could be expected with a broken lens. Finally, however, he concluded to bespeak another object-glass, which will be purchased for the use of the observatory; and the new lens having arrived in perfect order, he hopes before long to be able to resume his labours under better auspices.

The Cordoba Meteorological Bureau, established at his urgent representation by the national Government, has been organised and brought into working condition as rapidly as was practicable; but as the instruments were necessarily ordered from foreign countries, not more than half of them had arrived at the latest advices. Dr. Gould has, however, had the gratification of finding two gentlemen who have each carried on an uninterrupted series of observations for some dozen years past—one in Buenos Ayres, and the other near the Patagonian frontier—and he has secured the co-operation of about fifteen correspondents. The programme issued for the instruction of his observers differs apparently but little from that of the Smithsonian Institution, the hours of observation being seven, two, and nine, local time.

*THE COMMON FROG**

IX.

THE muscles connected with the human lingual apparatus are sufficiently complex. One such muscle—the *stylohyoid*—passes downwards on each side, from a process of the base of the skull to the corniculum of the os-hyoides or tongue-bone. The tongue-bone of the frog is, as we have seen, relatively far greater than is that of man, and the same may be said for the muscles attached to it, since we have no less than four muscles descending from the skull, and implanted into it, on each side.

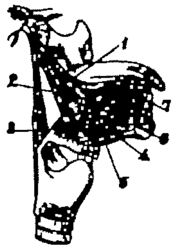

FIG. 63.

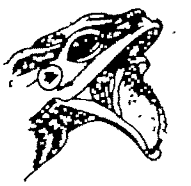

FIG. 64.

FIG. 63.—Muscles of the Right Side of the Tongue. 1, stylo-glossus; 2, stylo-hyoid; 3, stylo-pharyngeus; 4, hyo-glossus; 5, genio-hyoid; 6, genio-glossus; 7, lingualis

FIG. 64.—Head of the Frog *Phyllomedusa*, showing the tongue fixed in front, but free posteriorly.

This fact might well be supposed to bear direct relation to the size and mobility of the frog's tongue. This organ in the frog and toad is singularly different from the tongues of most familiar animals, in that it is not free and moveable in front, but *behind*. These Batrachians take their food by suddenly throwing forwards, out of the mouth, the free hinder end of the tongue. The insect or other small animal struck by it, adheres to it, on account of a viscid saliva with which it is coated. The prey is then suddenly drawn into the mouth and swallowed.

Here then is a ready explanation of the development of the *os-hyoides* and its muscles. There is a difficulty however in that two toads already described, the Pipa and the African form *Dactylethra* (Figs. 11 and 12), have no tongue whatever.

Moreover, there is another toad (*Rhinophrynus*) which is even more exceptional in its order than these two; in that its tongue is not free behind, but, like that of ordinary vertebrates, in front (Fig. 13.)

The fact is, that the large tongue-bone of these animals serves, with the muscles attached to it, as much to facilitate respiration as nutrition.

It has already been said that the frog has no ribs by the elevation and depression of which it may alternately fill and empty its lungs. Neither does it possess that transverse muscular partition, the diaphragm, or midrif, which in man's class is the main agent in carrying on that function.

The lungs of the frog are inflated as follows:—The mouth is filled with air through the nostrils and kept shut while the internal openings of the nostrils are stopped by the tongue, and the entrance to the gullet is closed. Then, by the contraction of the muscles attached to it, the os-hyoides is elevated; and every other exit from the mouth being closed, except that leading to the larynx, air is thus driven down the glottis into the lungs.

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* Continued from p. 189.

Thus for pulmonary respiration it is necessary to the frog to keep the mouth shut; and in this way, but for the action of the skin, the animal might be choked by keeping its mouth open.

It has been already stated that the typical segmentation of the limbs is wanting in all fishes, but present in all Batrachians that have limbs at all. Similarly in all Batrachians that have limbs at all the muscles of those limbs have essentially and fundamentally the same arrangement as in higher animals. In the higher animals, as in man, the muscles of the limbs belong to different categories named from the kinds of motion to which their contractions give rise.

Thus, when two bones are united by a moveable joint (as the thigh-bone and shin-bone) muscles which, by their contraction, tend to make the angle formed by such bones acute are termed "flexors." Those, on the contrary, which tend to open out such an angle are termed "extensors."

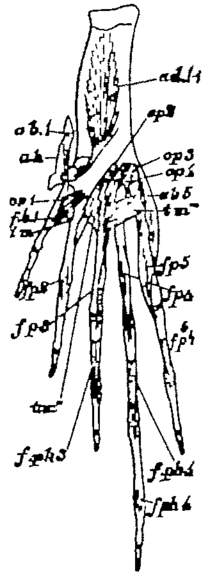


FIG 65.—Deep muscles of flexor surface of Frog's hind foot (The numbers indicate the digits to which the muscles belong.—No. 1 indicating the first digit or great toe). *ab*, abductors; *ad*, adductor, *fb*, flexor brevis, *fp*, flexores profundi; *fph*, flexores phalangium; *op*, opponens muscles; *tm*, transverse muscles.

In the forearm of man, and allied animals, there are muscles which tend by their contraction to place the hand in a position either of *pronation* or of *supination*.

When the arm and hand hang down, the *palm* being directed forwards, the position is that of supination, and the bones of the forearm are situate side by side.

When the arm and hand hang down, but the *back* of the hand is turned forwards, the position is that of pronation, and the radius crosses over the ulna. When we rest on the hands and knees, with the palms to the ground, the forearms are in pronation.

Muscles which tend to place the forearm and hand in the position of pronation are termed *pronators*; those which, by their contraction, tend to render it supine are called *supinators*.

It is somewhat surprising to find in an animal so nearly related to fishes as *Menobronchus* definite flexors, extensors, pro- and supi-nators essentially like those of higher animals; and these distinctions once established, persist up to man himself with increasing complications.

The muscular conformity between the highest and lowest of typically-limbed vertebrates is strikingly shown by the structure of the thigh and leg, the leading muscles of these parts in the frog being so like those of man that the practice of calling them by the same name is abundantly justified.

The perfection of man's hand has been justly the theme of panegyric, esteemed as widely as it is known. The delicacy and multiplicity of the motions of which it is capable are of course greatly due to the number and arrangement of the muscles with which it is provided.

One of the most important of these motions is that of the thumb as placed in opposition to the fingers, and effected by a muscle termed *opponens pollicis*.

An "opponens" muscle is one which passes from the bones of the wrist to one or other of the bones of the middle of the hand called *metacarpals*, and the *opponens pollicis* passes of course, as its name implies, to the metacarpal of the pollex or thumb.

No other finger of man's hand is furnished with such a muscle except the little finger, which possesses an *opponens minimi digiti*, passing from the wrist to the fifth metacarpal. The same condition obtains in the apes, though in them the opponens of the thumb is smaller and weaker than in man. Though the foot of man is furnished with many muscles, like the hand, yet not one of the toes is provided with an "opponens" or muscle, passing from the bones of the ankle to one or other of the bones of the middle of the foot, which latter are called metatarsals. The same is the case with the apes, except that the Orang-utan has a small "opponens" attached to the great toe.

This being premised, the foot of the Frog may well excite surprise as to its rich muscular structure. In addition to very numerous other muscles on both surfaces every one of the toes is provided with a separate opponens muscle, each having a muscle which passes from the bones of the ankle to its middle foot bone or metatarsal.

The question naturally occurs on beholding this prodigality of muscles—What special purpose is served by the Frog's foot? Surely mere jumping and swimming cannot require so elaborate an apparatus.

In fact, however, the Frog *does* make use of his feet for a purpose requiring actions no less dexterous and delicate than nest-building.

In 1872 Dr. Gunther observed a Frog busily occupied, and industriously moving its hind legs in a singular manner. On approaching closely he found it had constructed for itself a shelter in the shape of a little bower, constructed of dexterously interwoven blades of grass. The circumstances have been kindly transmitted to the author by the observer, in a private letter, as follows:—

"The 'nest-building' Frog was a large example of *Rana temporaria*, or *esculenta* (I forget which), which I had brought into the garden behind my house. It had taken up its abode in grass, near the edge of a tank, from which the turf sloped abruptly to the level of the garden. When I first disturbed the Frog from its lair, I found that it had lain in a kind of nest, which I cannot better describe than by comparing it to the form of a hare, with the grass on the edges so arranged that it formed a sort of roof over it. Sometimes the animal returned to it, sometimes it prepared a new form close to the old one, which remained visible for several days until it was obliterated by the growing grass.

"When in its nest, nothing could be seen of the Frog but the head.

"One day I poked the Frog out of its lair; after two or three jumps it returned to the old spot, and, squatting down on the grass, by some rapid movements of the hind legs it gathered the grass nearest to it, pressing it to its sides, and bending it over its body so as to be partially hidden.

"In all these operations no material was collected by the animal for its nest, but only the growing grass was either pressed down, or arranged so as to form a complete retreat.

"Unfortunately, the Frog soon disappeared altogether."

It is very probable that other functions, as yet unnoticed, may be performed by these members, since though the observation just above related is the first known observation of the kind, yet the manœuvre recorded is no doubt a constant habit of the animal.

Doubtless, also, the very singular actions performed by the male *Pipa* and *Obstetricans* are performed by the help of the hinder extremities.

At the same time that the Frog shows so startling a resemblance in its leg muscles to higher animals, it shows as striking a difference from the leg muscles of animals with which it is nearly allied,—namely, with those of its class-fellows, the *Urodela*.

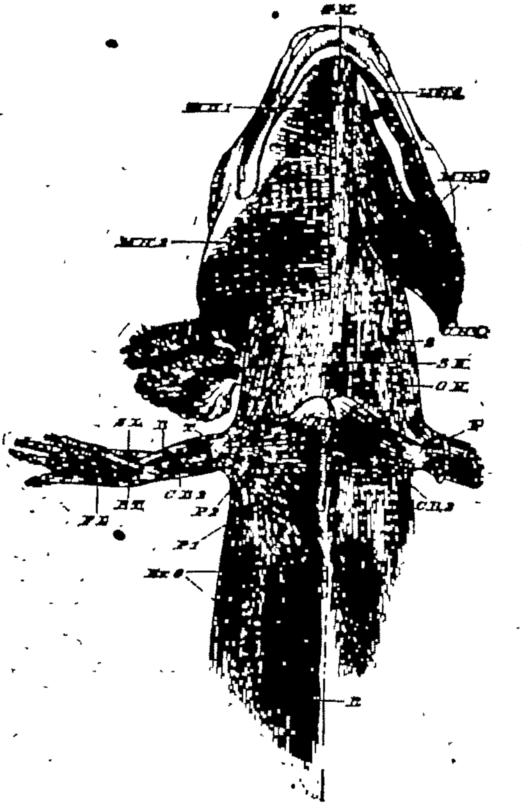


FIG. 66.—Muscles of Ventral Surface of *Menobranchus*. On the right side, superficial muscles; on the left side, deeper muscles, the mylo-hyoides, pectoralis, and external oblique being removed. Also superficial flexor muscles of right pectoral limb of *Menobranchus*. *B*, biceps; *CB*¹ and *CB*², coraco-brachialis; *CHE*, cerato-hyoideus externus; *EO*, external oblique; *FL*, flexor longus; *GH*, genio-hyoid; *MH*¹ and *MH*², mylo-hyoideus; *OH*, omo-hyoid; *P*, *P*¹, and *P*², pectoralis; *R*, rectus; *S*, subclavius; *SH*, sterno-hyoid; *SL*, supinator longus; *T*, triceps.

In Reptiles we meet with a muscle which takes origin from beneath the joints of the tail, and is inserted with the thigh-bone, and which has no certain representation amongst mammals, and is called the *femoro-caudal*.

In the Urodela we also meet with a *femoro-caudal*, but no such structure exists in the Anoura. This is not so surprising when we recollect the abortive condition of the tail of the Frog. It might, however, have been expected that in the Tadpole, during the co-existence of the tai with the hind legs, and while it thus externally resemble an eft—such a muscle would transitorily exist. Such, however, is not the case, and the distinction is a very remarkable one.

In one point, however, the Efts resemble the Frogs, namely, in the greater number and greater complexity as well as the greater size of the muscles of the hind-limbs than of the fore-limbs. It is well known that the Efts make use of their hind-limbs in attaching their eggs to the leaves and branches of aquatic plants; and further observations may show with regard to these animals facts as to the use of the members, as novel and interesting as the one just cited with regard to the nest-building actions of the Frog.

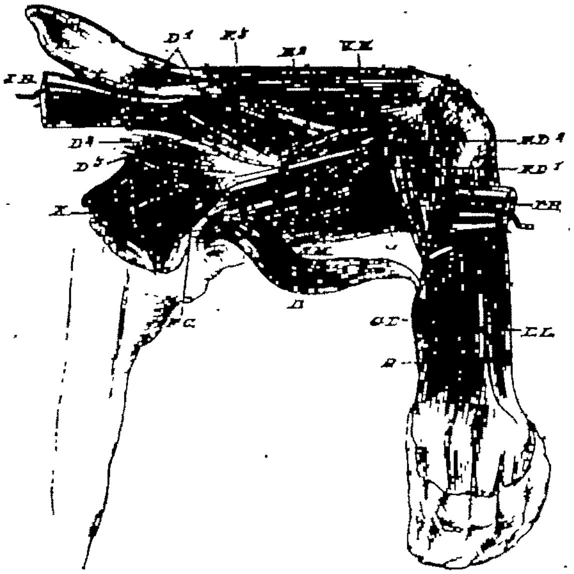


FIG. 67.—Deeper Muscles of Outer Aspect of Right Pelvic Limb of Parson's Chameleon; the ilio-peroneal cut reflected. *A*, adductor; *B*, biceps; *D*¹, gluteus primus; *D*², gluteus secundus; *D*³, gluteus tertius; *EL*, extensor longus digitorum; *F*² and *F*³, rectus femoris; *FC*, femoro-caudal; *FD*¹, flexor longus digitorum; *FD*², flexor tertius digitorum; *G*, gracilis; *GE*, gastrocnemius externus; *IP*, ilio-peroneal; *P*, peroneus; *S*, tibial adductor; *SM*, semi-membranosus; *VE*, vastus externus; *X*, gluteus maximus; *y*, tendon of femoro-caudal

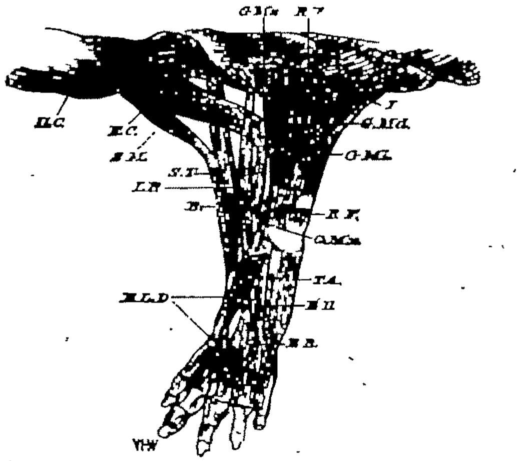


FIG. 68.—Deeper Muscles of Extensor Surface of Right Leg of Menopoma. *B*, biceps; *EB*, extensor brevis; *EH*, extensor hallucis; *ELD*, extensor longus digitorum; *FC*, femoro-caudal; *GMd* and *GM1*, muscles like the lesser gluteal; *GMx* and *RF*, great extensors of the thigh; *I*, muscle resembling the iliacus; *IC*, ilio-caudal; *IP*, ilio-peroneal; *SM* and *ST*, muscles like the semi-membranosus and semi-tendinosus respectively; *TA*, tibialis anticus.

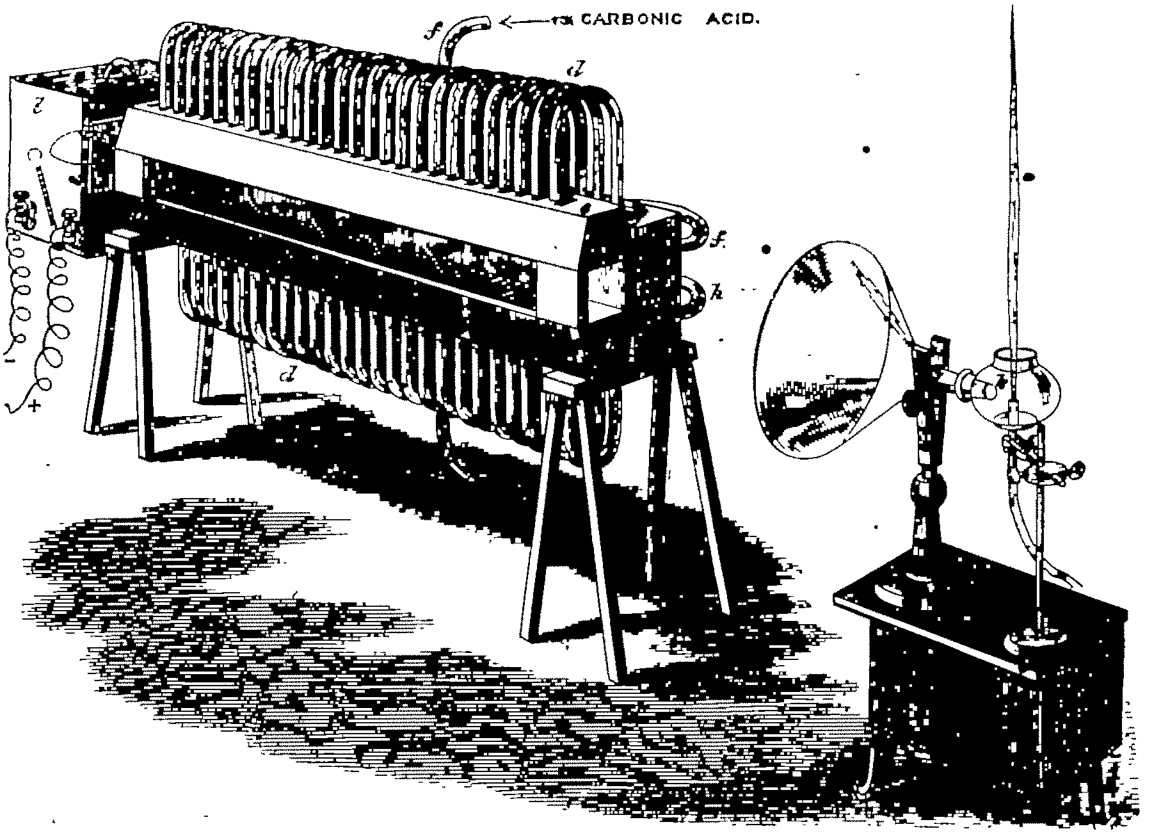
St. George Mivart

(*To be continued.*)

THE ACOUSTIC TRANSPARENCY AND OPACITY OF THE ATMOSPHERE *

II.

WE have now to consider the complementary side of the phenomena. A stratum of air, 3 miles thick, on a perfectly calm day, has been proved competent to stifle both the cannonade and the horn-sounds employed at the South Foreland; while the observations just recorded, one and all, point to the mixture of air and aqueous vapour as the cause of this extraordinary phenomenon. Such a mixture could fill the atmosphere with an impervious *acoustic cloud* on a day of perfect *optical* transparency. But, granting this, it is incredible that so great a body of sound could utterly disappear in so short a distance, without rendering any account of itself. Supposing, then, instead of placing ourselves behind the acoustic cloud, we were to place ourselves in front of it, might we not, in accordance with the law of conservation, expect to receive by reflection the sound which had failed to reach us by transmission? The case would then be strictly analogous to the reflection of light from an ordinary cloud to an observer placed between it and the sun.



[A tunnel 2 in. square, 4 ft. 8 in. long, open at both ends, and having a glass front, runs through the box *a b*. The space above and below is divided into cells opening into the tunnel by oblong orifices exactly corresponding vertically. Each alternate cell of the upper series—the 1st, 3rd, 5th, &c.—communicates by a tube (*d d*) with the upper reservoir (*g*), its counterpart in the lower series having a free outlet into the air. In like manner the 2nd, 4th, 6th, &c., of the lower series of cells are connected with the lower reservoir (*i*); and each has its direct passage into the air through the cell immediately above it. The gas distributors are filled from both ends at the same time; the upper with carbonic acid gas, the lower with coal-gas, by branches from their respective supply pipes (*f*, *h*). A well-padded box (*l*) opening upon the end of the tunnel forms a little cavern, whence the sound-waves are sent forth by an electric bell. A few feet from the other end of the tunnel, in a direct line, is a sensitive flame (*k*), provided with a funnel as sound collector, and guarded from chance currents by a shade.

The bell was set ringing. The flame, with quick response to each blow of the hammer, emitted a sort of musical roar, so regular were its alternate shortenings and lengthenings as the successive sound-pulses reached it. The gases were then admitted. Twenty-five flat jets of coal gas ascended from the tubes below, and twenty-five cascades of carbonic acid poured down from the tubes above. That which was a homogeneous medium had now fifty limiting surfaces, from each of which a portion of the sound was thrown back. In a few moments these successive reflections became so effective that not a single sound-wave having sufficient power to affect a flame so sensitive as to be knocked down, crushed, as it were, by a chirrup, or jingle, at twenty feet distance, could pierce the clear, optically-transparent, but acoustically-opaque atmosphere in the tunnel. So long as the gases continued to flow, the flame remained perfectly tranquil. When the supply was cut off, the gases rapidly diffused into the air. The atmosphere of the tunnel became again homogeneous, and therefore acoustically transparent, and the flame bowed down to each sound-pulse as before. Alternate layers of common air and air saturated with various vapours produce the same effect.]

My first care, in the early part of the day in question, was to assure myself that our inability to hear the sound did not arise from any derangement of the instruments. At one P.M. I was rowed to the shore, and landed at the base of the South Foreland cliff. The body of air which had already shown such extraordinary power to intercept sound, and which manifested this power still more impressively later in the day, was now in front of us. On it the sonorous waves impinged, and from it they were sent back to us with astonishing intensity. The instruments, hidden from view, were on the summit of a cliff

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* Royal Institution, Friday evening Discourse by Prof. Tyndall, D.C.L., LL.D., F.R.S. Jan. 16. (Continued from page 253.)

235 feet above us, the sea was smooth and clear of ships, the atmosphere was without a cloud, and there was no object in sight which could possibly produce the observed effect. From the perfectly transparent air the echoes came, at first with a strength apparently but little less than that of the direct sound, and then dying gradually and continuously away. The remark of my companion, Mr. Edwards, was: "Beyond saying that the echoes seemed to come from the expanse of ocean, it did not appear possible to indicate any more definite point of reflection." Indeed, no such point was to be seen; the echoes reached us as if by magic, from absolutely invisible walls. Arago's notion that clouds are necessary to produce atmospheric echoes is therefore untenable.

The reflection from aerial surfaces has never been experimentally demonstrated. It is wholly a matter of inference, and I wished very much to reduce it to demonstration. I made one or two rough experiments on the transmission of sound through a series of flames; and no doubt by proper arrangement such experiments might be made successful. I then thought that alternate layers of carbonic acid and coal gas, the one rising by its lightness, the other falling by its weight, would supply a heterogeneous medium suitable for the demonstration. To my assistant, Mr. Cottrell, who possesses in an eminent degree the skill of devising apparatus, I communicated this idea, leaving the realisation of it wholly to him, and he has carried it out in the most admirable manner. (For a sketch and description of the apparatus, see previous page).

During my recent visit to the United States I accompanied General Woodruff, the engineer in charge of two of the lighthouse districts, to the establishments at Staten Island and Sandy Hook, with the express intention of observing the performance of the steam-syren, which, under the auspices of Prof. Henry, has been introduced into the lighthouse system of the United States. Such experiments as were possible to make under the circumstances were made, and I carried home with me a somewhat vivid remembrance of the mechanical effect of the sound of the steam syren upon my ears and body generally. This I considered to be greater than the similar effect produced by the horns of Mr. Holmes; hence the desire, on my part, to see the syren tried at the South Foreland. The formal expression of this desire was anticipated by the Elder Brethren, while their wishes were in turn anticipated by the courteous kindness of the Lighthouse Board at Washington. Informed by Major Elliott that our experiments had begun, the Board forwarded to the Corporation, for trial, the noble instrument now mounted at the South Foreland. The principle of the syren is easily understood. A musical sound is produced when the tympanic membrane is struck periodically with sufficient rapidity. The production of these tympanic shocks by puffs of air was first realised by Dr. Robison. But the syren itself is the invention of Cagniard de la Tour. He employed a box with a perforated lid, and above the lid a similarly perforated disc, capable of rotation. The perforations were oblique, so that when wind was driven through the disc was set in motion. When the perforations coincided a puff escaped, when they did not coincide the current of air was cut off. The regular succession of impulses thus imparted to the air produce a musical note. Even in its small form, the instrument is capable of producing sounds of great intensity. The syren has been improved upon by Dove, and notably developed by Helmholtz.

In the steam syren patented by Mr. Brown of New York, a fixed disc and a rotatory disc are also employed, radial slits being cut in both discs instead of circular apertures. One disc is fixed across the throat of a trumpet-shaped tube, 16¼ ft. long, 5in. diameter where the disc crosses it, and gradually opening out till at the other extremity it reaches a diameter of 2 ft. 3 in. Behind the fixed disc is the rotating one, which is driven by separate mechanism. The trumpet is mounted on a boiler. In our experiments steam of 70 lbs. pressure has for the most part been employed. Just as in the air-syren, when the radial slits of the two discs coincide, a puff of steam escapes. Sound-waves of great intensity are thus sent through the air; the pitch of the note produced depending on the rapidity with which the puffs succeed each other; in other words, upon the velocity of rotation.

On October 8 I remained some time at the Foreland, listening to the echoes. Of the horn-echoes I have already spoken: those of the syren were still more extraordinary. Like the others they were perfectly continuous, and faded as if into the distance gradually away. The single sound seemed rendered complex and multitudinous by its echoes, which resembled a band of trumpeters first responding close at hand, and then retreating rapidly from us towards the coast of France. The syren echoes had eleven seconds duration, those of the horn eight seconds. With sounds of the same pitch the duration of the echo might be taken as a measure of the penetrative power of the sound.

I moved away from the station so as to lower the power of the direct sound. This was done by dropping into the sound-shadow behind an adjacent eminence. The echoes heard thus were still more wonderful than before. In the case of the syren, moreover, the reinforcement of the direct sound by the echo was distinct. One second after the commencement of the syren blast, the echo struck in as a new sound. This first echo, therefore, must have been flung back by a body of air not more than 600 or 700 ft. in thickness.

There appears to be a direct connection between the duration of the echoes and the distance penetrated by the sound. On October 17 the perfect clearness of the afternoon caused me to choose it for the examination of the echoes. The echoes of that day, when our transmitted sound reached its maximum, exceeded in duration those of all other days. We heard the syren fifteen miles off. On the close of the day we found its echoes fourteen to fifteen seconds in duration, this long duration indicating the distance from which they were thrown back.

The visual clearness of the atmosphere on the morning of Oct. 8, was very great, the coast of France was very plainly seen, the Grisnez lighthouse, and the monument and cathedral of Boulogne, were distinctly visible to the naked eye. At 5¼ miles from the station, the horn was heard feebly, the syren clearly. At 2.30 P.M., a densely black scowl overspread the heavens to the W.S.W. At this hour, the distance being 6 miles, the horn was heard very feebly, the syren more distinctly, all being hushed on board during the observations. A squall now approached us from the west. In the Alps, or elsewhere, I have rarely seen the heavens blacker. Vast cumuli floated in the N.E. and S.E.; vast streamers of rain were seen descending W.N.W.; huge scrolls of cloud to the N.

At 7 miles distance the syren was not strong, and the horn was very feeble.

The heavy rain at length reached us, but although it was falling all the way between us and the Foreland, the sound, instead of being deadened, rose perceptibly in power. Hail was now added to the rain, and the shower reached a tropical violence. We stopped. In the midst of this furious squall both the horn and the syren were distinctly heard, and as the shower lightened, thus lessening the local pattering, the sounds so rose in power that we heard them at a distance of 7¼ miles distinctly louder than they had been heard through the rainless atmosphere at 5 miles. This observation is entirely opposed to the statement of Derham, which has been repeated by all writers since his time, regarding the stifling influence of falling rain upon sound. But it harmonises perfectly with our experience of July 3, which proved water in a state of *vapour* so mixed with air as to form non-homogeneous parcels, to be a most potent influence as regards the stoppage of sound. Prior to the violent shower, the air had been in this flocculent condition, but the descent of the rain and hail restored in part the homogeneity of the atmosphere, and augmented its transmissive power. There may be states of the atmosphere associated with rain unfavourable to sound, but to rain itself I have never been able to trace the slightest deadening effect.

The observations continued till November 25. Up to that date we had no fog, but the experience of July 1 and of October 30, entirely destroy the notion that optical transparency and acoustic transparency go hand-in-hand. Both were days of haze sufficiently thick to hide the cliffs of the Foreland, but on the former the sounds reached 12¾, and on the latter 11¼ miles.

Reflection from the particles of fog and haze has been hitherto held to blot out sound. The late dense fog in London enabled experiments to be made which entirely controvert this conclusion. On December 10 I made some experiments over the Serpentine. The fog was very dense. Mr. Cotterell stood on the walk below the south-west end of the bridge dividing Hyde Park from Kensington Gardens, while I went to the eastern end of the Serpentine. He blew a dog-whistle, and an organ-pipe sounding Mi_3, which corresponds to 380 waves a second. I heard both distinctly. I then changed places with him, and listening attentively at the bridge, heard for a time the distinct blast of the whistle only. The organ-pipe at length sent its deeper note to me across the water. It sometimes rose to great distinctness, and sometimes fell to inaudibility. These fluctuations, of which various striking examples have been observed, are due to the drifting of acoustic clouds, which act upon a source of sound, as the drifting of ordinary clouds upon the sun. The whistle showed the same intermittence as to period, but in the opposite sense, for when the whistle was faint the pipe was strong, and *vice versâ*.

There seemed to be an extraordinary amount of sound in the air. It was filled with a resonant roar from the Bayswater and Knightsbridge roads. The railway whistles were extremely distinct, while the fog-signals exploded at the various metropolitan stations kept up a loud and almost constant cannonade. I could by no means reconcile this state of things with the statements so categorically made regarding the influence of fog.

The water was on this day warmer than the air, and the ascending vapour was instantly in part condensed, thus revealing its distribution. Instead of being uniformly diffused, it formed wreaths and striæ. I am pretty confident that had the vapour been able to maintain itself as such, the air would have been far more opaque to sound. In other words, I believe that the very cause which diminished the optical transparency of the atmosphere augmented its acoustical transparency.

This conclusion was confirmed by numerous observations made while the fog lasted.*

On Dec. 13 the fog was displaced by a thin haze. We could plainly see from one bank of the Serpentine to the other, and far into Hyde Park beyond. There was a wonderful subsidence of the sound of the carriages, church bells, &c. Being at the bridge I listened for the sounds excited at the end of the Serpentine. With the utmost stretch of attention I could hear nothing. I walked along the edge of the water towards Mr. Cottrell, and when I had lessened the distance by one half, the sound of his whistle was not so distinct as it had been at the bridge on the day of the densest fog. Hence the optical cleansing of the air by the melting of the fog had so darkened it acoustically, that a sound generated at the end of the Serpentine was lowered to at least one-fourth of its intensity at a point midway between the end and the bridge.

This opportune fog enabled me to remove the last of a congeries of errors which, ever since the year 1708, have attached themselves to this question. As regards phonic coast-signals, we now know exactly where we stand.

It is worth observing here that the solution of the deportment of hail, rain, snow, haze, and fog, as regards sound, depends entirely upon observations made on the 3rd of July, which was about the last day that one would have chosen for experiments on fog-signals. Indeed, it had been distinctly laid down that observations on such a day would be useless; that they might indeed enable us to weed away bad instruments from good ones, but could throw no light whatever on the question of fog-signaling. That the contrary is the case, is an illustration of the fact that the solution of a question often lies in a direction diametrically opposed to that in which it appears to lie.†

EXTRACTS FROM AN ADDRESS BY SIR W. THOMSON, TO THE SOCIETY OF TELEGRAPHIC ENGINEERS

I HAVE advisedly, not thoughtlessly, used the expression "terrestrial electricity." It is not an expression we are accustomed to. We are accustomed to "terrestrial magnetism;" we are accustomed to atmospheric electricity. The electric telegraph forces us to combine our ideas with reference to terrestrial magnetism and atmospheric electricity. We must look upon the earth and the air as a whole—a globe of earth and air—and consider its electricity whether in rest or in motion. Then, as to terrestrial magnetism, of what its relation may be to perceptible electric manifestations we at present know nothing.

You all know that the earth acts as a great magnet. Dr. Gilbert, of Colchester, made that clear nearly 300 years ago; but how the earth acts as a great magnet—how it is a magnet,—whether an electro-magnet in virtue of currents revolving round under the upper surface, or whether it is a magnet like a mass of steel or load-stone, we do not know. This we do know, that it is a variable magnet, and that a first approximation to the variation consists in a statement of motion round the axis of figure—motion of the magnetic poles, round the axis of figure, in a period of from 900 to 1,000 years. The earth is not a uniformly magnetised magnet with two poles, and with circles of symmetry round those poles. But a first expression—as we should say in mathematical language the first "harmonic term"—in the full expression of terrestrial magnetism is an expression of a regular and symmetrical distribution such as I have indicated. Now, this is quite certain, that the axis of this first term, so to speak, or this first approximation, which, in fact, we might call the magnetic axis of the earth, does revolve round the axis of figure.

When the phenomena of terrestrial magnetism were first somewhat accurately observed about three hundred years ago, the needle pointed here in England a little to the east of north; a few years later it pointed due north; then, until about the year 1820, it went to the west of north; and now it has come back towards the north. The dip has experienced corresponding variations. The dip was first discovered by the instrument maker, Robert Norman, an illustration, I may mention in passing, of the benefits which abstract science derives from practical applications—one of the most important fundamental discoveries of magnetism brought back to theory by an instrument maker who made mariner's compasses. Robert Norman, in balancing his compass cards, noticed that after they were magnetised one end dipped, and he examined the phemonenon and supported a needle about the centre of gravity, magnetised it, and discovered the dip. When the dip was first so discovered by Robert Norman it was less than it is now. The dip has gone on increasing, and is still increasing; but about 50 years ago the deviation from true north was greatest. Everything goes on as if the earth had a magnetic pole revolving from west to east round the true North Pole, at a distance of 20° from it. About three hundred years ago its azimuth from England was a little to the east of the north pole: then it came round, moving eastwards on the far side of the north pole, and round in a circle towards us on the left-hand side of the north pole, as looked to from England. That motion in a circle round the north pole has already been experienced within the period during which somewhat accurate measurements have been made—has been experienced to the extent of rather more than a quarter of the whole revolution; and we may expect that about 200 years from the present time the magnetic pole will be between England and the North Pole, so that the needle will thus point due north, and the dip be greater than it has been for 1,000 years, or will be for another. It is one of the greatest mysteries of science, a mystery which I might almost say is to myself a subject of daily contemplation—what can be the cause of this magnetism in the interior of the earth? Rigid magnetisation, like that of steel or the load-stone, has no quality in itself in virtue of which we can conceive it to migrate round in the magnetised bar. Electric currents afford the more favoured hypothesis; they are more mobile. If we can conceive electric currents at all, we may conceive them flitting about. But what sustains the electric currents? People sometimes say, heedlessly or ignorantly, that thermo-electricity does it. We have none of the elements of the problem of thermo-electricity in the state of underground temperature which could possibly explain, in accordance with any knowledge we have of thermo-electricity, how there could so be sustained currents round the earth. And if there were currents round the earth, regulated by some cause so as to give them a definite direction at one time, we are as far as ever from explaining how the channel of those currents could experience that great secular variation which we know it does. Thus we have merely a mystery. It would be rash to suggest even an explanation. I may say that one explanation has been suggested. It was suggested by the great astronomer, Halley, that there is a nucleus in the interior of the earth, and that the mystery is explained simply by a magnet not rigidly connected with the upper crust of the earth, but revolving round an axis differing from the axis of rotation of the outer crust, and exhibiting a gradual precessional motion independent of the precessional motion of the outer rigid crust. I merely say that has been suggested. I do not ask you to judge of the probability: I would not ask myself to judge of the probability of it. No other explanation has been suggested.

But now, I say, we look with hopefulness to the practical telegraphist for data towards a solution of this grand problem. The terrestrial magnet is subject, as a whole, to the grand secular variation which I have indicated. But, besides that, there are annual variations and diurnal variations. Every day the needle varies from a few minutes on one side to a few minutes on the other side of its mean position, and at times there are much greater variations. What are called "magnetic storms" are of not very unfrequent occurrence. In a magnetic storm the needle will often fly twenty minutes, thirty minutes, a degree, or even as much as two or three degrees sometimes, from its proper position—if I may use that term—its proper position for the time; that is, the position which it might be expected to have at the time according to the statistics of previous observations. I speak of the needle in general. The ordinary observation of the horizontal needle shows these phenomena. So does observation on the dip of the needle. So does observation on the total intensity of the terrestrial magnetic force.

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* Since the first notices of this lecture appeared in the newspapers, strong confirmatory evidence has been received

† The foregoing report was compiled from the notes of Prof. Tyndall. It is published with Prof. Tyndall's sanction, but was not written by himself.

The three elements, deflection, dip, and total intensity, all vary every day with the ordinary diurnal variation, and irregularly with the magnetic storm. The magnetic storm is always associated with a visible phenomenon, which we call, habitually, electrical;—aurora borealis, and, no doubt, also aurora of the southern polar regions. We have the strongest possible reasons for believing that aurora consists of electric currents, like the electric phenomena presented by currents of electricity through what are called vacuum tubes, through the space occupied by vacuums of different qualities in the well-known vacuum tubes. Of course, the very expression, "vacuums of different qualities" is a contradiction in terms. It implies that there are small quantities of matter of different kinds left in those nearest approaches to a perfect vacuum which we can make.

Well now, it is known to you all that aurora borealis is properly comparable with the phenomena presented by vacuum tubes. The appearance of the light, the variations which it presents, and the magnetic accompaniments, are all confirmatory of this view, so that we may accept it as one of the truths of science. Well now—and here is a point upon which, I think, the practical telegraphist not only can, but will, before long give to abstract science data for judging—is the deflection of the needle a direct effect of the auroral current, or are the auroral current and the deflection of the needle common results of another cause?

With reference to this point, I must speak of underground currents. There again I have named a household word to everyone who has anything to do with the operation of working the electric telegraph, and not a very pleasing household word I must say. I am sure most practical telegraphers would rather never hear of earth currents again. Still we have got earth currents; let us make the best of them. They are always with us; let us see whether we cannot make something of them, since they have given us so much trouble. Now, if we could have simultaneous observations of the underground currents, of the three magnetic elements, and of the aurora, we should have a mass of evidence from which, I believe, without fail, we ought to be able to conclude an answer more or less definite to the question I have put. Are we to look in the regions external to our atmosphere for the cause of the underground currents, or are we to look under the earth for some unknown cause affecting terrestrial magnetism, and giving rise to an induction of those currents? The direction of the effects, if we can only observe those directions, will help us most materially to judge as to what answer should be given.

It is my desire to make a suggestion which may reach members of this society, and associates in distant parts of the world. I make it not merely to occupy a little time in an inaugural address, but with the most earnest desire and expectation that something may be done in the direction of my suggestion. I do not venture to say that something may come from my suggestion, because, perhaps, without any suggestion from me, the acute and intelligent operators whom our great submarine telegraph companies have spread far and wide over the earth, are fully alive to the importance of such observations as I am now speaking of. I would just briefly say that this kind of observation is what would be of value for the scientific problem—to observe the indication of an electrometer at each end of a telegraph line at any time, whether during a magnetic storm or not, and at any time of the night or day. If the line be worked with a condenser at each end, this observation can be made without in the slightest degree influencing, and therefore without in the slightest degree disturbing, the practical work throughout the line. Put on an electrometer in direct connection with the line, connect the outside of the electrometer with a proper earth connection, and it may be observed quite irrespectively of the signalling; when the signalling is done, as it very frequently is at submarine lines, with a condenser at each end. The scientific observation will be disturbed undoubtedly, and considerably disturbed by the sending of messages, but the disturbance is only transient, and in the very pause at the end of a word there will be a sufficiently near approach to steadiness in the potential at the end of a wire connected with the electrometer to allow a careful observer to estimate with practical accuracy the indication that he would have were there no working of the line going on at the time. A magnetic storm of considerable intensity does not stop the working, does indeed scarcely interfere with the working, of a submarine line in many instances when a condenser is used at each end.

Thus, observations, even when the line is working, may be made during magnetic storms, and again, during hours when the line is not working, if there are any, and even the very busiest lines have occasional hours of rest. Perhaps, then, however, the operators have no time or zeal left, or, rather, I am sure they have always zeal, but I am not sure that there is always time left, and it may be impossible for them to bear the strain longer than their office hours require them. But when there is an operator, or a superintendent, or a mechanic, or an extra operator who may have a little time on his hands, then, I say, any single observation or any series of observations that he can make on the electric potentials at one end of an insulated line will give valuable results. When arrangements can be made for simultaneous observations of the potentials by an electrometer at the two ends of the line, the results will be still more valuable.

And, lastly, I may just say that when an electrometer is not available, a galvanometer of very large resistance may be employed. This will not in the slightest degree interfere with the practical working any more than would an electrometer, nor will it be more difficult to get results of the scientific observations not overpoweringly disturbed by the practical working if a galvanometer is used than when an electrometer is available. The more resistance that can be put in between the cable and the earth in circuit with the galvanometer the better, and the sensibility of the galvanometer will still be found perhaps more than necessary. Then, instead of reducing it by a shunt, let steel magnets, giving a more powerful direction to the needle, be applied for adjusting it. The resistance in circuit with the galvanometer between cable-end and earth ought to be at least twenty-times the cable's copper-resistance to make the galvanometer observations as valuable as those to be had by electrometer.

I should speak also of the subject of atmospheric electricity. The electric telegraph brings this phenomenon into connection with terrestrial magnetism with earth currents, and through them with aurora borealis, in a manner for which observations made before the time of the electric telegraph, or without the aid of the electric telegraph, had not given us any data whatever. Scientific observations on terrestrial magnetism, and on the aurora, and on atmospheric electricity, had shown a connection between the aurora and terrestrial magnetism in the shape of the disturbances that I have alluded to at the time of magnetic storms; but no connection between magnetic storms and atmospheric electricity, thunderstorms, or generally the state of the weather—what is commonly called meteorology—has yet been discovered.

The one common link connecting these phenomena hitherto known to us is exhibited in the electric telegraph. A telegraphic line—an air line more particularly, but a submarine line also—shows us unusually great disturbances, not only when there are auroras and variations of terrestrial magnetism, but when the atmospheric electricity is in a disturbed state. That it should be so electricians here present will readily understand. They will understand when they consider the change of electrification of the earth's surface which a lightning discharge necessarily produces.

I fear I might occupy too much of your time, or else I would just like to say a word upon atmospheric electricity, and to call your attention to the quantitative relations which questions in connection with this subject bear to those of ordinary earth currents and the phenomena of terrestrial magnetism. In fair weather, the surface of the earth is always, in these countries at all events, found negatively electrified. Now the limitation to these countries that I have made suggests a point for the practical telegraphists all over the world. Let us know whether it is only in England, France, and Italy that in fine weather the earth's surface is negatively electrified.

The only case of exception on record to this statement is Prof. Piazzi Smyth's observations on the Peak of Teneriffe. There, during several months of perfectly fair weather, the surface of the mountain was, if the electric test applied was correct, positively electrified; but Prof. Piazzi Smyth has, I believe, pointed out that the observations must not be relied upon. The instrument, as he himself found, was not satisfactory. The science of observing the atmospheric electricity was then so much in its infancy that, though he went prepared with the best instrument, and the only existing rules for using it, there was a fatal doubt as to whether the electricity was positive or negative after all. But the fact that there has been such a doubt is important. Now I suppose there will be a telegraph to Teneriffe before long, and then I hope and trust some of the operators will find time to climb the Peak. I am sure that, even without an electric object, they will go up the Peak. Now they must go up the Peak with an electrometer in fine weather, and ascertain whether the earth is positively or negatively electrified. If they find that on one fine day it is negatively electrified, the result will be valuable to science; and if on several days it is found to be all day and all night negatively electrified, then there will be a very great accession to our knowledge regarding atmospheric electricity.

When I say the surface of the earth is negatively electrified, I make a statement which I believe was due originally to Peltier. The more common form of statement is that the air is positively electrified, but this form of statement is apt to be delusive. More than that, it is most delusive in many published treatises, both in books and encyclopædias upon the subject. I have in my mind one encyclopædia in which, in the article "Air, Electricity of," it is said that the electricity of the air is positive, and increases in rising from the ground. In the same encyclopædia, in the article "Electricity, Atmospheric," it is stated that the surface of the earth is negatively electrified, and that the air in contact with the earth, and for some height above the earth, is, in general, negatively electrified. I do not say too much, then, when I say that the statement that the air is positively electrified has been at all events a subject for ambiguous and contradictory propositions; in fact, what we know by direct observation is, that the surface of the earth is negatively electrified, and positive electrification of the air is merely inferential.

Suppose, for a moment, that there were no electricity whatever in the air—that the air were absolutely devoid of all electric manifestation, and that a charge of electricity were given to the whole earth. For this no great amount would be necessary. Such amounts as you deal with in your great submarine cables would, if given to the earth as a whole, produce a very considerable electrification of its whole surface. You all know the comparison between the electricity of one Atlantic cable—the electro-static capacity of one of the Atlantic cables—with the water round its gutta-percha for outer coating, and the earth and air with infinite space for its outer coating.[*] I do not remember the figures at this moment; in fact, I do not remember which is the greater. Well, now, if all space were non-conducting—and experiments on vacuum tubes seem rather to support the possibility of that being the correct view—if all space were non-conducting, our atmosphere being a non-conductor, and the rarer and rarer air above us being a non-conductor, and the so-called vacuous space, or the interplanetary space beyond that (which we cannot admit to be really vacuous) being a non-conductor also, then a charge could be given to the earth as a whole, if there were the other body to come and go away again, just as a charge could be given to a pith ball electrified in the air of this room. Then, I say, all the phenomena brought to light by atmospheric electrometers, which we observe on a fine day, would be observed just as they are. The ordinary observation of atmospheric electricity would give just the result that we obtain from it. The result that we obtain every day in observations on atmospheric electricity is precisely the same as if the earth were electrified negatively and the air had no electricity in it whatever.

Well, now I have asserted strongly that the lower regions of the air are negatively electrified. On what foundation is this assertion made? Simply by observation. It is a matter of fact; it is not a matter of speculation. I find that when air is drawn into a room from the outside, on a fine day, it is negatively electrified. I believe the same phenomena will be observed in this city as in the old buildings of the University of Glasgow, in the middle of a very densely-peopled and smoky part of Glasgow; and therefore I doubt not that when air is drawn into this room from the outside, and a water-dropping collector is placed in the centre of the room, or a few feet above the floor, and put in connection with a sufficiently delicate electrometer, it will indicate negative electrification. Take an electric machine; place a spirit lamp on its prime conductor; turn the machine for a time; take an umbrella, and agitate the air with it till the whole is well mixed up; and keep turning the machine, with the spirit-lamp burning on its prime conductor. Then apply your electric test, and you find the air positively electrified.

Again—Let two rooms, with a door and passage between them, be used for the experiment. First shut the door and open the window in your observing room. Then, whatever electric observations you may have been performing, after a short time you find indications of negative electrification of the air. Then, during all that time, let us suppose that an electric machine has been turned in the neighbouring room, and a spirit-lamp burning on its prime conductor. Keep turning the electric machine in the neighbouring room, with the spirit-lamp as before. Make no other difference but this—shut the window and open the door. I am supposing that there is a fire in your experimenting room. Then, when the window was open and the door closed, the fire drew its air from the window, and you got the air direct from without. Now shut the window and open the door into the next room, and gradually the electric manifestation changes. And here somebody may suggest that it is changed because of the opening of the door and the inductive effect from the passage. But I anticipate that criticism by saying that my observation has told me that the change takes place gradually. For a time after the door is opened and the window closed, the electrification of the air in your experimenting room continues negative, but it gradually becomes zero, and a little later becomes positive. It remains positive as long as you keep turning the electric machine in the other room and the door is open. If you stop turning the electric machine, then, after a considerable time, the manifestation changes once more to the negative; or if you shut the door and open the window the manifestation changes more rapidly to negative.

It is, then, proved beyond all doubt that the electricity which comes in at the windows of an ordinary room in town is ordinarily negative in fair weather. It is not always negative, however. I have found it positive on some days. In broken weather, rainy weather, and so on, it is sometimes positive and sometimes negative. Now, hitherto, there is no proof of positive electricity in the air at all in fine weather; but we have grounds for inferring that probably there is positive electricity in the upper regions of the air. To answer that question the direct manner is to go up in a balloon, but that takes us beyond telegraphic regions, and therefore I must say nothing on that point. But I do say that superintendents and telegraph operators in various stations might sometimes make observations; and I do hope that the companies will so arrange their work, and provide such means for their spending their spare time, that each telegraph-station may be a sub-section of the Society of Telegraph Engineers, and may be able to have meetings, and make experiments, and put their forces together to endeavour to arrive at the truth. If telegraph operators would repeat such experiments in various parts of the world, they would give us most valuable information.

And we may hope that besides definite information regarding atmospheric electricity, in which we are at present so very deficient, we shall also get towards that great mystery of nature—the explanation of terrestrial magnetism and its associated phenomena,—the grand secular variation of magnetism, the magnetic storms, and the aurora borealis.

NOTES

We have frequently had occasion to refer to the energy and work of the Perthshire Society of Natural Science, and we rejoice to see that at its last meeting it has shown an example which we hope will be followed sooner or later by all scientific societies; it has resolved to make its influence felt in parliamentary elections. On the motion of the secretary, Dr White, the following resolution was unanimously adopted:—"That in respect that Britain is apparently rapidly losing that commercial and manufacturing supremacy which has heretofore distinguished her, and that it is high time that the Government of this country should take steps to retain that supremacy, and that means towards that desirable end is the appointment of a *responsible Minister of Education* whose duty it will be to see that our education machinery in all departments, both in extent and in efficiency, is kept up to the wants of the age, and that a thorough general education in the scientific principles on which the arts are founded (and without which training mere technical schools are of no use), is put within the reach of all, this society resolves that the candidates for the representation in Parliament of the County and City of Perth be respectfully requested to state, whether, in the event of their being elected they will use their influence to urge upon the Government; (1) the appointment of such responsible Minister of Education; (2) the promotion of scientific exploration expeditions, such as that of an Arctic expedition which the late Government was in vain requested to promote; (3) the providing of means for carrying on unremunerative scientific research." The secretary was accordingly directed to communicate with the candidates.

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[*] The earth's radius is about 630 million centimetres, and its electrostatic capacity is therefore 630 microfarads, or about that of 1,600 miles of cable.

THE post of Hydrographer to the Navy has been bestowed by Mr. Goschen on Capt. J. O. Evans, R.N., C.B., F.R.S., in succession to Rear-Admiral Richards, C.B., F.R.S., who has retired.

THE first four wranglers on this year's Cambridge Mathematical Tripos, are, George C. Calliphronas, of Gonville and Caius College; Walter W. R. Ball, of Trinity College; James R. Harris, of Clare College; and Andrew Craik, of Emmanuel College.

THE following lectures in Natural Science will be given at Trinity, St. John's, and Sidney Sussex Colleges, during Lent Term, 1874:—On Sound and Light (for the Natural Sciences Tripos), by Mr. Trotter, Trinity College, in Lecture-room No. 11 (Monday, Wednesday, Friday, at 11, commencing Wednesday, Feb. 4); On Electricity and Magnetism (for the first part of the Natural Sciences Tripos and the special examination for the Ordinary Degree), by Mr. Trotter, Trinity College, in Lecture Room No. 11 (Tuesday, Thursday, Saturday, at 11, commencing Thursday, Feb. 5); On Inorganic Chemistry, by Mr. Main, St. John's College (Tuesday, Thursday, Saturday, at 12, in St. John's College Laboratory, commencing Thursday, January 29). Attendance on these lectures is recognised by the University for the certificate required by medical students previous to admission for the first examination for the degree of M.B. Instruction in practical chemistry will also be given. On Palæontology (the Annuloida, &c.), by Mr. Bonney, St. John's College (Tuesdays and Thursdays, at 9, commencing Tuesday, February 3). On Geology (for the Natural Sciences Tripos, Physical Geology), by Mr. Bonney, St. John's College (Mondays, Wednesdays, and Fridays, at 10, commencing Wednesday, February 4). Lithology: demonstrations with the microscope every Saturday at 11, commencing February 7. The class will be limited to six, and preference gived to members of the above colleges. Elementary Geology (for the First Part of the Tripos and the special examination) (Tuesdays and Thursdays, at 11. commencing Thursday, February 5). On Botany, for the Natural Sciences Tripos, by Mr. Hicks, Sidney College (Tuesday, Thursday, Saturday, at 11, in Lecture-room No. 1, beginning on Tuesday, February 3). The lectures during this term will be on Vegetable Histology and Physiology. A Course of Practical Physiology, by the Trinity Prælector in Physiology (Dr. Michael Foster) at the new museums. Lectures on Tuesday, Thursday, Saturday, at 12, commencing Tuesday, January, 27. This course is a continuation of that given last term.

DR. SCHMIDT, Professor of Astronomy in the University of Athens, has just completed his great map of the Moon. It is *two metres* in diameter, and is a marvel of accurate mapping and minute draughtmanship. The shading is so exquisite that any part of the map may be examined by a lens without the appearance of coarse or rough work. The map represents the labour of thirty-four years, and is without doubt one of the greatest astronomical results of the century.

THE discourse at the Royal Institution on Friday next, Feb. 6, at 9 P.M., will be by Mr. A. H. Garrod, Fellow of St. John's College, Cambridge, "On the Heart and the Sphygmograph."

A MARINE and Fresh-water Aquarium is to be established in the Central Park, New York, in connection with the Free Museum and Menagerie already erected there; it will be placed under the superintendence of Mr. W. Saville Kent, F.Z.S., who was, until a short time ago, Curator of the Brighton Aquarium. It is intended to raise the requisite funds by public subscription, and we are very pleased to be able to add that it is proposed to endow the Institution, so that it may be made available for the purposes of scientific research.

A PROJECT is on foot for the erection of a public aquarium at Liverpool, and a Company has been formed for this purpose; a suitable site has been secured close to the Philharmonic Hall, and operations will, we believe, be commenced at once. It is estimated the building will cost about 45,000*l.*

THE exhibition of appliances for the economic consumption of coal, which has been formed in the Peel Park, Salford, by the Society for Promoting Scientific Industry, was formally opened on Friday. Mr. J. Lowthian Bell, who had been announced to open the Exhibition, was prevented from being present, but forwarded the copy of an address which he had intended to deliver. This was read by the secretary, Mr. Larkins. The Exhibition will remain open for some weeks, and wil doubtless receive its share of public notice when the elections are occupying less attention than they are at present.

WE learn from the *Athenæum* that the Trustees of the British Museum have agreed to resign their patronage into the hands of the Government.

AN interesting peculiarity in the habits of some Indian Siluroid fishes has been noticed at a recent meeting of the Zoological Society by Surgeon F. Day, which will be described in full in the forthcoming Part of the Proceedings of that Society. Mr. Day, when fishing at Cassegode, found that, after having caught a large number of specimens of various species of *Arius* and *Osteogeniosus*, there were several siluroid eggs at the bottom of the boats, and in the fish-baskets. These eggs were, on an average, half-an-inch in diameter; and on looking into the mouths of several of the males of both genera, from fifteen to twenty eggs were seen in each; those in the boats and baskets having evidently dropped out from a similar situation. The eggs were in different stages of development, some advanced so far as to be just hatched. They filled the mouth, extending as far back as the branchiæ. No food was found in the alimentary canal, though in the females it was full of nutriment.

IN a paper on the Meteors of January 2, read before the American Philosophical Society, by Prof. Daniel Kirkwood, the author states, founding on data extending from A.D. 849 to 1864, that the meteors of this group have probably a period of thirteen years; that the mean distance is 5.53, aphelion 10.06; and that the source of the meteors may be the fourth comet of 1860, which in its ascending node approaches very near the point passed by the earth about January 3. If the period be thirteen years, the comet should have returned in the latter part of 1873, and the maximum fall of the associated meteors should occur about 1877.

Two legacies have recently been left to the French Academy of Sciences for the purpose of founding prizes. The one, a perpetual legacy of 2,500 francs, has been bequeathed by the late M. Gay, to be awarded as a prize in physical geography; and the other, a sum of 10,000 francs, the interest of which is to be awarded to the author of an astronomical work.

A GENTLEMAN in Glasgow who does not wish his name to be given, has just made a donation to Glasgow University of 1000*l.*, for the better endowment of the chairs of astronomy, botany, and natural history.

AT the meeting of the Academy of Sciences at Paris on Monday, January 26, the place in the section of Anatomy and Zoology, vacant by the death of M. Coste, was filled up. M. P. Gervais was elected, but M. Alph. Milne-Edwards was a good second, obtaining 24 votes to M. Gervais's 33.

A NEW work by Mr. F. W. Burbidge, on "Cool Orchids, and how to grow them," is announced by Mr. Hardwicke, Piccadilly. It will be illustrated by coloured plates and wood engravings, and will be furnished with a copious list, in the shape of an index, of what are termed cool Orchids.

IN a despatch from Mr. Williams, H.M.'s Consul at Samoa, to the Foreign Secretary, dated Sydney, Oct. 28, 1873, it is stated that gold in quartz has been found in a valley in that island, about three miles from the Port of Apia; the samples assayed yielded at the rate of 3000 ozs. to the ton.

MR. J. F. GARDNER, geographer to Prof. Hayden's survey, in giving a short sketch of the method adopted by him to determine the altitude of the various points occupied by the party in the Rocky Mountains, states that the experience of the surveys of California and of the fortieth parallel show that in the determination of the altitude of any point a mercurial barometer is liable to an error varying from 150 to 300 feet, even when the base barometer is at the foot of the peak, and only 3000 feet below the summit. In connection with Professor Whitney (chief of the California Survey), the following plan was adopted for correcting the errors of barometrical work. Four points were chosen at successive levels of from one to 14,000 feet. These stations were carefully connected by levellings with a spirit level, and were occupied as permanent meteorological stations. The observations taken by field parties are classified according to their heights, and each class is referred to the base station which is nearest its own elevation; the lower station being Denver, the fourth the summit of Mount Lincoln (14,000 feet), where are a number of silver mines worked by Captain Breese. The central position of this peak admirably fits it for the base of reference. Besides the barometric determination of heights, two connected systems of trigonometric levelling have been carried over the whole area surveyed, and the check observations are so arranged that the probable error can be easily determined, and it is hoped that the system will prove accurate enough to throw some light on the amount of refraction at great elevations. By these methods the altitudes of many high points have been determined, from which to construct a map in contours 200 vertical feet apart, on a scale of two miles to one inch.

SIGNALLING between the earth and the planet Venus is a suggestion made in all good faith by a French astronomer, M. Charles Cros, who considers the coming transit of Venus to be a good opportunity for ascertaining whether there are inhabitants on that planet, and, if so, entering into relations with them. He says: "It is possible that Venus is inhabited; that amongst its inhabitants are astronomers; that the latter judge the passage of their planet across the solar disc to be an object to excite our curiosity; finally it is possible that these *savants* will strive in some way to make signals to us at the precise moment when they might suppose that many telescopes will be levelled at their planet."

IN a recent communication to the Connecticut Academy of Arts and Sciences, Prof. Marsh gave a statement of the results of his recent expedition to the Far West in search of fossil remains of extinct vertebrates. He said the richest field for exploration was found in the great basin of the pre-historic lake which is now drained by the Colorado River. This body of water was originally as large as all the present lakes of the North-West combined, and had existed so long that the sand washed down from the surrounding hills had accumulated to the depth of a mile. In the different strata of this bed at least ten distinct groups of extinct animals could be detected, among them some extremely remarkable forms. One of these was a rhinoceros with two horns; but these, not like those of the modern rhinoceros, in the axis of the body, but transversely. In a space of 10 ft. square he had sometimes found the bones of 30 different animals. The number of species of extinct mammals in these remains he estimates to be three times as great as that at present inhabiting the same locality.

A PAPER on Electrical Warfare will be read by Mr. Nath. J. Holmes, at the Society of Telegraph Engineers, on Wednesday, the 11th inst.

THE new Holmes' Shipwreck Distress Signal, of great power, will be exhibited from Primrose Hill on Thursday evening, 12th, at 8.30, in presence of the Marine Secretary of the Board of Trade. This signal is self-igniting in water, and inextinguishable.

THE Naples correspondent of the *Times*, writing on Jan. 25, states that Prof. Palmieri has just published the following letter in answer to the numerous applications sent to him for information:—"The activity of Vesuvius continues to increase in the crater towards the N.E. Frequent globes of smoke issue from the bottom of it, with a kind of hissing sound, accompanied by an unpleasant odour of chloridic and sulphuric acids. Not far from it, at the commencement of the grand fissure of 1872, alkaline sublimates make their appearance. Meanwhile the fire does not yet show greater activity at the bottom of the crater, where it will probably manifest itself, unless some eccentric eruption should occur before the internal resistance of this crater is overcome. The great subterranean energy now at work does, indeed, appear to be making an attempt at an outlet in various parts. On the 21st inst. a slight undulatory shock of earthquake was felt at Casamicciola, in the island of Ischia, and during the last week many have heard the low continuous mutterings of the mountain at a distance of 15 miles. As I write, however, the sismograph, which has been very agitated for some days, is more quiet." He also reports the melancholy death at Casamicciola of Mr. Moggridge, who having bathed in the open sea, died on his road to the hotel.

WE have received the Report of the *Senckenbergische naturforschende Gesellschaft* for 1872-73, a society of long standing, and with several eminent names in its list of members. The membership, we are glad to learn, shows a considerable increase during the year; though M. v. Fritsch states, in his report, that the efforts of the society are sadly hampered for lack of funds, and that "we exist and vegetate, rather than live." He laments, also, that the museum, which once stood fifth in importance in Europe, is being quickly surpassed by other like institutions, and thrown into the background; which is hardly creditable to a city of such wealth and culture as Frankfort. Among the researches detailed in this *Bericht*, we note a paper by Dr. Koch on the Arachnida of North Africa, especially those (hitherto unstudied) of the Atlas region, and the coast of Morocco; the material having been collected by Drs. von Fritsch and Rein. The new types are not very numerous, but the remarkably wide distribution of spider-species is confirmed; and good illustration afforded of the influence of climate and other local conditions in modifying type forms. Dr. Rein describes some plants found in the neighbourhood of Mogador, and also furnishes a sketch of the vegetation of the Bermudas. A new species of perforating cirripede, *Kochlorine hamata* N., is described by Dr. Knoll; M. Scheidel contributes a note on lake dwellings and their inhabitants; and there are interesting accounts of journeys to Iceland, and to the Puglia Petrosa, in Italy.

WE have received the first Annual Report of the "Haileybury Natural Science Society." It contains preliminary lists of the fauna and flora of the place, together with observations on the meteorology of the locality, and a humorous description of an experimental dinner at which the principal dish consisted of esculent snails which had been specially fed and fattened for the purpose by certain members of the Society. It need scarcely be added, that the repast amply rewarded the members for their generous devotion to the cause of Science.

THE additions to the Zoological Society's Gardens during the past week include three Mauge's Dasyures (*Dasyurus maugei*) from Australia, presented by Mr. J. Shaw; two Vulturine Guinea Fowl (*Numida vulturina*) from East Africa, presented by Dr. J. Kirk; a Chilian Sea-Eagle (*Geranoaetus aguia*) from Bahia, presented by Mr. J. Judge; an Indian Leopard (*Felis pardus*) presented by Mr. G. D. Elphinstone; two Orang Outangs (*Simia satyrus*) from Borneo, and a Ungko Gibbon (*Hylobates variegatus*) from Sumatra, deposited; two Wanderoo Monkeys (*Macacus silenus*) from the Malabar Coast; a Brown Monkey (*Macacus brunneus*) and two Adjutants (*Leptoptilus argala*) from India, two Pheasant-tailed Pigeons (*Macropygia phasianella*) from N.S. Wales, and two Jambu Fruit Pigeons (*Ptilonopus jambu*) from the Indian Archipelago, purchased.

SCIENTIFIC SERIALS

American Journal of Science and Arts, December 1873.—In a paper on the magnetic permeability (that is "conductivity," according to Faraday), and the maximum of magnetism of iron, steel, and nickel; by Mr. Henry Rowland, C.E., the results are expressed, and the reasoning is carried out in the language of Faraday's lines of magnetic force. The quantity introduced, in mathematical theories of induced magnetisation, depending on the magnetic properties of the substance, is in these treated as a constant; but it was shown, in twelve cases of iron and two of nickel, to vary between wide limits. The author finds that the magnetisation of good iron can never exceed 175,000 times the unit magnetic field (on the metre, gramme, second, system), nor can nickel exceed 63,000 times; and from these data, and with aid of a formula of Prof. Maxwell's for tension of lines of force, it is inferred that the greatest weight which can be sustained by an electro-magnet with an infinite current, is, for iron, 354 lbs. per square inch of section, and for nickel 46 lbs. The results of experiment closely agreed with this.—Prof. Henry Draper communicates a note on diffraction-spectrum photography, accompanied with a photograph printed by the Albert-type process. (See NATURE, vol. ix. p. 223.)—We note several geological papers, one of them, by Prof. Fontaine, describing a remarkable deposit of bituminous matter, termed Grahamite, in Ritchie County, West Virginia, chemically resembling the mineral Albertite of New Brunswick, but differing considerably from this in its geological relations.—The age of the Lignitic formation of the Rocky Mountain region is far from decided, owing to the contrary evidence afforded by fossil plants and animals; and the editors propose to cite the arguments from various sources, in order, if possible, to bring about agreement. They give in this number the conclusions of M. Lesquereux from fossil plants. He refers the Lignitic beds to the Upper and Lower Eocene; and he gives a number of facts showing the disconnection of American Eocene flora from that of the Cretaceous, indicating truly separate formations.—Mr. Comstock describes the geology of Western Wyoming.—Mr. Verrill communicates the results of a recent dredging expedition on the coast of New England. It was ascertained that the body of cold bottom water approaches so nearly to the Coast of Maine as to manifest itself distinctly within twelve or fifteen miles of Cape Elizabeth, both by its highly Arctic fauna, and its icy temperature, even in summer.—In a letter from Cordoba, dated Sept. 8, 1873, Dr. Gould describes a remarkable swarm of locusts then occurring.

Astronomische Nachrichten, No. 1970, Jan. 14, contains the following papers:—On the determination of longitude by star-occultation and the telegraphically determined longitude between Madras, Singapore, and Batavia, by Dr. Oudemans. The author mentions his observations in 1859 as giving a longitude for Batavia of 7h. 7m. 12·5 s., also others in later years giving rather a less result. In 1870-71, however, the telegraphic communication with Singapore was used, giving a mean result of 11m. 40·895 s. longitude from that place. The same author gives a note on Kaiser's original proof of Foucault's pendulum researches. The proof is given by Prof. Oudemans, by which the plane of motion of the pendulum moves in azimuth in 1 sec., 15″. sin ϕ. It is too long to give in full here, but appears simple and good. Prof. Oudemans has also two other papers on position observation made during the eclipse of Dec. 1871 at Java, and on the Spheroidal form of the earth, which consist chiefly of equations and tables which we have not space to introduce.—Dr. Holetschek gives ephemerides of a number of the minor planets.

Der Naturforscher, December 1873.—This number contains notes from the Bothkamp Observatory. In one of them M. Vogel gives observations of the spectra of several fixed stars, comparing the results obtained by Huggins and Miller. Another treats of periodic changes in the atmosphere of Jupiter. The observation that the occurrence of certain coloured stripes in Jupiter, and of bright egg-shaped spots in his equatorial zone coincided with the maximum epoch of sunspots, appears to be confirmed by a number of fresh data collected by the writer, Dr. Lohse. A third note describes observations of Venus in 1871-73, by M. Vogel, who thinks it probable that the planet is surrounded with an atmosphere in which floats a thick and dense layer of condensation products, so that little insight is afforded to the planet's surface, and the observation of spots helps but little to ascertaining the time of rotation or the position of the axis of rotation.—In physics, we have a note on the curious fact which M. Budde has recently studied, viz., that chlorine, when acted on by very refrangible rays of light, undergoes expansion and heating. Some experiments, made by M. Hirn, on the optical properties of flame, tend to show that flame is not perfectly transparent to light (as Arago and M. Offret have affirmed), but that particles in the glowing state are; the weakening of light in its transmission through flames is due to the various refractions it undergoes, and consequent dispersion. The author is led to some speculations on the sun's temperature, and he puts the case thus: If the glowing parts of the photosphere are intransparent, the temperature must (according to mathematical calculation), be nearly six million degrees; if they are transparent, it must be considerably less; and the lower, the greater the transparency. The problem is one for experimental physics, the question being, Are all solid or liquid bodies transparent and diathermanous when brought to a very high temperature? M. Hirn, we have seen, inclines to reply in the affirmative. We find accounts of Prof. Guthrie's discovery of a new relation between heat and electricity, and M. Herwig's experiments on pulverisation of electrodes in the voltaic arch.—Chemistry is represented by papers on the laws governing water of crystallisation, and the reduction of carbonic acid by phosphate of iron.—The action of camphor on plant life has been recently studied by M. Vogel at Munich, in a series of experiments which confirm an almost forgotten observation by Barton in the last century, that camphor has a stimulant effect on plants analogous to that of spirituous liquors or opium, in certain quantity, on the human system. There are also botanical notes on the influence of CO_2 on verdant growth of plants (M. Böhm), and on the geographical distribution of the Cupuliferæ (M. Oersted); and, in technology, M. Riche discusses the physical properties of certain alloys.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, Jan. 21.—Prof. P. Martin Duncan, F.R.S., vice-president, in the chair.—"The secondary rocks of Scotland (second paper). On the ancient volcanoes of the Highlands and their relations to the Mesozoic strata," by J. W. Judd, F.G.S. That the rocks forming the great plateaux of the Hebrides and the north of Ireland are really the vestiges of innumerable lava-streams, is a fact which has long been recognised by geologists. That these lavas were of *subaerial* and not *subaqueous* origin is proved by the absence of all contemporaneous interbedded sedimentary rocks, by the evidently terrestrial origin of the surfaces on which they lie, and by the intercalation among them of old soils, forests, mud-streams, river-gravels, lake deposits, and masses of unstratified tuffs and ashes. From the analogy of existing volcanic districts, we can scarcely doubt that these great accumulations of igneous products, which must originally have covered many thousands of square miles, and which still often exhibit a thickness of 2,000 ft., were ejected from great volcanic mountains; and a careful study of the district fully confirms this conclusion, enabling us, indeed, to determine the sites of these old volcanoes, to estimate their dimensions, to investigate their internal structure, and to trace the history of their formation. The following is Mr Judd's conclusion on the subject of his paper:—It appears that during the Newer Palæozoic and the Tertiary periods, the north-western parts of the British archipelago were the scene of displays of volcanic activity upon the grandest scale. During either of these, the eruption of felspathic lavas, &c., preceded, as a whole, that of the basaltic; and in both the volcanic action was brought to a close by the formation of "puys." The range of Newer-Palæozoic volcanoes arose along a line striking N.E. and S.W.; that of the Tertiary volcanoes along one striking from N. to S.; and each appears to have been connected with a great system of subterranean disturbance. It is an interesting circumstance that the epochs of maximum volcanic activity, the Old Red sandstone and the Miocene, appear to have been coincident with those which, as shown by Prof. Ramsay, were characterised by the greatest extent of continental land in the area. The Secondary strata were deposited in the interval between the two epochs of volcanic activity, and the features which they present have been largely influenced by this circumstance. Apart from this consideration, however, the volcanic rocks of the Highlands are of the highest interest to the geologist, both from their enabling him to decipher to so great an extent the "geological records" of the district, and from the light which they throw upon some of the obscurest problems of physical geology.—Remarks on fossils from Oberburg, Styria, by A. W. Waters, F.G.S. The author noticed the limited occurrence of Eocene deposits in Styria, and referred briefly to the researches of Prof. Reuss and Prof. Stur upon them. He then indicated certain species of fossils which he had detected in these beds, adding about nine species to Stur's list.

Anthropological Institute, Jan. 27.—Prof. Busk, F.R.S., president, in the chair.—Aniversary Meeting.—Before proceeding to read his address, the president referred to the financial condition of the Institute, which, although it showed that the receipts were adequate for the necessary expenditure on the present economical principles of management, would not admit either of paying off any more of the debt or of increasing the scope and usefulness of the Institute. Until the unfortunate and utterly indefensible secession of members early in 1873, on a purely personal question, the Institute, since its formation, had paid off the combined debts of the two old societies at the rate of 100*l.* a year. He appealed to the loyalty of the members now forming the Institute to make a united effort finally to extinguish the debt of 800*l.* A year's income would do it, and it was suggested that if each member contributed one year's subscription, that great result would be attained and the Institute would certainly before long occupy a high position amongst the scientific bodies of the kingdom. As an encouragement to the body of members and as an earnest of the sincerity and vigour of his colleagues in management, the president had much pleasure in announcing that nearly 250*l.* had been promised by members present at a council-meeting held that day, provided the sum of 500*l.* be contributed by other members of the Institute.—The president then delivered the annual address, in which he viewed the work done during 1873 by English and foreign anthropologists. Amongst a large number of topics he adverted at considerable length to the important contributions to craniometry, by Dr. H. von Jhering and Dr. Paul Broca, criticising the respective methods employed by those distinguished anthropologists; and concluded that part of his address with the observation that the study of craniology is almost futile when applied to highly civilised, and consequently much mixed peoples, and that its results are the more certain in proportion to the purity of race. That purity at the present time was rapidly disappearing, and with it the surest data for the determination of the problems involved in the antiquity and physical origin of man.—The following was the list of officers and council elected to serve for 1874:—President—Prof. Geo. Busk, F.R.S. Vice-presidents—John Evans, F.R.S.; Col. A. Lane Fox, F.S.A.; A. W. Franks, M.A.; Francis Galton, F.R.S.; Prof. Huxley, F.R.S.; Sir John Lubbock, Bart., F.R.S. Director—E. W. Brabrook, F.S.A.—Treasurer—Rev. Dunbar I. Heath, M.A. Council—Dr. John Beddoe, F.R.S.; W. Blackmore; H. G. Bohn, F.R.G.S.; Dr. A. Campbell; Hyde Clarke; Dr. J. Barnard Davis, F.R.S.; W. Boyd Dawkins, F.R.S.; Robert Dunn, F.R.C.S.; David Forbes, F.R.S.; Sir Duncan Gibb, Bart, M.D.; George Harris, F.S.A.; J. Park Harrison, M.A. J. F. McLennan; C. R. Markham, C.B. F.R.S.; Frederic Ouvry, F.S.A.; F. G. H. Price, F.R.G.S.; J. E. Price, F.S.A.; F. W. Rudler, F.G.S.; C. R. Des Ruffières, F.R.S.L.; E. Burnet Tylor, F.R.S.

EDINBURGH

Royal Physical Society, Jan 28.—Mr. Scot Skirving, president, in the chair.—The following communications were read: Note on the Crushed Boulders from the Old Red Conglomerate in Kincardineshire, by James C. Howden, M.D., —On Crushed Boulders from Arbroath, and other localities, by Mr. Charles W. Peach.—Report of the Dredging Committee for 1873, by James Middleton, M.B., convener. The meeting of the committee had been held conjointly with the Naturalists' Field Club. In all about 133 species of animals had been obtained, including two new to the Firth of Forth.—Note on the Suspension of Clay in Water, by Mr. William Durham. This research was undertaken in continuance of those recorded in the papers on the same subject read at the last meeting. As the general result of Mr. Durham's elaborate and careful series of experiments, it was found that clay held in suspension by water sinks more quickly if the water is slightly acidulated, and more slowly if a slight amount of an alkali is added, but that the conditions are reversed if a large amount of either substance is mixed with the water.

MANCHESTER

Geological Society, Jan. 27.—Mr. J. Dickenson Hill in the chair.—Mr. J. Aitken exhibited some new fossil fishes from the millstone grit, Yorkshire, and read a paper descriptive of the bed whence they were obtained. He said that evidences of fossils had been brought to the surface during the excavations connected with the scheme for taking water from Widdop colliery to the borough of Halifax by a tunnel cut through Wadsworth Moor, about two miles north of Hebdenbridge. After an examination, by no means exhaustive, there had been discovered seven specimens of *Goniatites*, three of *Nautili*, two of *Orthoceratites*, two of *Avicula pecten*, two of *Posidonia*, one of *Gastropod*, one of *Milania*, fish remains, &c. The discovery of the most remarkable character was a new species of *Acrolepis* presenting peculiar characteristics. The situation in which these remains occurred was near but somewhat above the middle of the shells which usually divided the third floors from the fourth or undermost grit.

GOTTINGEN

Royal Academy of Sciences, Nov. 1, 1873.—M. Schering communicated a paper on the Hamilton-Jacobi theory for forces whose measurement depends on the motion of bodies.—MM. Wagner, Philippi, and Tollens described some researches on the Allyl group, made with the view of establishing the constitution of allyl alcohol, and of some of its compounds, especially acrylic acid. They find new evidence, in opposition to Wislicenus, that acrylic acid, as well as acetic acid, propionic acid, and all other organic acids, contains the group CO_2H, and may therefore be classed with them.—MM. von Grote and Tollens described an acid obtained from cane sugar by means of dilute sulphuric acid; and M. Tollens gave the first results of an investigation as to combinations of starch with alkali.

Nov. 20.—Prof. Lüroth read a paper on reckoning by projections; and Prof. Hattendorff made some observations on Sturm's theorem.

Dec. 3.—M. Enneper communicated a paper on the general theory of surfaces.

Dec. 10.—The Society celebrated its 121st anniversary. The prizes for competition in the next three years were announced. In the physical section the Society invites experiments on the artificial production of some crystallised minerals, as stephanite, pyrargyrite, grey copper ore, galena, fluor spar; in order to solution of the question how crystallised sulphur and fluor-compounds have arisen in the natural state. In the mathematical section, the Society desires an investigation of current-work, *i.e.* the work done by the electro-motive forces in their action on the current electricity, especially in its relation to the heat produced from the current, and the *vis viva* produced from it immediately in the current electricty, or mediately, in other movable particles in the conductor. Papers on these subjects must be sent before Sept. 1875, in the former case, and Sept. 1876, in the latter. The prizes offered are fifty ducats each.—Prof. Ewald communicated an interesting paper on the so-called rhetorical ornaments of Oriental speech (a subject suggested, apparently, by the late visit of the "king of kings)."—M. Riecke presented a note on the function of leaf-teeth, and the morphological value of some leaf-nectaries. In the bud, the teeth often prevent the hermetical closure of the two folded halves of the leaf; which is perhaps important, that the bud may not suffer from the want of gas. A more evident function consists in the separation of resin or mucilage. *Prunus avium* is taken as a good example; and two other types of structure are also described. The teeth of leaves of *Prunus avium* are closely allied, morphologically, to numerous nectar-secreting organs in these and other kinds of leaf.

Dec. 17.—M. Bjerknes read a paper giving a generalisation of the problem of motions produced in a still inelastic fluid by the motion of an ellipsoid.—M. Wohler presented a list of the meteorites in the University collection at Gottingen.

VIENNA

Imperial Academy of Sciences, Dec. 4, 1873.—Prof. Mach stated that he had made experiments, during the summer, on the time required for rotation of the plane of polarisation by a current—a flint glass disc being rotated between the magnetic poles; but similar experiments by Villari had been described in *Pogg. Ann.* (No. 7, 1873), and the results were almost identical. Villari used a double plate; and Prof. Mach points out another very simple method for such researches, viz., the spectral observation of a sounding glass rod placed between the magnetic poles.—A paper by Dr. Dvorak described some experiments on the velocity of sound in gas-mixtures. If a mixture is made of two different gases, with densities d and d' respectively, and both with an expansive force 1, the velocity of sound V in the mixture $= \sqrt{\frac{2}{d + d'}}$ The author's results show close agreement with the theory. Thus for mixtures of carbonic acid and hydrogen, air and hydrogen, ordinary gas and CO_2, respectively, the observed and calculated numbers for the half wave-length of a given tone were these: 71·5, 71·0; 88, 89·0; 64, 63·3. The author remarks that for a simple gas, as well as for a mixture of gases, the gas theory implies not one velocity, but a graduated series of velocities, of sound; and perhaps the prolongation in sound of a cannon shot heard at a distance may be thus explained.—Dr. Exner communicated a determination of the temperature at which water has a maximum of density. He improved on Rumford's method by using thermo-elements instead of a mercury thermometer. The value obtained was 3·945°.

PARIS

Academy of Sciences, Jan. 26.—M. Bertrand in the chair.—The following papers were read: On the various reactions of the compounds of oxygen and nitrogen, by M. Berthelot.—On the production of yeast in a mineral solution containing sugar, by M. Pasteur. The author described the growth of yeast in a solution of inorganic substances such as enter into the composition of its ash added to a solution of sugar. M. Trécul replied at some length to certain of M. Pasteur's remarks.—On the liquefaction and solidification of acetylene by the silent electric discharge, by MM. P. and A. Thenard. The author found that this gas condensed at the rate of four or five cubic centimetres a minute into a solid horny body isomeric with acetylene; by varying the conditions of experiment a liquid isomer was also obtained.—Experimental researches on Newton's rings, by M. P. Desains.—Direct demonstration of the equation $\int \frac{dQ}{T} = 0$ for every closed and reversible cycle, by M. A. Ledieu. This paper formed a sequel to the author's other papers on thermo-dynamics, lately published.—Note on Poncelet's teaching of applied mechanics, by General Morin.—A note from Prof. Nordenskiold was read; he has detected iron, nickel and cobalt in the carbonaceous dust found in 1870 on the Greenland snow; traces of phosphorus were also found.—Instructions for M. Doûmet-Adanson's travel in Tunis, by M. Cosson. The instructions are issued to M. Adanson, who is about to undertake a botanical exploration of Tunis.—On magnetism, by M. J. M. Gaugain.—New researches on the rejoining end to end of the fibres of sensory with the fibres of motor-nerves, by M. A. Vulpian.—Organogenesis compared with androgenesis in its relation to natural affinities, by Ad. Chatin. This portion of the author's paper deals with the *polygonoid* and *cactoid* plants.—Researches on the silicified plants of Autun; study of the genus *Myelopteris*, by M. B. Renault.—On the presence of a considerable proportion of potassic nitrate in two varieties of *Amaranthus*, by M. A. Boutin. The author found that *A. atropurpureus* contained 22·7 and *A. ruber* 16·0 per cent. of the weight of the dried plant; he suggested a possible future cultivation of the plant on this account.—On the theory of the flight of birds, by MM. H. and L. Planavergne.—On a statistical chart showing the distribution of the population of Paris, by M. Vauthier.—On the geometrical properties of rational fractions, by M. F. Lucas.—On the determination of the pluckerian numbers of envelopes, by M. H. G. Zeuthen.—On the theory of numerical equations, by M. Laguerre.—On the breaking of magnetised needles, by M. Bouty. The author found that if the steel was very brittle and broke like glass the two portions are magnets of the same magnetic moment, but not so if the steel has to be bent backwards and forwards before it breaks.—On certain peculiarities in the efflorescence of the two hydrates of sodic sulphate, by M. D. Gernez.—Researches on the reaction of argentic chloride on phosphoric di-iodide, by M. Arm. Gautier.—On the isomerism of terebenthene and terebene, from a physical point of view, by M. J. Riban.—On the alterations of the soft matter (of the brain) accompanying the tearing and cutting back of the sciatic nerve in the rabbit, by M. G. Hayem.—On the pluvial *régime* of the torrid zones in the Indian and Pacific Ocean basins, by M. V. Raulin.—Note on Professor Tyndall's experiments on the acoustic transparency of air, by M. W. de Fonvielle.—On the production of crystals of calcic oxalate and ammonic-magnesic phosphate, by M. E. Monier. During the meeting, the Academy elected M. P. Gervais as successor to the late M. Coste, of the section of Anatomy and Zoology.

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THURSDAY, FEBRUARY 12, 1874

A MINISTER FOR SCIENCE

WE are glad to see that the *Times* has at last opened its pages to the question of the propriety of appointing a responsible Minister, whose duty it shall be to look after the interests of Science and of scientific research and education, and take charge of the scientific institutions of the country—institutions whose efficiency is at present sadly crippled from the want of a single responsible head. The whole question could not be better stated than in Colonel Strange's letter which appeared in the *Times* of the 6th inst., and which we consider so admirably to the point, that we gladly reproduce it here. We hope the letter will lead to further discussion, and that whatever Government may hold the reins in the forthcoming Parliament, the important question now raised may meet with serious attention. Colonel Strange's communication runs as follows:—

"No subject affecting the material interests of England is more important at the present day than that discussed at Manchester by Lord Derby, and by yourself in your leader of the 12th ult.

"'Scientific industry' is one of those clever phrases calculated to catch the eye and ear by its novelty, while it expresses what is already well known by other antiquated names. Lord Derby defines it and explains its meaning in a variety of ways; but throughout his whole speech he is talking, while never naming it, of nothing more nor less than scientific research. The utilisation of redundant natural forces and of waste substances, on which he insists as a primary object of the new movement, is to be brought about by patient, continuous, systematic research, and by nothing else. I own I prefer the old words to the new, but if by using new words old wants come to be recognised and supplied, I shall not complain.

"I, and many who think with me, maintain that scientific research must be made a national business; that the point at which Science, in most of its leading branches, has now arrived and the problems presented for solution are such as to need for their adequate treatment, permanent well-equipped establishments with competent staffs worked continuously and systematically. Lord Derby truly describes it as a case in which what is 'everybody's business is nobody's business.' We must make it somebody's business. We must make it the State's business. We have tried individual enterprise, which so many hold to be all-sufficient. There is more individual enterprise in England than in any country in the world, and yet we are being rapidly outstripped by nations who, though they encourage private exertion, are wise enough not to rely on it, but to establish a system free from the caprice, the incompleteness, the liability to interruption and cessation incident to all individual labour in whatever field. If asked to describe the system we propose to establish, our reply is in one word, 'completeness.' A steam-engine is a system, composed of many parts, each and all essential to its useful action. Furnace, boiler, cylinder, pistons, connecting rods, beam, and fly-wheel—all controlled by a governor. Tested by the condition 'completeness, what is Lord Derby's new society? What is any private society? A mere connecting rod—a most useful link in the system, not to be dispensed with, but still a mere link. Where are the other parts? Is there a trace of them in England?

"The first essential to any system is a head. No domestic household, no manufactory, no ship, no army or navy, no public or private establishment of any kind, and these are all 'systems,' can hold its own for a day without a head. But at the present hour there is no head to the science of England. The proposed remedy for this deficiency will have been anticipated as obviously a Minister of State, who shall be responsible to the nation through Parliament for everything connected with the scientific business of the country. For want of this head what have we done? The various scientific institutions at present maintained by the State are distributed according to the following list, which was correct some time since, but may have undergone recent changes:—1, Royal Observatory, Greenwich, under the Admiralty; 2, Royal Observatory, Edinburgh, under the Office of Works; 3, Royal Observatory, Cape of Good Hope, under the Colonial Office; 4, 5, 6, the Observatories at Madras, Calcutta, and Bombay, under the India Office; 7, Ordnance Survey of Great Britain, under the Office of Works; 8, the Great Trigonometrical Survey of India, under the India Office; 9, Exchequer Standards Office, under the Board of Trade; 10, the Royal School of Mines, under the Privy Council: 11, British Museum, under 50 irresponsible trustees; 12, Meteorological Office, governed by an unpaid, and therefore irresponsible, Committee of the Royal Society, under the Board of Trade; 13, the Royal Botanic Gardens of Kew, Edinburgh, and Dublin, under the Board of Works; 14, the Geological Survey, under the Privy Council. My list is perhaps not quite complete, but as it stands it shows that we place our scientific institutions under no less than seven different Departments of State, all of which have other matters besides science to attend to. Can anyone pretend there is any trace of a system here? Is it not a grotesque caricature of State administration?

"Granted that there must be a Minister for Science—and I am happy to say that those who have given most attention to the question now admit that there must—then the whole of the institutions I have named, besides some others now in existence, and many others that must before long be founded, would be placed under him. This would secure the great object of harmony and unity of parts, of provision for modification and extension, and of definite responsibility to the nation through Parliament, none of which objects are obtainable or seem even dreamt of at present.

"Whether such a Ministry should be created as additional to what we at present possess, or whether some existing Minister should be charged with Science; whether the Science Minister should not also take Education, Art, and Music under his care; whether he should not have permanent unparliamentary advisers, and if so on what scale and how constituted, besides many other points, are all extremely important questions, admitting of a great variety of answers; but compared with the fundamental necessity for a Minister at the head of a Department controlling the whole public scientific activity of the kingdom, they are matters of subordinate detail.

"The Royal Commission on Science, presided over by the Duke of Devonshire, has, for nearly three years, been most assiduously engaged in collecting a body of information of infinite value, and they will no doubt forward many important recommendations on the evidence they have taken; but for my part, as one deeply interested in their proceedings, to which I have contributed largely as a witness, I do not hesitate to say that if they only succeed in obtaining the creation of a Science Minister, that result alone will amply repay the country for the cost of their investigations.

"Let this be done, and we should cease to witness the farce of consulting the Chancellor of the Exchequer about observing eclipses of the sun, the Prime Minister about scientific Arctic expeditions, and the Treasury about tidal reductions. We should perhaps, too, then perceive that overworked Law Officers are not the best managers of a great, or what should be a great, technical Museum, and that fifty irresponsible gentlemen, however eminent individually, ought not to be entrusted with the grandest collection of Art and Natural History in the world. Nor would a wise statesman like Lord Derby fail to perceive, with all science concentrated under one view for his inspection, that a private local Society will prove no match for the complete and powerful State systems of Germany, France, and other Continental nations."

PINK AND WEBSTER'S "ANALYTICAL CHEMISTRY"

A Course of Analytical Chemistry (*Qualitative and Quantitative*). By William W. Pink and G. E. Webster. (London: Lockwood & Co., 1874.)

THIS work forms a volume of Weale's Rudimentary Series, and is advertised "as specially adapted for the use of those students who intend competing in the Advanced or Honours Stage Examinations (Inorganic Chemistry) of the Science and Art Department, also for preparing those intended to sit for the higher class examinations at Colleges, Public Schools," &c. The success which several well-known serial publications of a similarly special nature have deservedly had, appears to have stimulated the publishers of Weale's Series to embark in this enterprise. As the excellence of most of their former publications will be generally admitted, we can only regret that a literary (?) production displaying such deplorable ignorance should ever have found a place in their series. It has rarely been our duty to pass judgment on a more carelessly got-up book. Had it not been advertised as specially adapted for the use of the Science Classes under the Science and Art Department, we might have put it aside with a hearty laugh over the many absurd blunders it contains. Since a practice has, however, sprung up of late to cater for the wants of Science Classes, by printing books (sometimes obtained on commission) privately, and advertising them by means of post-cards, at so many postage stamps a copy; whereby these books manage to escape the eye of the reviewer, and as we fear that much mischief is being done by certain cheap cram-books, strung together with a view to save the teacher as much trouble as possible, our readers will perhaps bear with us if we examine the book before us somewhat closely. If rumour speaks true, some teachers manage to teach chemistry—even analytical chemistry—without ever touching a test-tube or performing the simplest experiments. Questions from previous examinations are eagerly collected and "worked" in the belief that the examiner is sure to give, if not the same questions, at least others of a similar nature. We need not fear giving offence to those earnest and hard-working men, engaged, often on a mere pittance and under most adverse and discouraging circumstances, in imparting a sound knowledge of chemistry in places which would not otherwise be reached by any educational efforts, if we conclude from the course of analytical chemistry before us, that some teachers (Mr. Webster styles himself Lecturer on Metallurgy and the Applied Sciences, Nottingham) are deplorably ignorant of the science they profess to teach.

Beginning on p. 4, we are told that "the term atom is sometimes applied to a compound as well as simple radicals, such as ammonia, hydroxyl, &c.": that "for fixed solids which do not vaporise, the atomic weights are referred to the element lithium, the atomic weight being determined by the amount of heat which any body contains, when it is at the same temperature as lithium, both being the same weight, lithium being considered as seven." On p. 7, "difference of attraction is called the bond affinity, that is, it is assumed that the different atoms possess power, lines of force, or *points of attraction*, called by Dr. Frankland *bonds*." On p. 12, we are informed, that "there are four different forms of notation, or formulæ in present use, two of which are *graphical*, viz. the *glyptic* and *graphic* formulæ. The other two, viz. the empirical and the constitutional or rational, are the symbolic representations." We give it upon the authority of our joint authors, that "Dr. Crum Brown was the first to introduce this form of formulæ, and that it has now been adopted by Dr. Frankland, and generally throughout the kingdom." And on p. 14, we are told, that "students who do not already understand the constitutional formulæ are strongly advised to obtain a complete knowledge of them, not only as an addition to their knowledge, but because the other is now not recognised by many colleges, or *allowed* in many examinations." For fear our authors' inadvertence should lead to further mischief, we may at once state that, to our knowledge, such is not the case, and that the authors are as much in the dark about what is recognised by many colleges or "allowed in many examinations" as they are about chemical analysis.

We can only pick out some of the choicest specimens from the authors' analytical bouquet. Beginning on p. 26, we are told that "deflagration is the arrangement of the crystals of a substance, and is, in ordinary terms, the crackling of a body when exposed to heat;" on p. 28, that "hardly any amount of reading or lecture-hearing can produce a practical analyst, as only practice can make perfect, and therefore the student is strongly recommended to make the experiments himself." We for once entirely agree with the theory, but strongly object to the "practice" of our joint authors. The information on p. 30, that "melted lead cannot be poured even in a cold platinum crucible without spoiling it, and that a drop of lead, tin, or bismuth, falling upon a red-hot platinum vessel invariably makes a hole in it," we owe probably to the sad experience gained by the metallurgical partner in the joint-authorship, and science-students possessing platinum vessels must surely feel thankful for the hint. Great confusion of ideas seems to prevail, however, on the subject of platinum, for we are told on p. 31 that "platinum combines easily with silica and carbon, so that the contact of platinum crucibles with charcoal at a very high temperature must be avoided," together with several other absurd precautions which we will not quote. On p. 33, there figures an apparatus for rapid filtration in an atmosphere of steam, which we have seen before in Normandy's Introduction to his translation of Rose, and which we should have thought had been superseded long ago by more perfect methods of filtration.

As specimens of analytical knowledge (?) we quote p. 57, "hydrochloric acid gives a precipitate on dilution with water (distilled) if $BaCl_2$ or SO_2Ho_2 be present;" p. 38, "dilute sulphuric acid contains more lead, and lead is scarcely soluble in concentrated acid;" p. 42, "A solution of baric chloride must be neutral to test-paper, after precipitation by sulphuric acid;" p. 43, "sodic carbonate must completely volatilise;" p. 46, "hydrofluosilcic acid can be obtained from the chemists in india-rubber bottles."

The analytical tables on pp. 57 to 73 are equally deficient and faulty. We are told to test for ammonia, after having ignited on platinum foil; "a watch-glass becomes corroded on the addition of baric chloride to a neutral solution of salts," "hydrobromic acid turns starch-paper blue," "sodic hydrate," on p. 94, "precipitates light-coloured ferric hydrate which turns dirty-green." Upon heating chlorates, p. 115, "a very violent deflagration ensues." The authors appear never even to have prepared oxygen gas.

The quantitative knowledge displayed by the authors is quite on a par with the choice bits of qualitative chemical information so liberally and innocently volunteered by them. We will not tire our readers, however, by any further quotations, but cannot refrain from firing a parting shot or two by quoting from p. 120, where we are told that "Chlorine is prepared by the mixing of salt, hydrochloric acid and manganic oxide; this last, MnO_2, has no chemical reaction in the last equation;" and from p. 136, on which we are told that "in order to keep the edges of the balance free from rust, it is a very good practice to place inside the case a beaker, half-filled with sulphuric acid or baric chloride." A dialyser is described on p. 171 as "an apparatus having sides and top of gutta-percha, and bottom of parchment, and is used for the separation of urea and other crystallisable salts from urine."

Need we do more than recommend the authors to act upon their own advice (p. 2), and "to speedily endeavour to obtain a complete knowledge of the composition of bodies, and make themselves conversant with the formulæ &c.," of which they exhibit so deplorable an ignorance, before they again venture upon enlightening the public on the subject of chemistry.

THE RACES OF MANKIND

The Races of Mankind: being a Popular Description of the Characteristics, Manners, and Customs of the Principal Varieties of the Human Family. By Robert Brown, M.A., Ph.D., &c. (Cassell, Petter, and Galpin).

THE rapid growth of interest in Anthropology is proved by the appearance, one after another, of popular illustrated works: Mr. J. G. Wood's "Natural History of Man" in 1868-70, an English translation of M. Louis Figuier's "Human Race" in 1872, and just now (though without the date it ought to have on the title-page) this first volume of a work on "The Races of Mankind." Of these, the productive M. Figuier's book is too worthless to say much of, and the comparison lies between the first and last. Both are valuable, and the ground they cover is so far different, that they may be usefully placed side by side in the ethnologist's library. It will be remembered that Mr. Wood's account of Africa occupied the first of his two volumes, so that his account of the races of Asia, America, Polynesia, &c., had to be disproportionately condensed into the second. Dr. Brown, we trust, will be able to keep his scale more uniform. His first volume treats entirely of American races, and he speaks with personal knowledge of the Esquimaux and North-west tribes, compiling as to other tribes with discretion, and generally from not too hackneyed authorities. Such of Dr. Brown's illustrations as are taken from photographs and real drawings are good, and preferable to the too picturesque and imaginative cuts of Mr. Wood's artists. But Dr. Brown inserts some drawings which he had better for truth and good taste have left out. Thus, the Indian scalping his victim at page 68, though no doubt more like the reality than the engraving in vol. ii. of "Schoolcraft," from which it is a kind of rationalised copy, is a piece of sensational make-up; while on the next page a scene of Indians torturing a captive by a slow fire on his stomach, is still more objectionable. At page 284 is a representation of Conibos shooting turtle; this is evidently a fancy picture, and arrows shot at such an angle would glance off the animal's carapace; the arrows should have been shown of heavier make, and so sent up as to fall almost perpendicularly.

As only the first part of Dr. Brown's work is yet out, it may perhaps be a service to make some suggestions. Native words are sometimes wrongly printed, which gives an air of carelessness to the descriptions they form part of. Thus "inniut" instead of "innuit" (p. 5); "Manco Capas" and "Manià Dello" (p. 119), which appear to be intended for the usual forms, "Manco Capac" and "Mama Oello" (Mr. C. R. Markham would say that "Ccapac" and "Ocllo" are the really proper forms). At page 274, the account of the "couvade," the custom of the husband being put to bed or otherwise treated with reference to his wife's bearing a child, is compiled very inaccurately. Lastly, though references are generally given where long abstracts have been made from books of travel, Dr. Brown seems somewhat apt to make statements and use arguments without due mention of the sources whence he derived them. One consequence is, that he makes himself personally responsible for any blunder in the matter he thus appropriates. Thus, at page 147 a passage is inserted of which the following is part:—"In the Ladrone Islands, the Spaniards found the natives unacquainted with fire; and when Magellan set fire to the huts of the Marian islanders, they looked upon the flame as a living creature which fed upon wood." Unless my memory deceives me, this passage is copied out of Buchner's "Man in the Past, Present, and Future," and has been already commented on in NATURE, not only as embodying statements which have been disproved, but as showing a certain geographical weakness in the writer, who did not know that the Ladrone and the Marian Islands are the same. E. B. T.

OUR BOOK SHELF

Typhoid Fever: its Nature, Mode of Spreading, and Prevention. By William Budd, M.D., F.R.S. Pp. 193. Three plain and one coloured lithograph. (Longmans, 1873.)

THIS handsome volume is a thesis on the question of how typhoid or enteric fever is propagated. Dr. Budd adopts what is known as the contagion theory, and believes that every case of the disease is the result of direct poison, conveyed either by the air or more frequently in water, from the intestine of one patient to that of another. This theory is generally disbelieved by the best medical authorities in London and Paris; but, as Dr. Budd points out, it is not in large towns that the transmission of disease can best be traced. He describes with minute exactness as to time, place, and other important conditions, outbreaks of this terrible disease in secluded country villages, in schools, and other isolated institutions, where he was able to trace the steps of the epidemic from house to house or from room to room. We believe that a candid perusal of these cases will bring the conviction that the theory of contagion is fairly proved. Many of them are at all events almost decisive against the theory that this enteric fever is "pythogenic," *i.e.* is the result of a poison which may be produced by any decomposing sewage under favourable circumstances, without previous contamination from a diseased person. The practical importance of the question is, that if enteric fever only spreads as Dr. Budd and other contagionists maintain, it is possible, and therefore of the utmost importance, to check its propagation. A great part of the book is devoted to this point, and the mode of destroying diseased products is carefully detailed.

One obvious objection to the contagion theory is that it only accounts for the spread, and not for the origin, of the fever. But, as Dr. Budd argues, the same applies to small-pox and every other undoubtedly contagious disease. However the first case came about, no one supposes that fresh ones now arise spontaneously, any more than naturalists who believe that worms and buttercups once came into being for the first time, expect to find a worm appear in a drop of water without an egg, or a buttercup in a meadow without a seed.

The comparison of typhoid disease to the eruption of small-pox, which is revived by Dr. Budd, has been long and deservedly abandoned: indeed the strictly pathological part of this book is the least satisfactory. Notwithstanding a somewhat "drawing-room" appearance, it is no doubt intended for pathologists and physicians to study; and for them we cannot see the advantage of the four illustrations, one of which forms an elaborately coloured frontispiece; they show nothing but what has often been figured before, and is now universally familiar. The style also is now and then too ambitious, suggesting rivalry with the wretched newspaper writing quoted on p. 110 as "lively and facile." On the whole, however, the book is as solid as it is earnest, and may be compared without detriment with Dr. Macnamara's well-known work defending an almost identical theory and practice with regard to the propagation and prevention of Asiatic cholera.

The facts and arguments contained in it will no doubt be duly weighed by the medical profession, and the public will benefit by the result. P. S.

Inorganic Chemistry, Elementary. By Raphael Meldola, F.C.S. (London: Thomas Murby, 1873.)

THE present little volume constitutes one of a series produced by the same publishers as "Science and Art Department Text-books." We must congratulate Mr. Meldola on having produced in a small compass a thoroughly good and sound introduction to the science of chemistry, and it is all the more welcome in these days of "Science Series," when so many badly done "Text-books" are being produced. The information is well and clearly stated, and is sufficiently free from technicalities to be easily understood by the beginner. The book is plainly and well printed, but we cannot congratulate the publishers on the execution of the few and simple woodcuts, every one of which has been spoiled in the cutting. We hope that in a future edition the work will receive better treatment, as a well-done woodcut is a great aid to the beginner in understanding his author's descriptions of various experiments.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Photographic Society

THE sweeping condemnation of the Photographic Society conveyed in an article in NATURE, vol. ix. p. 263, can only have been written under a want of knowledge or misrepresentation of facts. I will not say one word about any dissension which may exist in the Society, but as the statements you have published are calculated to injure the Society very materially, I will ask you, in common justice, to make public the transactions of the Society for the past year, so that the readers of NATURE may judge for themselves whether in a body which does not profess to be a purely scientific one, science is altogether ignored, whether "no man of eminent scientific capacity takes part," and whether the society is altogether beneath contempt as at present conducted. This I ask you to do not only in justice to the society, but to the gentlemen whose names are mentioned below.

1873. January meeting. "The Photographic Operations at the Royal Observatory in connection with Magnetical and other records," by James Glaisher, F.R.S.; "Instantaneous Microphotography," by E. J. Gayer, M.D.

February.—"On the Principles of the Chemical Correction of Object-Glasses," by Prof. G. G. Stokes, D.C.L., Sec. R.S.

March.—"A Contribution to the Early History of Photography," by H. Baden Pritchard, F.C.S.

April.—"Uranium Printing," by John Spiller, F.C.S.; "The Chemical Theory of the Latent Image," by Capt. Abney, R.E., F.C.S., F.R.A.S.

May.—"Improvements in Carbon Printing," by Mons. A. Marion.

June.—"Remarks on three Wet Processes," by Jabez Hughes; "Photo-collotype Printing," by Capt. Waterhouse, B.S.C.

December.—"Photography in the Arctic Regions," by Lieut. Chermside, R.E.

So far as investigations are concerned, I can mention two, at least, now being undertaken by members of the society, touching the process and nature of film best suited for the Transit of Venus observations.

BADEN PRITCHARD, Hon. Sec.

9, Conduit Street, W., Feb. 7

Animal Locomotion

THERE are two or three points in Dr. Pettigrew's new book as to which, perhaps, many of your readers in common with myself would be glad of a little light. First, in speaking of the gannet, he says: "Each wing, when carefully measured and squared, gave an area of 19½ square inches." But how is such an area obtained from the dimensions given? They are: "girth of trunk, 18 inches," *i.e.*, about 5 inches for its width; "expanse of wing from tip to tip across the body, 5 feet," so that each wing would stretch about 33½ inches from root to tip; "across secondaries, 7 inches," and this we may take as about the average width of the wing. Multiplying length of wing by width (33½ × 7), we get therefore an area of 234½ square inches. Similarly Dr. Pettigrew assigns the heron's wing an area of 26 square inches, although the dimensions he gives yield an area of about 311 square inches. A friend of mine has the temerity to suggest that for some reason or unreason Dr. Pettigrew has divided the true area by 12, for so 234½ (if we neglect the half inch) gives just 19½, and 312 instead of 311 gives 26.

But (as a second matter) my friend's notion of Dr. Pettigrew's arithmetic receives some colour from the sentence following the one before quoted, viz., "The wings of the gannet, therefore [each wing being supposed 19½ square inches], furnish a supporting area of 3 feet 3 inches square." So of the heron. Having told us that the area of each wing is 26 square inches, he says, "Both wings consequently furnish an area of 4 feet 4 inches square." Here, surely, square inches have been treated as if they were linear, and only 12 of them instead of 144 reckoned to the square foot!

Once more (as was observed in your review a week or so ago), Dr. Pettigrew maintains, in opposition to all other experimenters, that in flight the downward stroke of the wing is directed forwards and not backwards. Now, to say nothing of the "singularity" of representing the wings in his own case as concavo-convex, and in that of his opponents as flat (much to the detriment of the latter), the whole of Dr. Pettigrew's "mathematical demonstration" of his position is so extremely original that I fear for the uninitiated it is only explaining *obscurum per obscurius*. Would he condescend to accepted methods and prove his case by the parellelogram of forces? As it is, his proof amounts simply to this:—"As the under surface of the wing, which is a true kite, looks upwards and forwards, it tends to carry the bird upwards and forwards." No doubt, if the wing remain still, and the bird have already a sufficient velocity. A kite is sustained or elevated by an extraneous force, either the wind impinging against its under surface, or *vice versâ* when the boy runs. But whence comes the bird's motion, before its wings can act as kites? Dr. Pettigrew nowhere tells us this, but starts with his bird already flying. Thus he says:—"The bird, when flying, is a body in motion. It has already acquired momentum. If a grouse is shot on the wing, it does not fall vertically downwards, as Borelli and his successors assume [Shade of Borelli!], but downwards and forwards. The flat surfaces of the wings are consequently made to strike downwards and forwards, as they in this manner act as kites to the falling body, which they bear or tend to bear upwards and forwards." Here it is unmistakeable that the function of the wings in generating velocity is confounded with their function in directing the velocity already generated; just as if one should confound a steamer's rudder with her screw. The question is, How do the wings generate velocity? In this respect it is immaterial whether the bird is at rest or in motion. But to this there can be but one answer, at least if we are still to believe that "action and reaction are equal and opposite;" the answer that is, that everybody gives but Dr. Pettigrew. The downward motion of the wing is wholly concerned with sustaining or elevating against gravity. A backward movement must carry the bird forward; Dr. Pettigrew's forward movement must make it fly tail first.

JAMES WARD

Trinity College, Cambridge, Feb. 2

YOUR reviewer resorts to very strong language, without, it appears to me, justifying his procedure. In reviewing my volume he exclaims, "Imagine our disappointment on finding that, instead of the work being by the hand of a master, its author is deficient in the knowledge of the first principles of physics, and of the undoubted meaning of some of the most simple terms employed in the science; his argument, if it may be so called, being but little more than a long series of vague and fanciful analogies, incorrectly stated physical facts, and untenable theories." . . . "We must say that we expected better things of Dr. Pettigrew, and regret that he has not, before now, learned that there are errors in his methods and results that cannot be tolerated by a thinking public, which prefers accurate reasoning rather than dogmatic statement, and well-grounded fact to fanciful analogy" (NATURE, vol. ix. p. 221). One would naturally have expected after such announcements an exposure of false theories and a criticism of the nomenclature employed, but Mr. Garrod condescends upon neither. He takes refuge in general statements and implies what he does not attempt to prove.

He states, *e.g.* "that it is at present impossible to obtain from any form of fuel, a sufficient percentage of the potentiality which it possesses for doing work, to work an engine sufficiently compact and light for the wings which it has to drive." Now this is utter nonsense. In 1868 Mr. Stringfellow, of Chard, Somersetshire, exhibited at the Crystal Palace a flying machine which with its engine, boiler, water, fuel, flying surfaces, and propellers only weighed 12 lbs. The engine of this machine exerted the third of a horse power and obtained the 100*l*. prize of the Aeronautical Society of Great Britain as being at once the lightest and most powerful steam-engine ever made.

What bird weighing 12 lbs. can Mr. Garrod inform me exerts a third of a horse power in flying?

This one fact proves that in the ordinary steam-engine we have a power more than equal to the production of flight.

Mr. Garrod takes exception to my statement that "weight when acting upon wings, or, what is the same thing, twisted inclined planes, must be regarded as an independent moving power."

This point will be best illustrated by an example. If a gannet drops from a cliff with *expanded motionless wings* it can sail for an incredible distance, the weight of the body dragging upon the wings, doing the principal part of the work. This is a matter of observation, and the principle may be exemplified by the following simple experiment. If an apparatus composed of two quill feathers stuck in the end of a cork be made to fall from a height it will be found to travel *downwards and forwards* in a curve, the forward curve equalling half the space through which the apparatus descends. Here we have no muscular movement to direct or influence the motion in any way, and it certainly seems to me to afford an explanation of the manner in which mere *weight*, or gravity acting upon weights, may by the aid of wings be made to propel a body from one point to another.

Mr. Garrod proceeds—"After such indications of imperfect knowledge, nothing in the way of mechanical theories could cause surprise, and we are therefore not astonished to find it laid down as the fundamental principle of flight, that *the up-stroke of the wing aids in propulsion*, and that in the down-stroke *the inferior surface of the wing is directed downwards and forwards*." If Mr. Garrod attempts to elevate a natural wing or an artificial one properly constructed, even in a strictly vertical direction, he will find that it inevitably darts upwards and forwards in a curve and carries the hand with it. In this manner, as experiment proves, *the ascent of the wing aids in propulsion*. If again Mr. Garrod attempts to depress the wing vertically downwards, he will as certainly find that it darts *downwards* and *forwards* in a curve, the hand being carried in the direction specified. The upward forward and downward forward curves, being united as they are in flight, give a *waved track*. If the wings did not dart *forwards* both during their ascent and descent, the body of the bird could not be transferred from one place to another in a horizontal waved line which it is. Mr. Garrod is evidently imperfectly informed on the subject of flight, for he inquires "Who can see any close relation between the flight of birds and that of a kite?" The merest tyro in mechanics will, I think, perceive this on a moment's reflection. The kite is pulled *forwards* on the moving air by the string. The kite formed by the wings of a bird is *pushed* forwards on the moving air by the weight of the body.

I do not forget, as Mr. Garrod insinuates, that a kite requires a string. The following passage, written in 1867, will show this. "The wing of a bird acts after the manner of a boy's kite, the only difference being that the kite is pulled forwards upon the wind *by the string* and the hand, whereas in the bird the wing is pushed forwards on the wind by the weight of the body and the life residing in the pinion itself."*

Mr. Garrod's words are—"Dr. Pettigrew seems to forget that a kite needs a string, and yet, backed by his false analogy, *he has the presumption* to quote the experimental verifications and opinions of such able and ingenious thinkers as Borelli and Marey, the authors of the true theory of flight, only to reject them." To one who has experimented on the subject of flight for the last 10 years, the term *presumption* in this sentence sounds strange. One may, I venture to think, without presumption, differ from another after such mature deliberation. Marey's theory of flight, which is nearly, if not identical, with my own, was not promulgated till nearly two years after I had published mine. This point will be fully discussed in the *Athenæum* of Feb. 14. In fact Marey frankly admitted this in a letter to the French Academy of Sciences in reply to a reclamation lodged by me with that learned body.

His words are:—"J'ai constaté qu'effectivement M. Pettigrew a vu avant moi, et représenté dans son Mémoire, la forme en 8 du parcours, de l'aile de l'insecte; que la méthode optique à laquelle j'avais recours est à peu près identique à la sienne. . . . Je m' empresse de satisfaire à cette demande légitime, et je laisse entièrement la priorité sur moi, à M. Pettigrew relativement à la question ainsi restreinte." (*Comptes Rendus* for May 16, 1870, p. 1093.)

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* On the various modes of flight in relation to aeronautics: Proc. Roy. Instit. of Great Britain, March 22, 1867.

The next point which Mr. Garrod takes up is the "induced currents" of the wing. I state that "the efficiency of the wing is greatly increased by the fact that when it ascends it draws a current of air up after it, which current, being met by the wing during its descent, greatly augments the power of the down-stroke. In like manner, when the wing descends, it draws a current of air down after it, which, being met by the wing during its ascent, greatly augments the power of the up-stroke." This is simply a statement of fact, and if Mr. Garrod causes a natural or artificial wing to vibrate he will find that the wing takes a greater catch of the air when a down- and up-stroke or an up- and down-stroke are made in rapid succession, than when a single stroke is made either in the one direction or in the other. This point becomes especially clear if a large artificial wing be constructed on the insect type and made to vibrate in a horizontal direction. If such a wing have its anterior margin slightly elevated and made to travel from right to left of the operator it draws after it a current of air which, being met by the wing when it is reversed and made to pass from left to right, acts as an autumn breeze to a kite. The wing literally flies on the current which it creates. It ascends at each thrust and carries the hand of the operator with it. Similar remarks are to be made of the tail of the fish. It is in this way that the *back air* and *back water* are utilised, and herein lies the excellence of the elastic reciprocating screw, as found in Nature, and as contra-distinguished from the rigid rotatory screw employed in navigation.

Mr. Garrod, adducing no proof in refutation of this and similar experiments, states "that these *induced currents* are of no real service in flight, because in their production there is as much force lost as there may be gained from their subsequent employment on the reversal of the action of the wing, if the bird's body has not advanced sufficiently far to be in each stroke beyond the range of their action, which is probably the case." On what authority does Mr. Garrod make this assertion? When a bird flies in still air, the wing of necessity must vibrate. The quicker it vibrates the more marked the reaction obtained from the air, and the greater the elevating and propelling power. The *induced currents* powerfully contribute to this reaction from the fact that the wing and the air are both moving, and moving in opposite directions. This, as explained, is a matter of experiment, and can readily be verified.

Lastly Mr. Garrod attacks my views on muscular movements. Here again he adduces no counter-proof, and, adhering to the old doctrine, contents himself by saying, "We are not ashamed to say that such has always been and still is our idea." This is not saying much. He takes exception to my statement that muscles have a centripetal or shortening power and a centrifugal or elongating power. Can he inform me how the left ventricle of the heart opens after a vigorous contraction, in which all the blood contained in the ventricular cavity is ejected and the ventricle converted into a solid muscular mass, if not by a spontaneous *elongation* of all its fibres?

Edinburgh, Jan. 27 J. BELL PETTIGREW

Specific Gravity of Sea-water

IN reference to Mr. Strachan's letter in NATURE, vol. ix. p. 183, calling attention to the discrepancy between Dr. Frankland's results and my own, permit me to state that they were not obtained from the same series of samples, and that the figures given by Dr. Frankland were, I believe, obtained by the use of a balance on shore, and also that from the way in which his specimens were packed, they were not liable to any appreciable loss by evaporation. They were not, however, taken from that part of the North Atlantic which was examined during the time that I was on board the *Porcupine* in 1869, to which alone my observations refer. My own results were obtained, as stated on p. 503 of "The Depths of the Sea," by delicate glass hydrometers, so graduated that the sp. gr. could easily be read to the fourth decimal place. Two instruments only were employed for the 105 observations made, and though they gave identical results, I had no opportunity of comparing their indications with the results obtained by a balance from the same specimen of water. I may remark here, however, that though the *absolute* results may not be quite correct, the relations between the sp. gr. of surface, intermediate, and bottom waters, pointed out on p. 505 of "The Depths of the Sea," as well as the range of variation, are probably very near the truth, since the same instruments were employed in all the determinations, and at the end of the series they indicated the same as at the commencement, when placed in a test solution, which was preserved for the purpose of detecting possible variations in the instruments themselves.

Clifton, Bristol, Jan. 17 WM. LANT CARPENTER

THE LINNEAN SOCIETY

WE regret to hear of an unpleasant event which took place at the meeting of the Linnean Society on Thursday last (5th inst.). So far as we have been able to gather the particulars they are as follows.

When the usual minutes had been read at the commencement of the meeting, a Fellow of the Society rose in his place and endeavoured to propose a motion reflecting upon the conduct of the President at the preceding meeting. The President (Mr. George Bentham, F.R.S.) ruled that the Fellow was out of order and that his motion could not be put, and requested the would-be mover of it to sit down in his place. In spite of frequent calls to order, however, this gentleman persisted in his endeavours to bring forward his grievances, and to address the meeting. At last Mr. Bentham, finding that his efforts to preserve order were vain, and that the mover of the motion (who had given no sort of notice of his intentions) was backed up by a body of clamorous friends assembled specially for the purpose, quitted the chair and left the meeting-room, followed by the Secretary and all the other members of the Council present.

As the chair of the Linnean Society can only be taken by a member of Council, the meeting thus came to a premature end, much to the disappointment of those who had assembled to hear Mr. W. K. Parker read his paper on the osteology of the woodpeckers.

We regret to have to add that, in consequence of this untoward event, Mr. Bentham has tendered his resignation as President of the Society. But we trust that the Fellows who caused the disturbance will, upon reflection, feel that however much they might have considered themselves aggrieved by the President's decision at the previous meeting, they were not justified in the course they pursued. In all meetings the decision of a chairman upon a point of order is held to be final, at all events for the occasion. More especially should this be the case in a learned society assembled for the discussion of scientific problems, and not for vulgar wranglings and disputes upon immaterial subjects.

We trust therefore that an ample apology will be offered to the President by these gentlemen, and that he will be induced to retain his chair until the approaching anniversary meeting of the Society, when he had already given notice of his intention not to accept re-nomination. The great services which Mr. Bentham has rendered to Science generally and to the Linnean Society in particular, are too well known to the readers of NATURE to render it necessary for us to descant upon them in these columns. The Linnean Society has just acquired a new and most convenient abode in the apartments at Burlington House, recently provided for it by the liberality of the country, and it would be a great misfortune if disunion should succeed in marring the work of those who are now endeavouring to make the Society still more useful and more prosperous than it has been in past times.

POLARISATION OF LIGHT*

IV.

THE phenomena exhibited by selenite are also produced by other crystals, but the facility with which plates of the former substance can be obtained, causes them to be generally used in preference to others. There is, however, a peculiar class of crystals, of which quartz, or rock crystal, is the most notable, which gives rise to effects different from those hitherto described.

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* Continued from p. 209.

If a ray of light pass through a plate of quartz which has been cut perpendicularly to the axis, or line parallel to the main planes bounding the crystal, it is as usual divided into two; but the vibrations in each ray, instead of being rectilinear and at right angles to one another, are circular and in opposite directions. That is to say, if the motion of vibration in one ray is directed like the hands of a clock, that in the other is directed in the opposite sense; and the light in each ray is then said to be circularly polarised. The motion of a series of particles of ether, which when at rest lie in a straight line, is circular, and, as in plane polarisation, successive; and consequently, at any instant during the motion such a series of particles will be arranged in a helix or corkscrew curve. The sweep of the helix will follow the same direction as that of the circular motion; and, on that account, a circularly polarised ray is spoken of as right-handed or left-handed, according to the direction of motion. A right-handed ray is one in which, to a person looking in the direction in which the light is moving, the plane of vibration appears turned in the same sense as the hands of a watch. Or, what is the same thing, to a person meeting the ray, it appears turned in the opposite sense, viz., that in which angles when measured geometrically are usually reckoned as positive.

The question, however, which mainly concerns us is the condition of the vibrations after emerging from the plate of quartz and before entering the analyser. In the passage of the ray through the plate the ether is subjected to a double circular motion, one right-handed, the other left-handed; but, as one of these motions is transmitted with greater velocity than the other, it follows that at any given point and at the same instant of time one of the revolutions will, in general, be more nearly completed than the other, or, to use an expression adopted in plane polarisation, there will be a difference of phase. The motions may be represented by two clock hands moving at the same rate in opposite directions, and the difference of phase by the angle between them when one of them is in the position from which angles are reckoned. As both are supposed to move at the same rate, they will have met in a position midway between their actual positions; and if we consider a particle of the ether (say) at the extremity of the clock-hands, it will be solicited when the hands are coincident by forces producing two opposite circular motions. Now, whatever may have been the forces or structural character within the crystal whereby this double circular motion is perpetuated, it is clear that when the ray emerges into air the particle of ether immediately contiguous to the surface of the crystal will be acted on by two sets of forces, one whereby it would be caused to follow the right-handed and the other the left-handed rotation. Each of these may, as is well known, be represented by a pair of forces, one directed towards the centre of the circle, the other in the direction of the motion and at right angles to the first, or, to use geometrical language, one along the radius and towards the centre, the other along the tangent and in the direction of the motion. The two forces acting along the tangent being in opposite directions will neutralise one another, and the resultant of the whole will, therefore, be a force in the direction of the centre. The particle in question, and consequently all those which following in succession serve to compose the entire ray until it enters the analyser, will vibrate in the direction of the diameter drawn through the point under consideration; or, to express it otherwise, the ray will be plane-polarised, and the plane of vibration will be inclined to the plane from which angles are measured by an angle equal to half the difference of phase on emergence due to the thickness of the crystal. The retardation being the same absolute quantity for all rays, will, as in the case of plane polarisation, be a different fraction of the wave-length for rays of different colours, and will be greater for the shorter waves than for the longer. Hence the planes of vibration of the different coloured rays, after emerging from the quartz, will be differently inclined. Each ray will therefore enter the analyser in a condition of plane polarisation; and if the analyser be turned round, it will cross the vibrations of the various coloured rays in succession, and extinguish each of them in turn. Each of the images will consequently exhibit a gradual change of colour while the analyser is being turned; and the tints will be, as explained before, complementary to those which are successively extinguished. For a given plate of quartz the order of the tints will be reversed when the direction of rotation of the analyser is reversed. But it should be here explained that there are two kinds of quartz, one called right-handed and the other left; and that, for a given direction of rotation of the analyser, these cause the colours to follow one another in opposite orders. A similar effect is produced by turning the polariser round in the opposite direction.

The angle of rotation of the plane of vibration for any particular colour varies, as stated above, with the thickness of the plate; while for a given thickness it increases nearly as the square (product of the quantity into itself) of the wave-length decreases. In mathematical language it varies approximately inversely as the square of the wave-length. If this law were accurately true, the product of the angles of rotation into the square of the corresponding wave-lengths (λ) would be the same for all rays. The following are some measurements made by Brock, with a quartz plate one millimetre thick, which show that the law may be considered as true for a first approximation.

Rays	Rotations	Rotations × λ^2.
B	15° 18	7,238
C	17° 15′	7,429
D	21° 40′	7,511
E	27° 28′	7,596
F	32° 30′	7,622
G	42° 12′	7,842

If the colours exhibited by a plate of quartz when submitted to polarised light be examined by a spectroscope, in the way described when we were speaking of selenite, the spectrum will be found to be traversed by one or more dark bands, whose position and number depend upon the thickness of the plate. But there will be this difference between plane and circular polarised light, that if the analyser be turned round, the bands will never disappear, but will be seen to move along the spectrum in one direction or the other, according as the plate of quartz be right-handed or left-handed, and according to the direction in which the analyser is turned. This is, in fact, identical with the statement made before, that the analyser in its different positions successively crosses the plane of vibration of each ray in turn, and extinguishes it.

This being so, it is clear that a change of colour exhibited by a quartz plate when submitted to plane-polarised light and examined with an analyser, forms a test of a change in the plane of original polarisation. And if the plate be composed of two parts, one of right-handed, the other of left-handed quartz, placed side by side, any change in the plane of polarisation will affect the two parts in opposite ways. In one part the colours will change from red to violet, in the other from violet to red. At two positions of the polariser, or analyser, the colours must be identical. With plates, as usually cut, one of these identities will be in the yellow, the other at the abrupt passage from violet to red, or *vice versâ*. In this case the field appears of a neutral tint, *teinte sensible* or *teinte de passage*, as the French call it, and the slightest change in the plane of polarisation exhibits a marked distinction of colour, one part verging rapidly to red, the other to violet. This arrangement is called a biquartz, and affords a very delicate test for determining the position, or change of position, of the plane of polarisation, especially in cases where feebleness of light or other circumstance interfere with the employment of prismatic analysis.

If the thickness of the plate be such that the difference of rotation of the planes of vibration of the rays corresponding to the two ends of the visible spectrum (or, as it is sometimes termed, the "arc of dispersion") be less than 180°, there will be one dark band in the spectrum; because there can then be only one plane of vibration at a time at right angles to that of the analyser. If the arc of dispersion is greater than 180° and less than 360°, there will be two bands. And so on for every 180° of dispersion.

This mode of examination by means of prismatic analysis is the most accurate yet devised for measuring the angle of rotation produced by circular polarisation; especially if solar light be employed, and the fixed lines used to form a scale of measurement.

The property of circular polarisation is, however, not confined to quartz. Among solids, chloride of sodium is the only other known instance, but among fluids and fluid solutions there are not a few.

The following list is given by Verdet. The angles have reference to the red rays given by a plate of glass coloured with oxide of copper, and are affected with the sign + in the case of right-handed, and with − in the case of left-handed rotation. The length of the column of the solution is in every case one decimetre.

Essence of turpentine . . .	−29°·6
,, lemon . . .	+55°·3
,, bergamot . . .	+19°·08
,, bigarade . . .	+78°·94
,, aniseed . . .	− 0°·70
,, fennel . . .	+13°·16
,, carroway . . .	+65°·79
,, lavender . . .	+ 2°·02
,, peppermint . . .	+16°·14
,, rosemary . . .	+ 2°·29
,, marjoram . . .	+11°·84
,, sassafras . . .	+ 3°·29
Solution of sugar 50 per cent. .	+33°·64
,, quinine 6 per cent. in alcohol .	−30°.

It will be noticed that the rotary power of all these substances is much less than that of quartz.

A mixture of liquids, one or both of which is active, generally exhibits a rotatory action represented by the sum or difference of their separate powers (a neutral liquid being considered to have a power represented by 0); but this law is true only when no chemical action takes place between the elements of the mixture. Saccharine solutions vary not only in the amount but also in the character of their power of rotation; thus cane sugar is right-handed, but grape sugar left-handed.

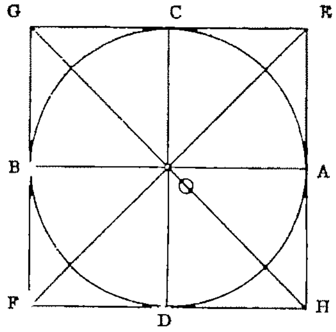
The property in question has been turned to practical use by employing the rotatory power of a saccharine solution as a measure of the strength of the solution. For this purpose a tube containing the solution to be examined is placed between two Nicol's prisms. The simple fact of circular polarisation is proved by a feeble exhibition of the phenomena shown by a plate of quartz cut perpendicularly to the axis. But for accurate measurement various expedients have been adopted. If a biquartz be inserted behind the analyser (the end of the apparatus next the eye being considered the front), then for a certain position of the analyser the two halves will appear of the same colour. When the tube for examination is inserted the similarity of colour will be disturbed; and the angle through which, right or left, the analyser must be turned in order to restore it will be a measure of the rotary power of the fluid.

Another method is as follows:—Use a single quartz instead of a biquartz; in front of it place a pair of quartz wedges, with the thin end of one opposite the thick end of the other; the outer surfaces having been cut perpendicularly to the axis. If the plate be right-handed, the wedges must be left-handed, and *vice versâ*. The wedges must be made to slide one over another so as together to form a plate of any required thickness, and a scale connected with the sliding gear registers the thickness of the plate produced. When the tube is removed the wedges are adjusted so as to compensate the quartz plate, and their position is considered as the zero point of the scale. When the tube is replaced, the wedges are again adjusted so as to compensate the action of the fluid in the tube, and the difference of the readings gives the thickness of quartz necessary for the compensation. The rotatory effect of a given thickness of quartz being supposed known we know at once the effect of a thickness of the fluid under examination equal to the length of the tube.

Another method has been based upon the principle of Savarts bands; but sufficient has perhaps here been said to illustrate the principle of the saccharometer.

Circular polarisation may, however, be also produced by other means, namely, by total reflexion, and by transmission through doubly-refracting plates of suitable thickness.

It will perhaps be best to begin with the last. And in order the better to understand the process we must consider briefly the result of compounding two rectilinear vibrations under different circumstances.



Suppose a particle of ether to be disturbed from its point of rest O in a direction O A. The attraction of the particles in its neighbourhood would tend to draw it back to O; and let O A be the extreme distance to which under these attractions it would move. Having reached A it would return to O, and passing through O with a velocity equal to that with which it started under the disturbing force, it would move to a point B equidistant from O with A, but in the opposite direction. And if, as is generally supposed, the ether is perfectly elastic, or that there are no internal frictions or other conditions whereby the energy of motion is converted into other forms of energy, the oscillations or vibrations of the particle between the points A and B will continue indefinitely. Now suppose that while these vibrations are going on, a second disturbing impulse, equal in intensity, but in a direction at right angles to the first, be communicated to the particle, it is clear that the effect on the motion of the particle will be different according as it takes place at the point of greatest velocity O, or at that of no velocity A or B, or at some intermediate point. Our object is to consider the effects under these various circumstances.

A complete vibration consists in the motion from O to A, thence to B, and finally back to O; so that if O be the starting point the passage through A will be removed one-fourth, the passage through O from A towards B will be one-half, the passage through B will be three-fourths, and the passage through O from B to A a complete vibration from the commencement. This being so, suppose that the second impulse be communicated while the particle is at O on its way towards A, then the impulses may be considered as simultaneous and the vibrations to which they give rise will commence together, and the waves of which they form part will be coincident. If the second impulse take place when the particle is at A, the two sets of vibrations or waves to which they belong will have a difference of phase (*i.e.* the first will be in advance of the second) equal to one-fourth of a vibration or one-fourth of a wave-length. If the second impulse take place when the particle is at O on its way to B, the difference of phase will be half; if when it is at B the difference will be three-fourths of a wave-length.

The particle being at O, and subject to two simultaneous impulses of equal strength, one in the direction of A, the other in that of C, must move as much in the direction of C as in that of A, that is, it must move in a straight line equally inclined to both, namely O E in the same figure. And inasmuch as the two impulses in no way impede one another, the particle will move in each direction as far as it would have done if the other had not taken place. In other words, if we draw a square about O with its sides at distance equal to O A or O B, the extent of the vibration will be represented by O E where E is a corner of the square. The complete vibration will then be represented by the diagonal E F in the same way as it was by the line A B in the first instance. If the impulse had been communicated at the instant of passage through O on the way to B, it is clear that a similar train of reasoning would have shown that the vibration would have been in the other diagonal G H. We conclude, therefore, that if two sets of rectilinear vibrations, or plane waves, at right angles to one another combine, then when they are coincident they will produce a rectilinear vibration, or wave, whose plane is equally inclined to the two, and lying in the direction towards which the motions are simultaneously directed. In the figure this is represented by the dexter diagonal. When the two sets of waves have a difference of phase equal to half a wave length, their combination gives rise to a wave represented in the figure by the sinister diagonal.

Suppose now that the second impulse is communicated at the instant when the particle is at A; in other words, that the two sets of waves have a difference of phase equal to one-fourth of a wave-length. At that instant the particle will have no velocity in the direction of A B (for convenience, say eastwards), and will consequently begin to move in the direction of the second impulse, say northwards. But as time goes on the particle will have an increasing velocity westwards and a diminishing velocity northwards, it will therefore move in a curve which gradually and uniformly bends, until when it has reached its greatest distance northwards it will be moving wholly westwards. And as the motion not only will be the same in each quadrant, but would be the same even if the directions of the impulses were reversed, it is clear that the curvature of the path will be the same throughout, that is to say, if two sets of waves of the same magnitude in planes perpendicular to one another, and with a difference of phase equal to one-fourth of a wave-length combine, they will produce a wave with circular vibrations.

If the second impulse be given when the particle arrives at B, that is, if the waves have a difference of phase equal to three-fourths of a wave-length, similar considerations will show that the motion will be circular, but in the opposite direction.

Suppose, therefore, that we allow plane-polarised light to fall upon a plate of doubly refracting crystal cut perpendicularly to the axis in the case of a uniaxal crystal, or in the case of a biaxal to the plane containing the two axes, say a plate of mica which splits easily in that direction; then the vibrations will, as before explained, be resolved in two directions, at right angles to one another. And further, if the original directions of vibration be equally inclined to the new directions, *i.e.*, if it be inclined at 45° to them, the amount or extent of vibration resolved in each direction will be equal. Further, if the thickness of the plate be such as to produce retardation or difference of phase equal to a quarter of a wave, or an odd number of quarter wave-lengths, for the particular ray under consideration; then the two sets of vibrations on emerging from the mica plate will recombine, and, in accordance with the reasoning given above, they will form a circular vibration, left-handed or right-handed according as the retardation amounts to an integral number of three-quarter wave-lengths or not.

It thus appears that a plate of mica which retards one of the sets of waves into which it divides an incident set by an odd multiple of quarter-wave lengths, affords a means of producing circular from plane polarisation. It remains to be shown that, with the same plate in different positions, right or left handed circular polarisation may be produced at pleasure. Suppose that the original vibrations are in the direction E F in the foregoing figure; the mica plate will resolve them into the two directions A B, C D, one of the rays, say the first, will be transmitted with greater velocity than the other, and the vibrations along C D will be one-fourth of a wave-length behind those along A B. This will correspond to the case discussed above, and will give rise to a circular vibration in a direction opposite to that of the hands of a clock. Suppose, however, that the plate be turned round through a right angle, so that the vibrations which are transmitted with greater velocity are placed parallel to C D, and those which are transmitted with lesser along A B. The ray whose vibrations are along A B will then be a quarter wave-length in advance, or, what comes to the same thing, they are three-quarters of a wave-length in rear of the others; and this condition of things produces, as explained before, a circular vibration in a direction the reverse of the former. It thus appears that the plate placed in one direction will convert plane into right-handed circular polarisation; and if turned round through a right angle from that position will convert plane into left-handed circular polarisation. A like change from right-handed to left-handed circular polarisation, or *vice-versâ*, may obviously be effected by turning the original plane of polarisation through a right angle; so that it shall lie between lines of concurrent instead of between lines of discordant motion.

W. SPOTTISWOODE

(*To be continued.*)

A COMPLETE SPECIMEN OF A PALÆOTHERIUM

FROM *La Nature* we learn that the palæontological collection of the Museum of Natural History of Paris has just been enriched by the addition of a new specimen of very great scientific interest, which is the entire skeleton of *Palæotherium magnum*, imbedded in a large block of gypsum and marl, the whole being exhibited in the anatomical department of the museum.

The *Palæotherium magnum*, whose name alone indicates its ancient existence, was first recorded by the great French naturalist Cuvier, in his celebrated "Recherches sur les Ossemens Fossiles." It is an animal which is entirely extinct, without any present representative. Individuals of the species must have been extremely abundant during the period that it existed. Modern zoologists place it among the Perissodactylates, that is to say, with the at present existing rhinoceros, tapir, and horse. It forms part of the fauna which is found abundantly embedded in the deposits of gypsum. All palæontological collections, even the most humble, have for a long time been provided with the remains, or more or less complete portions of this fossil form, but none have yet had the good fortune to obtain a complete skeleton.

The principal result of the examination of the new specimen which we are describing has been to show that until now very inexact notions have been entertained as to what this animal truly was when the proportions and general contour of the tapir were assigned to it, as was done even by Cuvier himself.

Far from being bulky and almost massive, as was thought, *Palæotherium magnum* is now evidently seen to be a very slender animal, with an extremely graceful carriage, with the neck longer than in the horse, and a general contour much on the same type as that of the Llama.

Without attempting a detailed study of its osteological structure, we may mention that *Palæotherium magnum* had a height a little less than that of a middle-sized horse Three toes are found on each of the feet; the head, much like that of a tapir, had most probably also the rudiment of a trunk; the femur has a third trochanter; the dentary system is composed, in each jaw, of six incisors, two canines, and fourteen molars, these latter corresponding with the same teeth in the rhinoceros.

Palæotherium magnum, like its congeners, of which about a dozen species are at present known, was herbivorous, and without doubt lived in large herds. Its existence carries us back to that age of our earth which is termed the Eocene period, and it is in the middle of that period, which comprises the gypsum deposits or their geological equivalents, that its remains are discovered, as well as those of all the other species of the same genus.

PALÆOTHERIUM MAGNUM

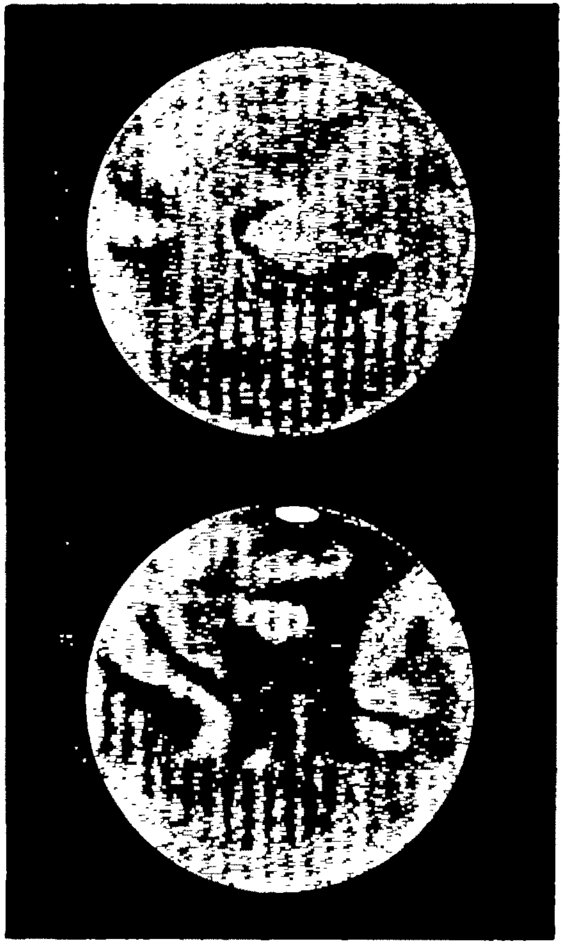
Nevertheless it made its appearance even before the gypsum formation, its presence having been detected in the beds of coarse limestone, which are inferior to and therefore more ancient than that formation.

It is the plaster quarries of Montmartre, Pantin, and La Villette, near Paris, which have for a long time held the privilege of furnishing to palæontologists the numerous remains that are known of this fossil species. The Palæotherium, which forms the subject of this notice, was obtained from a plaster-quarry situated at Vitry-sur-Seine. It was, however, even a few days ago, as we see it to-day, exposed on one side, and on the other encrusted in its stony resting-place in the ceiling of a subterraneous gallery, a little more than four yards high. Only a few have visited it, although M. Fuchs, a civil engineer, the proprietor of the quarry where this magnificent specimen was found, offered to give it to the Museum.

The gift so generously offered was immediately accepted; and Prof. Gervais, with a scientific zeal which ought to be fully acknowledged, occupied himself with the direction of the important task of taking it intact to Paris.

MARS

THE characteristic appearance of this planetary body, long familiar to astronomers, has of late become generally known. Remarkable neither for situation, magnitude, brilliancy, retinue or complexity of arrangement, inferior in each of these respects to some, and in many of them to several of the members of the solar family, one circumstance alone invests it with a peculiar interest—its resemblance to ourselves. Such a resemblance obviously does not exist in the mightier and more nobly attended external planets: the banded skies of two and the strong atmospheric absorption of the two others revealed by the spectroscope, sufficiently show that they belong to classes mutually indeed dissimilar, but each differing, and perhaps widely, from our own. With the swift and fiery Mercury we can have as little sympathy; and though Venus would offer a more promising analogy, the configuration of her beautiful surface is not well seen or readily interpreted. Mars therefore remains; and while, fortunately for astronomers, he occupies such a position that his features are fairly accessible, they bear an aspect so comparatively intelligible that, whatever may be the case as to our other fellow-subjects in the solar monarchy, we are ready to claim that globe as a close relation of our own, inferior indeed in magnitude and importance, if importance is indicated by an attendant, but arranged in a corresponding manner by the Great Creator as the seat of life and intelligence.



MARS IN 1862

Such a supposition has been gradually and surely advancing from an early period of telescopic astronomy. The polar whiteness detected by Huygens and Cassini I. as far back as 1672 would naturally suggest the idea of a snowy deposit, which assumed almost the form of certainty, when the elder Herschel showed that its extent was regulated by the Martial seasons, and that it wasted steadily down with the advance of vernal heat. From the obvious division of the surface into brighter and darker portions, the existence of an atmosphere at least would be inferred, so long as they were supposed to be variable; but as the evidence of their general permanence increased under the eye of Herschel I. about a century ago, this impression gave place to the more definite recognition of something corresponding to the outlines of lands and oceans, with occasional variation from atmospheric condensations; and thus by degrees we have been led to acknowledge, in that remote and otherwise unimportant globe, a most interesting counterpart of our own.

This conclusion has not, however, been attained by an uninterruptedly continuous or an uniformly satisfactory process of deduction; and even at the present time it is perhaps not universally received. Schröter referred the darker portions to atmospheric obscuration, a notion which pervaded others of his investigations, not to their advantage; and a more recent observer of considerable ability, the late Prof. Kaiser, of Leiden, whose decease in his 64th year took place July 28, 1872, has, in a very elaborate and interesting report of the work done on the planet at that observatory, expressed his doubts as to the certainty of the more customary inference. Whatever may be our own impressions on the subject, his criticisms and conclusions exhibit so much of the genuine spirit of an impartial student that some notice of them, as they are found in vol. iii. of the Annals of the Leiden Observatory, may be worth the attention of our readers. This observatory, it should be noted, is provided with a Merz achromatic of 7 (French ?) inches aperture, and was therefore, under Kaiser's superintendence, fairly competent for physical researches commensurate with the present demands of science; as it is well known, and indeed especially brought out by the observations we are about to notice, that much larger telescopes are not invariably, or even generally, available in proportion to their magnitude. The addition, in 1872—too late therefore for a share in the professor's observations—of an 8½ inch With-Browning reflector, will hereafter not only afford an interesting comparison of instruments, but if the result corresponds with others obtained elsewhere, will be found a step in advance as regards efficiency.*

In selecting Mars as the subject of special inquiry, Prof. Kaiser laid a solid foundation by consulting every work within his reach, representing or describing the physical aspect of the planet, from the earliest and rudest efforts in 1636 to the elaborate delineations of the present day. No less than 412 drawings thus passed through his hands: upwards of 320 others he could not procure; and the aggregate is doubtless much in defect of the existing total. He did however well in securing so many; more, probably, than any other areographer, if such a word may be allowed. But the result of their comparison and discussion was not as satisfactory as might be wished. The first specimens of representation were of course mere rude attempts. Those of Huygens, however, in 1659, discovered by Kaiser in his "day-book" (of which the most valuable portion was edited by him in 1847) are comparatively well drawn; and Hook, in 1666, caught the true character of what he saw, though Kaiser doubts whether his spots could be as readily identified as has been supposed. We next find Herschel I. taking up the subject in 1777, and continuing his observations till 1783. He first noticed the eccentric position of the two white spots in the polar regions, as well as their diminution from solar action; 5 out of his 31 figures show a broad white band passing obliquely across the disc, and he speaks of changes in the markings from passing clouds and vapours: some of his dark spots can be identified with more recent representations, but not the whole. Of the numerous drawings (217) of Schröter, Kaiser was unable to avail himself, as the Areographische Fragmente, rescued from the disastrous fire at Lillenthal in 1813, were left unpublished at his death. These, however, through the intervention of Dr. Peters of Altona, have subsequently been traced to their safe custody in the hands of Schroter's descendants, and have recently been thoroughly examined by Dr. Terby of Louvain, whose report has been published by the Belgian Académie Royale des Sciences. From the figures contained in this, and another interesting essay by the same astronomer, it appears that many coincidences may be traced between the views of Schröter and other observers, though his preconceived idea of the vaporous nature of the darker features deprived his observations of some of the value otherwise due to them as the results of eminent zeal and perseverance.

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* A curious error on the part of Prof. Kaiser may here be noticed. He has referred (p 23) to a drawing of Mars by Browning as having been taken with a silvered mirror by Barnes. This gentleman was merely the proprietor of the speculum, which, like the others mounted by that optician, was the work of a most accomplished artist, Mr. With, of Hereford.

Passing by several observers of minor note, of whom Kaiser has given a minute enumeration, we reach the opposition of 1830, which the near concurrence of the aphelion of the Earth and the perihelion of Mars rendered eminently favourable, enlarging the apparent diameter of the latter to $23''\cdot1$. The close and systematic investigation then entered upon by Beer and Mädler forms a most important epoch in the progress of areography, and for the first time a series of drawings were executed, little resembling anything previously known, which have ever since been referred to with confidence as a starting point for future inquiries, and which, it might have been hoped, would have set many questions at rest: and so they did; but as Kaiser remarks, Later representations have again unsettled points which had been supposed to have been then decided. The comparative failure of the same observers in subsequent oppositions admitted of explanation from the increased distance and altered presentations of the planet; and little advance was made by Mädler in 1841, even with the renowned refractor at Dorpat, to the care of which he had succeeded: the apparent diameter, then, however, was only $15''\cdot1$, its minimum, attained in 1837, being $13''\cdot3$. Of the near approach in 1845, when the disc was enlarged to $23''\cdot5$, no observations seem to have fallen into Kaiser's hands, excepting those of the American astronomer Mitchell. Confining ourselves still to the more important representations, at the expense of doing scanty justice to the Professor's elaborate memoirs, we find that 1854 produced the beautiful designs of Jacob, and 1856 the still more delicate ones of De La Rue, both great advances on anything previously published. In 1858, Secchi, who had been for some years at work at Rome, brought out a series of drawings in much harmony with themselves, but only partial agreement with previous delineations. At length came the favourable opposition of 1862, when the point was taken up by many of the first observers armed with some of the most powerful telescopes in existence; but the result, we regret to add, was very different from what might have been anticipated. Secchi, with his magnificent achromatic, Lassell and Rosse with their colossal reflectors, produced such an unsatisfactory and in part contradictory set of drawings as had never been published before. The weather was not always in fault; and though Mars was rather low, Lassell repeatedly found very sharp definition; Lord Rosse's excellent draughtsman once used a power of 1,200; and the quality of Secchi's instrument and sky compensated to a great extent for his smaller aperture; but then the expected clearing up of difficulties terminated in the annoyance of disappointment. Kaiser assures us that nowhere are there such discrepancies as between Rosse and Lassell, even when the same hemisphere was obviously in sight; it could hardly be imagined that they had the same spot in view: and Secchi is so far from setting the matter straight, that his figures scarcely seem to refer to the same body; and for any purpose of accurate deduction the Leiden Professor felt obliged to put all three aside. He is even induced to say, "the largest telescopes give the worst results, and show themselves very liable to mislead the observer: correct delineations of the celestial bodies require before all things a very practical designer that gives way to no fancies; and such a designer is not apt to possess the most powerful telescopes of the earth." We are here merely reproducing the censor's words, without venturing an opinion as to the soundness of his criticism.

But, fortunately as it would seem for areography, instruments of more moderate dimensions were employed to better purpose during that and the subsequent opposition of 1864; and the agreement of the beautiful designs of Lockyer in the former year with those taken by Kaiser himself, then and in 1864, as well as with those of Schmidt and Phillips, was far more satisfactory: and the same might be said to a considerable extent as to Dawes, whose instrument, however, was of a superior rank.* The difference between Lockyer and Lassell, in one instance, was so wide, that identity of date alone proves that they had the same hemisphere under their eyes; while, on the contrary, the concurrence between Lockyer and Kaiser, though the latter speaks with great diffidence of his own designs, justified him in believing that a pretty correct representation had been attained of a broad girdle round the equator.

The labour which the Leiden professor has bestowed upon a comparison of all the least discordant drawings, and the punctilious accuracy of his protracted discussions, would be little appreciated from so brief a sketch of them as can be attempted here. He was himself so little pleased with the result as to express an opinion that the only safe inference from the oppositions of 1862 and 1864 is, that the art of drawing celestial objects is at much too low a pitch to justify accurate deductions as to their physical character. And this, though it looks like the language of disappointment, and is hardly reconcileable with the striking agreement which he often remarks between the drawings of different observers, or the same observer at different times, seems to have been his deliberate impression. He ascribes the variations in part to the differences of presentation and perspective, in part to faulty delineation; and while he admits that atmospheric condensations may have occasioned small apparent changes, he thinks on the whole that they are evidences of the unsteadiness of our air, leaving almost always an uncertainty as to the minuter spots and shadings, and a want of confidence in the correctness of one's own delineation. The discrepancies among his 412 designs are so "enormous" that no one would believe that they were intended for the same body. These differences arose, however, in part from natural causes. Even in the most favourable case, the spots are only seen with any distinctness or in their true form in the centre of the disc; those lying near the limb being greatly foreshortened and not recognisable in their real shape; and this difficulty is very considerably enhanced by the imperfect transparency of the planet's atmosphere and its frequently strongly illuminated precipitations. The inclination of the axis may vary its position at different times about 60° as regards the spectator, and consequently different oppositions bring before him entirely different features in that central position where alone they can be well observed or drawn.

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* It is much to be regretted that only a part of the drawings of this great observer have as yet been published in *fac-simile*. Those given in the Monthly Notices (XXV, 223) omit, as Kaiser remarks, some of the most curious presentations of the globe; and the reproduction of others by Proctor does not profess minute accuracy.

And to these sources of difficulty it might, we think, have been added that, in consequence probably of our study of geography from projections of the globe in which the effect of perspective is designedly counteracted as much as possible, we are apt to have a very defective idea of the amount of apparent distortion which it occasions towards the edges of the visible hemisphere. But even when all this has been allowed for, we find, the professor tells us —nor indeed do we need opportunities as extensive as his to convince ourselves of it—that the differences are much too great to be altogether thus explained; and he concludes that the more conspicuous ones are errors in representation. If there is occasional agreement as to the forms, there is still much risk in referring them to the same object, until it has been ascertained by computation that the presentation of the globe towards the spectators was nearly the same. Fortunately, during any given opposition, the position of the planet's axis shifts but little, and in other oppositions the same presentation recurs from time to time; but instead of the correspondence naturally anticipated, the differences are for the most part, as he expresses it, "enormous." And yet amongst them all, coincidences come to the surface, too remarkable to admit the idea of fortuitous resemblance; and we must suppose that many who have taken pencil in hand have not been sufficiently careful as to form and shading, but have followed arbitrary and perhaps very mistaken impressions, from which nothing but absurd and absolutely contradictory inferences of a physical nature could be drawn.

T. W. WEBB

(*To be continued.*)

THE ADMIRALTY CHARTS OF THE PACIFIC, ATLANTIC, AND INDIAN OCEANS *

THESE charts have been compiled by Captains Evans and Hull, of the Hydrographic Departments of the Admiralty, from Maury's pilot charts, Fitzroy's and Fergusson's wind charts, charts issued by foreign Governments, and from the works of Dovè, Neumayer, Buchan, and documents in the Hydrographic Office of the Admiralty. They show for the four seasons the pressure, winds, and temperature over the parts of the globe covered by the sea. January, February, and March are properly grouped together into one season, these being the three coldest months as regards the oceans in the northern, and the three warmest in the southern hemisphere.

The most important piece of new work in these charts is the "isobars," or lines of equal barometrical pressure, which are given for the seasons. These isobars for the sea, taken in connection with Buchan's isobars published in 1868, may be regarded as the first approximation to a complete representation of the earth's atmosphere over both land and sea. We have minutely examined these isobars, comparing them with the large amount of new information collected during the past five years from many places situated on the coasts of the continents, or in islands scattered over the ocean, and can come to only one conclusion, viz., that the greatest care has been taken in their construction. Among the very few cases to which slight exception might be taken is the isobar of 29·7 in. of July, August, and September, drawn to southwards of Japan, which observations do not appear to warrant. It should also be pointed out that a serious omission has been made in not stating how the ship barometric observations were reduced to the mean pressures from which the isobars have been drawn.

We are now in a position to draw one or two general conclusions of great importance regarding the distribution of atmospheric pressure over the ocean. In the ocean, to westwards of each of the continents, there is at all seasons an area, or patch, of high pressure, from 0·10 inch to 0·30 inch higher than is found on the coast westward of which it lies. The distance of the centre of the space of high pressure from the coast varies from 20° to 35° of longitude, the average distance being nearly 30°. The position of the centre of the space varies from about 22° to 35° north or south latitude, or stating it roughly it lies about the zones of the tropics. In these spaces the absolute pressure is greatest during the winter months of the respective hemispheres—a condition of things probably due to the fact that during the winter season of the northern hemisphere the great mass of the earth's atmosphere is disposed about the tropic of Cancer, and during the winter season of the southern hemisphere, about the tropic of Capricorn. The position and shape of the isobars seem to be largely determined by that of the continents adjoining. Thus the rounded form of the southern portion of North America, the bending eastward of the west coast of South America from Payta to Arica, and the form of the north-western part of Africa and its "lie" from S.W. to N.E. are all more or less impressed on the isobars bounding the contiguous spaces of high pressures. These spaces are less prominently marked west of those continents which have the least breadth in lat. 30°; thus the area of high pressure is less marked west of the Cape than it is west of Australia, and still less than to the west of North America. The isobars are much farther apart on the western than on the eastern side of these areas of high pressure; indeed in many cases they are as it were drawn out so as almost to reach the continent lying to westward; and in some cases there is even a tendency towards, or the actual appearance of, secondary areas of high pressure to eastwards of continental masses. This is most distinctly seen to eastward of Australia.

We have dwelt thus particularly on these spaces of high pressures because of their importance in atmospheric physics, but more especially because of their vital connection with prevailing winds and the general circulation of the atmosphere. Out of these high pressures, the wind blows in all directions anti-cyclonically in accordance with the well-known "Buys Ballot's Law of the Winds," of which relation the wind charts before us afford abundant confirmation. Keeping this relation between wind and pressure in view, we have presented in these high pressures the proximate causes of the prevailing winds over the greater portion of the ocean; and through the prevailing winds, the drift currents and other of the surface-currents of the sea; and thereby the anomalous distribution of the temperature of the sea as seen in the Chile, Guinea, and other currents, and the peculiar climates of the coasts past which these currents flow.

The small area of high pressure to the east of Australia may be singled out as perhaps the most interesting of the new facts in the charts. During winter the winds along the east and south of Australia blow inwards upon the interior of that continent, whereas in New Zealand the prevailing winds at the same season are north-westerly and westerly, the directions being thus generally opposite on these two coasts facing each other. The space of high pressure between gives a ready explanation of the direction of these winds, as well as of the heavier rainfall on the west of the South Island of New Zealand as compared with that of the North Island, and of the south-east as compared with the south-west of Victoria.

Like praise cannot be given to the charts of the isothermals of air for January, April, July, and October. In the October chart, the isothermal of *60° cuts the* east coast of South America near lat. 27°; now at Monte Video, the mean temperature of October is 61·2°, at Buenos Ayres 61·3°, and at Bahia Blanca, in 38°·4 S. lat. 59°·7; that is, the isothermal of 60° should cut the South American coast 11° of latitude farther to the south. The January isothermal of 60° is drawn passing through New

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* "Wind and Current Charts for the Pacific, Atlantic, and Indian Oceans," published at the Admiralty, October 1872, under the superintendence of Rear-Admiral G H Richards, C.B., F.R.S , Hydrographer

Zealand near lat. 40°, and the isothermal of 50° near Dunedin; now the mean temperature of January at Southland situated at the extreme south of New Zealand is 57°·6, and at Dunedin (550 ft. high) 57°·5; in other words, the isothermal of 60° and not that of 50° ought to pass near Dunedin. Dr. Hector's meteorological reports during the past seven years place this beyond all doubt, and it is unfortunate that the summer climate of this important colony of Great Britain should have been so misrepresented as to appear to be colder than that of Iceland, and altogether insufficient for the ripening of wheat, barley, and other cereals. The July isothermal of 90° is represented as having its eastern extension at the entrance to the Persian Gulf in 57° E. long. Now Murray Thomson's and Blandford's meteorological reports show that the isothermal of 90° extends eastward to about 77° E. long., so as to embrace the Punjaub and the upper tributaries of the Ganges to the west, being thus 20° farther east than is represented on the chart.

The truth is, that, excepting for the months of January and July, there have been no isothermal charts of the months for the whole globe yet published which do not contain many gross errors similar to those we have pointed out. The time is surely not far off when a committee of the British Association, or some competent authority, will take up this subject, and give us a set of new isothermal lines laid down from all data which the great expansion meteorology has received of late years has made available.

The two charts showing the isothermals of the sea for the extreme months, February and August, and the chart showing the surface currents of the ocean, are very valuable. A supplementary chart showing the currents south and east of Asia during the monsoon season is also given. We should suggest for the second edition of the Charts, that charts of the surface currents for both February and August should be given for the whole globe, it being only thus that these important aids to navigation can be adequately presented.

It was pointed out in NATURE some years ago that the prevailing winds and surface currents of the Atlantic are all but absolutely coincident. These Charts enable us now to extend the remark to the prevailing winds and surface currents over all the oceans. Keeping out of view the deep-water currents of the sea to which Carpenter has given so much attention, it is now placed beyond all doubt that it is to the winds we must look as the prime movers of oceanic currents.

MR. GARROD'S NEW CLASSIFICATION OF BIRDS

AT the scientific meeting of the Zoological Society, on Tuesday, February 3, Mr. A. H. Garrod introduced a new Classification of Birds, based mainly on the disposition of their muscles and other soft parts. The following is an abstract of his paper:—

The osteology of birds, judging from the unsatisfactory state of their classification in the present day, is not sufficient in itself as a basis for distinguishing the mutual relations of the different families and genera; and as the peculiarities in the soft parts are very constant, they deserve more consideration than they have hitherto received. The researches of Hunter, Nitzsch, Macgillivray, Owen, and others, have brought to light many facts in visceral anatomy and pterylosis, all of which are of great value in classification. Sundevall is the only ornithologist who seems to have made any generalisations respecting myology, and these have an important bearing on the subject.

My method of work, Mr. Garrod went on to say, has been the following:—After having carefully dissected a few birds that are known to be but distantly related, a comparison of the notes on the individuals examined showed that there were important myological differences between them. Further dissection of species related more or less intimately, indicated broadly the relative value of the peculiarities that were found, when taken in connection with the most approved classification of the present day; and as observations became more numerous the relative importance of the facts observed was more easy to estimate. The muscles which have, on account of their marked tendency to vary in the class Aves, attracted the most of my attention, are all situated in the thigh, and they are five in number: (1) the *femoro-caudal*, which runs from the linea aspera of the femur, near its head, to the sides of the tail vertebræ; (2) the *accessory femoro-caudal*, which runs parallel to the last, and behind it, from below the femur-head to the ischium; (3) the *semitendinosus*, which crosses the first-named muscle superficially, and arises from the lower part of the ischium, to be inserted into the inner side of the tibia-head; (4) the *accessory semitendinosus*, which arises from the distal end of the linea aspera, and joins the fibres of its larger namesake obliquely just before their insertion; (5) the *ambiens*, that peculiar slender muscle which arises from just above the acetabulum, and after running obliquely through the ligamentum patellæ, joins the tendon of the flexor perforatus digitorum. My observations on these five muscles have been made on more than 500 species of birds, including more than 600 specimens, and the results are recorded in a tabular form, in a paper now in course of publication in this Society's Proceedings. For the present, no more attention need be paid to these muscles themselves, but only their presence or absence considered; therefore, to simplify description, a myological formula will be employed which indicates all the facts required in a very precise manner. Calling the first four of the above-mentioned muscles, A B X and Y, respectively, and omitting from the formula thus based, the symbol or symbols which represent any that are deficient, it is clear that a bird, like the common fowl for example, which possesses them all, would be represented by ABXY; and the eagle, in which the femoro-caudal is alone present, by A; whilst the sparrow, which only wants the accessory femoro-caudal, must have the formula A X Y; and the duck, which only lacks the accessory semitendinosus, is represented by A B X. By this means it is possible to make important statements respecting the myology of any bird in a very concise form, which gives great facility towards the comparison of different species. It must here be mentioned that individuals of a species and species of a genus do not vary among themselves in the muscles under consideration. The following table gives the myological formula of the different families of birds, as far as my dissections enable me to go, the only important types omitted being Eurypyga, Psophia, Todus, and Bucco. They are arranged in an order to be subsequently explained, and the presence or absence of the ambiens-muscle is indicated by + or — after each formula:—

TABLE I.

Struthionidæ B X Y +	I.
Casuariidæ { A B X Y —	
B X Y —	*Picidæ* { *A X Y* —
Tinamidæ A B X Y +	*A X* —
Palamedeidæ A B X Y +	*Ramphastidæ A X Y* —
Gallinæ { A B X Y +	*Capitonidæ A X Y* —
(excl. Turnix) B X Y +	*Upupidæ A X Y* —
Rallidæ A B X Y +	*Bucerotidæ A X Y* —
Otididæ B X Y +	*Alcedinidæ A X* —
(Incl. Cariama and Serpentarius.)	
Phœnicopteridæ B X Y +	
Musophagidæ A B X Y +	II.
Centropinæ A B X Y +	
Cuculinæ A X Y +	*Passeres* { *A X Y* —
Psittaci A X Y ±	*A X* —
Anatidæ A B X +	*Trogonidæ A X* —

Spheniscidæ	A B X +
Colymbidæ	A B X +
Podicipidæ	B X —
Procellariidæ	A B X Y + A B X + A X +
Ciconiidæ	A X Y +
Cathartidæ	A X Y + X Y +
Ardeidæ	A X Y— X Y—
Phalacrocoridæ	A X +
Phaethontidæ	A X Y +
Fregatidæ	A +
Falconidæ	A +
Strigidæ	A —
Gruidæ	A B X Y +
Charadriidæ	A B X Y + B X Y +
Laridæ	A X Y +
Alcidæ	A B X —
Columbæ	A B X Y ±

Meropidæ	*A X Y*—
Galbulidæ	*A X Y*— *A X*—
Caprimulgidæ	*A X Y*—
Steatornithidæ	*X Y*—
Coraciidæ	*A X Y*—
Momotidæ	*A X Y*—

III.

Cypselidæ	*A* —
Trochilidæ	*A* —

On looking at the formulæ in the above table it will be seen that there is a tendency to similarity in those that are placed in juxtaposition; and further, that the presence or absence of the ambiens muscle, indicated by the signs + and —, is more constant than the other characters. Thus, among the *Cuculidæ*, the *Picidæ* and *Ardeidæ*, the ambiens does not vary whilst one or other of the rest is inconstant. There are more reasons than the above for assigning primary importance to the ambiens muscle, which depend on the nature of the tip of the oil-gland and the cæca of the intestine. For, with but few exceptions, those birds which possess the ambiens muscle have cæca to the colon and a tuft of feathers on the oil-gland, whilst those in which the ambiens muscle is absent, have either cæca and a nude oil-gland, or a tufted oil-gland and no cæca. The true relationship of the exceptions is, however, indicated by other collateral characters, the most important of which is the presence or absence of the accessory femoro-caudal (B); that muscle being never found in those birds in which the ambiens is always absent, so that any bird with it developed, is certainly related to those in which the ambiens is present. These facts lead me to propose the division of the class Aves into two primary sub-classes,—the *Homalogonati*,* in which the ambiens is present, and the *Anomalogonati*† in which it is always absent. The former of these are printed in the above table in Roman letters, the latter in italics.

It may be asked, why, on the above principles, are the Ardeidæ and the Strigidæ placed with the Homalogonatous birds, especially as the latter have a nude oil-gland? The position of the latter of these two families is no doubt uncertain, but the sum of characters is in favour of the places assigned to it.

Next, respecting the most important sub-divisions of the Homalogonatous, and the Anomalogonatous birds. Taking the latter first, because they are fewer in number, and more clearly separable, they are found to fall naturally into three well-defined orders:—(1) those in which the oil-gland is nude and the cæca of the intestine present; (2) those in which the oil-gland is tufted and the cæca are absent; and (3) those in which the oil gland is nude and the cæca are absent. These three sections of the Anomalogonatous birds are indicated in Table I. by the corresponding numbers, the Picidæ heading the first, the Passeres the second, and the third comprising the Macrochires only. To most ornithologists the not unreasonableness of this arrangement will be fairly apparent.

TABLE II.

Class AVES
- *Sub-class* HOMALOGONATI
  - *Order* I. *GALLIFORMES*
    - *Cohort* (α) STRUTHIONES
      - *Family* 1. *Struthionidæ*
        - *Sub-fam.* 1. Struthioninæ
        - „ 2. Rheinæ
      - „ 2. *Casuariidæ*
      - „ 3. *Apterygidæ*
      - „ 4. *Tinamidæ*
    - „ (β) GALLINACEÆ
      - *Family* 1. *Palamedeidæ*
      - „ 2. *Gallinæ*
      - „ 3. *Rallidæ*
      - „ 4. *Otididæ*
        - *Sub-fam.* 1. Otidinæ
        - „ 2. Phœnicopterinæ
      - „ 5. *Musophagidæ*
      - „ 6. *Cuculidæ*
        - *Sub-fam.* 1. Centropinæ
        - „ 2. Cuculinæ
    - „ (γ) PSITTACI (†)
  - *Order* II. *ANSERIFORMES*
    - *Cohort* (α) ANSERES
      - *Family* 1. *Anatidæ*
      - „ 2. *Spheniscidæ*
      - „ 3. *Colymbidæ*
      - „ 4. *Podicipidæ*
    - „ (β) NASUTÆ
      - *Family* 1. *Procellariidæ*
      - „ 2. *Fulmaridæ*
        - *Sub-fam.* 1. Fulmarinæ
        - „ 2. Bulwerinæ
  - *Order* III. *CICONIIFORMES*
    - *Cohort* (α) PELARGI
    - „ (β) CATHARTEÆ
    - „ (γ) HERODIÆ
    - „ (δ) STEGANOPODES
      - *Family* 1. *Phaëthontidæ*
      - „ 2. *Pelecanidæ*
      - „ 3. *Phalacrocoridæ*
      - „ 4. *Fregatidæ*
    - „ (γ) ACCIPITRES
      - *Family* 1. *Falconidæ*
      - „ 2. *Strigidæ*
  - *Order* IV. *CHARADRIIFORMES*
    - *Cohort* (α) COLUMBÆ
    - „ (β) LIMICOLÆ
      - *Family* 1. *Charadriidæ*
      - „ 2. *Gruidæ*
      - „ 3. *Laridæ*
      - „ 4. *Alcidæ*
- *Sub-class* ANOMALOGONATI
  - *Order* 1. *PICIFORMES*
    - *Family* 1. *Picariæ*
      - *Sub-fam.* 1. Picidæ
      - „ 2. Ramphastidæ
      - „ 3. Capitonidæ
    - „ 2. *Upupidæ*
    - „ 3. *Bucerotidæ*
    - „ 4. *Alcedinidæ*

* With the knee normal, that is, with the ambiens crossing it.

† With the knee abnormal; that is, with the ambiens deficient.

Order II. *PASSERIFORMES*
Family 1. *Passeres*
" 2. *Bucconidæ* (?)
" 3. *Trogonidæ*
" 4. *Meropidæ*
" 5. *Galbulidæ*
" 6. *Caprimulgidæ*
" 7. *Steatornithidæ*
" 8. *Coraciidæ*
Sub-fam. 1. Coraciinæ
" 1. Momotinæ
" 3. Todinæ (?)

Order III. *CYPSELIFORMES*
Family 1. *Macrochires*
Sub-fam. 1. Cypselinæ
" 2. Trochilinæ

The Homalogonatous birds must be divided upon a different basis, and their myological formulæ here come into service. Before going further it is necessary to show that the habits of the species are not the cause of their myological peculiarities in most cases, though probably in some they do affect them. The Heron and the Swallow have the same formula, and yet how different their habits? the same may be said of the Owls and the Swifts; the Kaleege and the Flamingo. The Auk and Guillemot, however, are most probably but distantly related to the Ducks and Penguins if the peculiarity in the nasal bones has the importance that I assign to it; nevertheless, the muscles of their legs agree more with them, than with the other Schizorhinal birds. By a glance at Table II., the manner in which the Homalogonati may be best subdivided according to the facts that I have been able to bring forward, may be obtained. Commencing with the orders, the *Galliformes* include all those birds related to the Fowls; and notwithstanding the high opinions to the contrary, I cannot feel justified in separating the Struthious birds away from this group. It is not difficult, after having seen the formula of the Musophagidæ and Cuculidæ (Table I.), to recognise that these families have nothing to do with the Anomalogonatous birds, although they are peculiar in the former having no cæca, and the latter a nude oil-gland. The Psittaci also cannot be placed anywhere else.

The *Anseriformes* all agree, with the exception of the Storm-Petrels, which are also otherwise difficult to place, in wanting the accessory semitendinosus (Y), and in having the great pectoral muscle very elongate. The whole family of petrels are exceptions in this point also, and may have to be put in the next order, amongst the Ciconiiformes.

The *Ciconiiformes* include amongst them the Accipitres, but myology is in no point more clear than with regard to the unnaturalness of that family as at present defined. Every Eagle, Hawk, true Vulture, and Owl, has for formula A. The Secretary Bird, which is generally placed with them, is represented by B X Y; from which it is seen to be as different from them as it can possibly be. This shows that the position of *Serpentarius* must be changed; that it is not a raptorial bird at all; and that, as in formula and general appearance it resembles *Cariama*, it must be placed near it and the Bustards. Similar arguments indicate that the Cathartidæ are not true Accipitrine birds, but must form an independent family, though still in the same order as the Falcons.

The *Charadriiformes* all possess the peculiar nasal arrangement which I have termed Schizorhinal. The *Turnicidæ* and *Parridæ* are included with the *Limicolæ*, and the *Pteroclidæ* with the *Columbæ*.

The justification of many of the smaller divisions of the above orders will be seen by comparing the myological formulæ, and by a review of the osteological, pterylographical, and visceral arrangement of each.

In any attempt at classification on new facts, it must be remembered that there must be great inequality in the importance of the results arrived at in each order as freshly defined. In one family there may be a uniformity in a particular structure which is greater than could possibly have been expected; whilst in another the previously constant character may be one of the most uncertain. For instance, the left carotid artery is alone present in all the Passerine birds that have ever been examined; but amongst the Bustards the Great Bustard has two, Denham's only the right, and *Tetrax* only the left. Therefore it is not to be wondered at that myology is equally uncertain in its indications sometimes, though on other occasions its teaching is most decided. In the above attempt at a new arrangement, it has been my endeavour to bring forward the results of observations made during a considerable time, with the facts obtained from previous work always kept prominently in the foreground.

NOTES

In a Congregation held at Oxford on Feb. 10, Prof. H. Smith introduced a statute providing that the certificate of the examiners appointed under the authority of the Delegates of the Examination of Schools, when given in Greek, Latin, and Elementary Mathematics, be accepted in lieu of Responsions. He represented that in Mathematics the standard would be higher than in Responsions; in Greek and Latin it would be equal, owing to the requirement of translation of "unseen pieces." The candidate would also have to pass in some other subject. It was therefore inconceivable that the idle should select the Schools Examination as the easier. The standard would be kept up by the employment of the same class of examiners as in other University examination. The preamble of the statute was accepted.

Dr. H. Alleyne Nicholson, Professor of Natural History in University College, Toronto, has been appointed to the Professorship of Zoology in the Royal College of Science, Dublin, vacant by the resignation of Dr. Traquair. Prof. Nicholson is known as the author of many papers on the Graptolites, and as a writer of several text-books of zoology.

The Smith's Mathematical Prizes have been adjudged to Mr. Walter W. R. Ball, second wrangler, 1874, and Mr. George Stuart, B.A., Emmanuel College, Cambridge, bracketed fourth wrangler, 1874.

A meeting of those who have signified their interest in the formation of the new Physical Society will be held on the 14th inst., at 3 o'clock P.M., in the Physical Laboratory, South Kensington.

A general meeting of the Provisional Committee for the establishment of the Scientific Societies Club was held on Jan. 29 at the Westminster Palace Hotel, when an organising committee was appointed with a view to the early opening of the club. The number of "original members" is nearly complete, 231 gentlemen having given in their names. Among the Provisional Committee we notice the names of Dr. Gladstone, Prof. Lawson, and Prof. Morris, and others known to Science.

At the meeting of the Paris Academy of Sciences on Feb. 2 the place of Correspondent of the Astronomical Section, left vacant by the election of Sir George Airy to a Foreign Associateship, was filled up. M. Tisserand obtained 25 votes and M. Stéphan 23. The former was therefore elected. At the same meeting the Academy, sitting in secret committee, received the report of the committee appointed to select candidates for the Chair of Embryology at the College of France. M. Balbiani was placed first, M. Gerbe second. The election was announced for the 9th inst.

WE have just received from Mr. Gerard Krefft the cast of a fossil specimen of extreme interest. It is that of one of the teeth of an extinct species of *Ceratodus* found with the usual *Diprotodon* remains in the alluvial deposits of the Darling Downs district of Queensland. This able naturalist has named the fish indicated by this fossil in honour of the present Colonial Secretary of Queensland, *Ceratodus palmeri*. It is larger than the corresponding tooth—the left upper dental plate—of *C. forsteri*, the enamel being rather coarser and the surface more undulated than that of Forster's fish. In the specimen under consideration, three of the prongs are perfect, being three-fourths of an inch in width. Mr. Krefft mentions that the existing fish is called "Jeevine," and not "Barramundi;" also that it never goes ashore, and is not caught, as supposed by some, with hooks baited with frogs.

THE *Academy* has been favoured by Dr. Kirk with the following private telegram, which he received from Brigadier-General Schneider, C.B., Her Britannic Majesty's political Resident at Aden, with reference to the news of the death of Livingstone. Dr. Kirk considered that the details given in the telegram as published concerning Livingstone's death and the embalming of his body presented so many doubtful points which required clearing up, that he was anxious to ascertain whether Cameron had convinced himself of the accuracy of these reports by personal examination of the messengers who, it is said, preceded Livingstone's dead body to Unyanyembe, and among whom was Chumah, his servant; or whether the reports had come to his ear, before Chumah himself reached Unyanyembe, in the usual untrustworthy and exaggerated native manner. He therefore telegraphed to General Schneider; but, as will be seen by the reply from General Schneider, it cannot be ascertained at present whether Cameron actually saw Chumah. The evil tidings may have preceded him by some days; and there is nothing for it but to wait the receipt of Cameron's written advice:—"General Schneider to Dr. Kirk.—Aden February 2, 5.15 P.M.—Captain Prideaux merely says Chumah went ahead and gave intelligence to Cameron."

THERE has been instituted by the French Government, under the Minister of Public Instruction, a Commission of Scientific and Literary Voyages and Missions. The object of the Commission, we learn from *Les Mondes*, is (1) to discover what are the most useful scientific and literary enterprises; (2) to examine the projected voyages and missions proposed to the Minister; (3) to study the programmes of these missions, to give detailed instructions to those who undertake them, and to carry on correspondence, if necessary, during the voyage; (4) to examine, on their return, the works on which the voyagers have reported, and prepare their publication in a record of Missions, when that is founded; (5) to name to the Minister such voyagers as may be worthy of honourable reward after the completion of their enterprise; (6) to appeal to the various administrations to concentrate on certain enterprises all the resources at the disposal of the state. The Under-Secretary of State is President of the Commission, and M. Beulé Vice-President; while, among the members are, MM. Felix Ravaisson, Conservator of the Louvre Museum, Leon Renier, Chevreul, Milne-Edwards, D'Avezac, President of the French Geographical Society.

THE *Paris Journal* gives a curious account of an hotel situated in the *Rue des Petites Ecuries*, which has a *clientèle* of living phenomena. It is an hotel of the lowest order, which was fitted up by a French barman for housing extraordinary creatures. The *homme chien* and his son Fedor lived there for some time. The giant of Folies Bergeres (8 ft.) dwelt there. He was an intimate friend of a dwarf whom he carried in his arms every evening, when taking his daily promenade after dark. There are also a good many acrobats and lion-tamers admitted into the house. Mdlle. Christine, the double sisters, were not a lodger; they had an agent of their own, an Englishman. Most of these curious specimens of humanity are placed under the direction of the hotel-keeper, who procures engagements for them at certain prices, according to their *demerits*, and directs them either to some of the minor theatres, concert-halls, or to the booths erected at suburban fairs. A *Table d'hote* of the Petites Ecuries Hotel, where all these strange creatures come together, is the most extraordinary sight in the whole town.

THE sale of several works on the book-stalls at railway-stations. has been prohibited by the Minister of the Interior, Amongst these we notice "Les Ballons du Siege," by M. W. de Fonvielle, who, as it is known, escaped from Paris in a balloon during the investment of Paris, and delivered lectures in London. M. W. de Fonvielle, who was just returning from London when the prohibition was issued, has written to the minister in order to ascertain the real facts of that extraordinary decision.

DR. A. ERNST prints (unfortunately in Spanish) under the title "La fécula y las plantas farináceas del nuevo mundo," a list of 100 plants of the New World which yield starch, with detailed accounts of the more important ones.

IN the discussion which followed, Sir Bartle Frere's address, at the opening of the African Section of the Society of Arts, Mr. Hyde Clarke read a letter from Lieut. Maurice, Private Secretary to Sir Garnet Wolseley, dated "Head Quarters, Yancoomassie," from which we take the following extract; it may prove of some interest to students of the Science of language:—"A somewhat curious piece of word-coining, which has fallen under our notice here, may interest you in connection with the broader aspects of the subject of which you write. The Ashantees having experience of our rockets only as they come to them in destructive form at the end of their journey, call them by the sound they make, 'Schou-schou,' or something of the kind. The Fantees, on the other hand, adopt bodily into their Language our own names for those things which they have not seen before. Thus to the Houssa or the Fantee, in speaking to one another, our rockets are named rockets, while their enemies call them schou-schou. It is possible that as war has not been in savage times an uncommon condition of mankind, analogous causes for different names having been adopted by different nations may have been not unfrequent in the past."

THE Council of the Statistical Society have founded a bronze medal, under the title of "The Howard Medal," to be presented to the author of the best essay on some subject in "Social Statistics," a preference being given to those topics which Howard himself investigated, and illustrated by his labours and writings. The title of the Essay to which the Medal will be awarded in November 1874, is as follows:—"The state of Prisons, and the condition and treatment of Prisoners, in the Prisons of England and Wales, during the last half of the eighteenth century, as set forth in Howard's "State of Prisons," and work on 'Lazarettos.'" The Essays must be sent to the Assistant Secretary of the Society on or before September 30. The competition is open to any competitor, providing the Essay be written in the English language.

WE called attention last week to the course taken by the Perthshire Society of Natural Science, in reference to the present election of Members of Parliament. The Society sent questions to the candidates for the City and County of Perth, relating to the appointment of a responsible Minister of Education, to State help for Science, and to the promotion of scientific exploration expeditions, such as that of an Arctic expedition. The Liberal candidates sent no reply; the Conservative candidates sent favourable answers. The following is the reply of Sir William Stirling Maxwell, the Conservative candidate for the County of Perth:—

"In reply to your letter, I beg to say that I have long been of opinion that the Existing Education Department, and all our Public and Literary Institutions should be placed under the general supervision of a responsible Minister. In Parliament I was generally inclined to favour the expenditure of money for scientific objects when the Government thought proper to sanction them; and an Arctic expedition, and various researches, unremunerative in a pecuniary sense, might fairly fall into the list of such objects."

IN June last year Prof. O. C. Marsh, the discoverer of *Dinoceras and Brontotherium*, started on a five months' geological expedition to the Rocky Mountain regions and the Pacific coast, to study, as he had done on previous occasions, the Cretaceous and Tertiary formations, which are there so rich in vertebrate remains. From Fort McPherson, Nebraska, they proceeded to Niobrara under the escort of two companies of United States cavalry, which were indispensable on account of the hostile position of the Indian tribes. Among the other places visited were Fort Bridger, Wyoming; Idaho and Oregon; Colorado and Kansas. The expedition was very successful, and the collections procured were large, containing many new forms. It is much to be regretted that no English geologists have accompanied Prof. Marsh, as most of the fossils peculiar to the regions he is exploring, are quite unknown in this country, except from descriptions.

THERE will be held at Christ Church, Oxford, on Saturday February 28, an election to a Junior Studentship in Physical Science, tenable for five years from the day of election. It will be of the annual value either (1) of 100*l*. (inclusive of an allowance for room rent), if the Governing Body shall so determine; or (2) of 85*l*. (also inclusive of an allowance for room rent), which sum may be raised to the larger sum above-named after the completion of one year's residence, if the Governing Body shall so determine. Candidates must call on the Dean on Wednesday, February 18, at 1.30 P.M. The examination will follow at 2 P.M. Candidates must not have exceeded the age of 20 on the 1st of January last, and must produce certificates both of the day of their birth, and of good character. Papers will be set in Chemistry, Physics, and Biology; but candidates will not be expected to offer themselves for examination in all these subjects.

PROF. COPE has recently explored the beds of the late tertiary formation, called Pliocene, as it occurs in north-east Colorado. He discovered twenty-one species of vertebrata, mostly mammals, of which ten were new to science. Four are *carnivora*, six horses, four camels, two rhinoceroses, one a mastodon, &c. The most important anatomical results attained are that all the horses of the formation belong to the three-toed type, and that the camels possess a full series of upper incisor teeth. The discovery of a mastodon, of the *M. shisticus* type, constitutes an important addition to the fauna. One of the horses is distinguished by its large head and slender legs, much longer than in the common horse. A full account of these results will shortly appear in the report of Dr. Hayden's Geological Survey of Colorado.

THE additions to the Zoological Society's Gardens during the past week include a Suricate (*Suricata zenic*) from S. Africa and a West African Python (*Python sebæ*), presented by Mr. J. H. Coonley; a Feline Douroucouli (*Nyctipithecus felinus*) from Brazil, presented by Mr. G. Hollis; a common Kingfisher (*Alcedo ispida*), British, presented by Mr. A. Yates; a Collared Fruit Bat (*Cynonycterus collaris*), an Axis Deer (*Cervus axis*), and a Molucca Deer (*C. moluccensis*), born in the Gardens; two De Fillippi's Meadow Starlings (*Sturnella de filippi*) from Rio de la Plata, received in exchange.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Jan. 22.—"On the Physiological Action of the poison of *Naja tripudians* and other Indian Venomous Snakes," II., by Drs. Brunton and Fayrer.

The results of these investigations show that the poison of the cobra is similar in its action to that of Ophiophagus, Bungarus, and other Colubrine snakes, whilst that of Daboia is similar to the virus of Echis, the Trimeresuri, and other viperine snakes, the chief difference between them being the greater tendency in the viperine poison to cause hoemorrhage or more severe local symptoms. The blood of animals killed by the viperine snakes generally remains fluid after death, whilst that of animals killed by colubrine snakes form a firm coagulum.

The conditions caused by the action of the poison are illustrated by the symptoms manifested by man and the lower animals. The Cobra, Ophiophagus Hydrophidæ, and Bungarus are all very deadly. The Viperine Daboia, and Echis are scarcely less so, whilst the Indian Crotalidæ, such as the Trimeresuri, are much less so. A series of experiments is detailed which illustrate the physiological action of the virus on the nervous system, the muscles, the blood, the respiration, the circulation, and the function of excretion, and also the mode in which death is produced.

The fatal action is shown to be due (1) to arrest of the respiration by paralysis of the muscular apparatus, by which that function is carried on. (2) Or by rapid arrest of the heart's action, in cases where the poison has found direct entry by a vein, *e.g.* the jugular. In such, death is almost instantaneous, and the heart is found to have ceased to beat when in systole. The physiological import of this is very interesting and important, and it was demonstrated by Dr. Brunton, who explained its probable mode of action in certain ganglionic centres in the heart; a subject which gave rise to some discussion; (3) or death may be due to a combination of arrest of respiration and of the heart's action; (4) or it may be due where the quantity of poison is small, or its quality less active, to secondary causes of the nature of other septicæmia, a purely pathological question not discussed.

The mode in which paralysis of respiration (the ordinary form of death) is induced, has been most thoroughly investigated, and it may be said that the question is now settled.

The virus absorbed into the blood either by inoculation into the areolar tissue, or by application to a mucous membrane, affects the cerebro-spinal nerve-centres, the nerves and their peripheral distribution, more especially of the motor nerves. The sensory nerves are less and later affected, and the intelligence generally latest of all, and slightly. The complete loss of it, and the convulsions which precede death, is mainly caused by the circulation of venous blood, the result of the impeded respiration.

Muscular force and co-ordination are gradually lost; paralysis and asphyxia being the evidence thereof. In ordinary cases, the heart goes on beating vigorously long after apparent death, and with artificial respiration, may be kept up for many hours.

The investigations recorded, were made with cobra and daboia poison, sent to England from Bengal in the dried state, a condition in which it resembles gum arabic, and its activity is great. The animals experimented on were dogs, cats, rabbits, guinea-pigs, fowls, pigeons, small birds, frogs. Its action on all these, and the mode in which functions and tissues are affected, are recorded in detail, as well as the extent to which the action of the poison is modified when introduced through different channels.

It has now been clearly shown that the poison acts, when introduced into the stomach, or when applied to a mucous or serous membrane. The idea that it was only effective when injected directly into the blood, is erroneous. It is, no doubt, more certainly and rapidly fatal when it enters the blood direct.

It is also shown that it may be eliminated by the excreting organs, and that there is, therefore, reason to hope that life may be saved if it can be artificially sustained long enough to admit of complete elimination being accomplished, as in the case of curare poisoning; but from the more complex action of the cobra poison this remains a subject of doubt.

By artificial respiration the circulation has been maintained, both here and in India, by Dr. Ewart and Mr. Richards, for many hours; and in one case, after complete paralysis had occurred, symptoms of reaction and elimination were obtained; but no complete recovery has yet occurred. The doubt still remains whether the nervous system that has sustained so much damage, is capable of ever resuming its functions, even though elimination be complete.

The so-called antidotes appear to be inert; all that have been submitted to trial, including the intra-venous injection of ammonia, have failed to have any satisfactory effect. Artifical respiration has certainly prolonged life, and partial recovery has followed, but no life has actually been saved by it.

The microscopic appearances of the blood are described, but no very remarkable change was observed beyond crenation of the corpuscles or diminished aggregation into rouleaux. Chemical examination of the blood and its gases is still needed and further analysis of the poison is desirable.

It is shown that the activity of the poison is scarcely impaired by drying, excepting perhaps so far as regards its *local* action.

Dilution with water, glycerine, liq. ammoniæ, and liq. potassæ did not destroy its activity, nor did coagulation by boiling in the ordinary way. The boiling for half-an-hour under a temperature of 102° C. seemed to destroy the activity of one specimen which was injected into a bird.

The poison acts on all life, on the lower and higher vertebrata, the invertebrata, and even on vegetable life; for it retards, although it may not arrest the germination of seeds. But it acts most vigorously on the warm-blooded animals.

The most remarkable fact connected with it is that it has little or no effect on poisonous snakes. They can neither poison themselves nor their congeners; or if at all, very slightly so, whilst the poison acts rapidly and fatally on innocent snakes, lizards, fish, and mollusca.

With reference to the means of preventing death, it may be said that those that mechanically prevent the entry of the poison into the circulation by means of the ligature, excision, or cautery are the most reliable, but that they are only so when applied immediately.

No means that offer any hope of benefit should be neglected, and it is possible that stimulants such as alcohol and ammonia may be useful; and in some cases, where the poisoning has been severe but not fatal, do good and even determine recovery where death would have otherwise resulted. The so-called antidotes, however, beyond any actions of this kind that they may possess, are apparently quite inert.

Transfusion of blood is alluded to, but the experiments hitherto proposed have not met with success. A more perfect way of accomplishing it may be more successful.

Zoological Society, Feb. 3.—Dr. E. Hamilton, vice-president, in the chair. The secretary read a report on the additions that had been made to the society's menagerie during the month of January, 1874, amongst which were specially noticed a female Water-Deer (*Hydropotes inermis*), a pair of Pink-headed Ducks (*Anas caryophyllacea*), and a Dusky Monkey (*Semnopithecus obscurus*), acquired by purchase, and two Vulturine Guinea-fowls (*Numida vulturina*), presented by Dr. J. Kirk.—An extract was read from a letter addressed to the secretary by Mr. Luigi M. L. Albertis, containing an account of a new species of kangaroo, of which he had lately obtained a living specimen from New Guinea, and which he had proposed to call *Halmaturus luctuosus*.—Dr. Cobbold communicated the second part of a series of papers entitled "Notes on the Entozoa;" being observations based on the examination of rare or otherwise valuable specimens contributed at intervals by Messrs. Charles Darwin, Robert Swinhoe, Charles W. Devis, the late Dr. W. C. Pechey, Dr. Murie, and others.—Mr. Garrod read a paper in which he proposed a new classification of birds, details of which will be found in another page.

Chemical Society, Feb. 5.—Prof. Odling, F.R.S., president, in the chair.—The secretary read a preliminary notice on the action of benzyl chloride on the camphor of the Lauraceæ (*Laurus camphora*), by Dr. D. Tommasi.—Dr. C. R. A. Wright had a paper on the Isomeric Terpenes and their derivatives: Part III. On the essential oils of wormwood and citronelle; being a detailed account of his experiments on these substances, a preliminary notice of which was communicated to the society some time since.—The other communications were a preliminary notice on the perbromates, by M. M. Pattison Muir, F.R.S.E.; and on the coals from Cape Breton, their cokes and ashes, with some comparative analyses, by Henry How, D.C.L. The latter paper giving the amount of coke produced by slow and quick coking, from the main seam coal of Sydney mine, Nova Scotia, and the Lingan coal, also analyses of the ashes left by these coals.

Royal Microscopical Society, Feb. 4.—Anniversary meeting.—Chas. Brooke, F.R.S., president, in the chair. The report of the council and the treasurer's statement of accounts were submitted and adopted, and the officers and council for the ensuing year were elected. The president delivered an address, and concluded with obituary notice of Fellows deceased since the last annual meeting. The following gentlemen were elected as officers and council. President—Chas. Brooke, F.R.S. Vice-Presidents—Dr. Braithwaite, F.L.S.; J. Millar, F.L.S.; W. Kitchen Parker, F.R.S.; F. H. Wenham, C.E. Treasurer—J. Ware Stephenson, F.R.A.S. Secretaries—H. J. Slack, F.G.S.; C. Stewart F.L.S. Council—J. Bell, F.C.S.; F. Crisp, B.A.; Dr. W. J. Gray; J. E. Ingpen; S. J. McIntire, H. Lee, F.L.S.; W. T. Loy; Dr. H. Lawson; H. Perigal, F.R.A.S.; A. Sanders; C. Tyler, F.L.S.; T. C. White. Assistant Secretary—Walter W. Reeves.

Royal Horticultural Society, Jan. 21.—Scientific Committee.—A. Smee, F.R.S., in the chair.—The Rev. M. J. Berkeley sent portions of holly stems pierced by the larva of the wood leopard moth (*Zeuzera Æsculi*).—Prof. Thiselton Dyer exhibited a small branch of *Vitis gongylodes* from the Victoria House at Kew. The end appeared to have been broken off, and the adjacent internodes had (apparently in consequence) swollen into a mass like a small cucurbitaceous fruit.—Prof. Lawson remarked that an Indian vine (*Vitis quadrangularis*) ordinarily had the internodes swollen, though not to anything like the same extent.—A conversation then arose as to the production of aerial roots by vines.—Mr. Worthington Smith, F.L.S., detailed the results of a series of experiments made with the object of ascertaining how far perfectly sound potatoes can be contaminated by contact with infected ones.—Mr. Andrew Murray, F.L.S., made some remarks on interesting plants suitable for horticulture which he had met with in the Rocky Mountains.

General Meeting.—Mr. W. A. Lindsay, secretary, in the chair.—Prof. Thiselton Dyer made some remarks on a parasitic fungus, which was proving exceedingly destructive to hollyhocks. It has been identified by Berkeley in this country, and subsequently by Durieu de Maisonneuve, in France as *Puccinia Malvacearum* of Montagne; it was first described from specimens collected in Chili by Bertero.

EDINBURGH

Geological Society, Dec. 18, 1873.—On some points in the connection between Metamorphism and Volcanic action, by Prof. Geikie, president. After adverting to his previously published views regarding the connection between the protrusion of granite and ordinary volcanic rocks, the author proceeded to point out that the facts were probably capable of a wider interpretation. The metamorphism of large areas was well known to be intimately related to the contortion and plication of rocks, highly metamorphosed regions being those where the rocks had undergone the most intense pressure and crumpling. Heat would necessarily be evolved in the process of compression, and might have been in some parts sufficient actually to fuse the rocks. Such fused portions were probably recognisable in the masses of granite, syenite, porphyry, and other so-called igneous rocks so common in metamorphosed regions. These views were shared by many able geologists of the present day. The author, referring to the recent memoir of Mr. Mallet, pointed out that such conditions as those indicated by the facts of metamorphism were eminently suggestive of the probability that volcanic action had accompanied metamorphism. The extensive crumpling of the rocks of a region indicates a weak part of the crust of the earth through which the internal heat would for a time be more easily transmitted to the surface, while the effect of that crumpling would be greatly to increase the store of heat out of which volcanic energy arises. Hence both by the access given along the line of weakness to the internal heated mass of the earth, and by the increased temperature due to the contortion, water finding its way downward from the surface would encounter conditions eminently favourable for the production of volcanoes. If this speculation has any ground of truth, we should expect to find some evidence of the association of volcanic masses with wide tracts of metamorphism. Without travelling beyond our own country, we seem to have corroboration of it all along the flanks of the highly-contorted, and, over the Highlands, intensely-metamorphosed Silurian hills. The author then gave some details as to the probable thickness of rock under which the present metamorphosed rocks of the Highlands lay at the time of their metamorphism, and showed that it was probably comparatively small. They were in great measure, if not entirely, metamorphosed before the time of the Lower Old Red sandstone. But the process of metamorphism was no doubt a very prolonged one, and we should therefore be prepared to find proofs of its progress at widely separated periods. It is now well known that low down in the Old Red sandstone of the Midland Valley of Scotland enormous sheets of felspathic lavas and tuffs occur, forming such chains of hills as the Saidlaws, Ochils, and Pentlands. No earlier traces of volcanic action have yet been met with in Scotland, but these masses prove that when that action began it was developed upon an enormous scale. The author believed the inference might with much probability be drawn that this vast effusion of volcanic material was a consequence, or it might even be to some extent an accompaniment, of the crumpling and metamorphism of the older Silurian rocks. He drew attention to the way in which these volcanic rocks bordered the Silurian areas on both sides of the broad lowland valley, and to the numerous remarkable bosses of granite, syenite, and porphyry by which the Silurian tracts were pierced. That many of these bosses were formed by the actual fusion of the stratified rocks themselves seemed to him highly probable. But he held also that some of them marked the lower parts of the actual orifices out of which the volcanic materials of the Old Red sandstone had issued. He alluded especially to the singular rounded or dome-shaped hills of granite, felstone, and quartz-porphyry by which the Silurian uplands of the southern counties are dotted, and which, from their general form and their relations to the surrounding stratified rocks recall some of the characters of true volcanic "necks." The sheets of lava and tuff have been preserved in the broad lowland valley owing to faulting and subsidence, while they have been removed from the surrounding hills by denudation, so as to uncover the roots of the pipes or funnels from which they were emitted. After the enormous masses of volcanic materials erupted during the period of the Lower Old Red sandstone, the underground forces gradually declined in vigour, and as the author had shown, became reduced in Permian times to the production of a few small cones scattered over the midland valley, and down the valley of the Nith. The remainder of the paper was devoted to the Tertiary volcanic rocks of the western Highlands. The author showed that in Skye, Raasay, and Mull, masses of granite and quartz-porphyry were associated with the volcanic rocks in such a way as to suggest a community of origin. Even at a distance from the main mass of the basalt plateaux, granite occurred which was almost certainly of Tertiary date. The picturesque granite of Arran, for example, which had long been known to be at least post-carboniferous, he now firmly believed to be of the same age as the terraced hills of Skye and Mull, that is, younger in date than the soft clays on which London is built, and it appeared to be associated with actual *coulées* which had, in some cases, suffered an enormous denudation like that of the Scur of Eigg. He had not yet been able to show that the renewed and prodigious outburst of volcanic action in Tertiary times had been associated with the metamorphism of any wide region, and perhaps no data are obtainable to throw light upon this question. But the extravasation of granite rocks at several places seemed to indicate that metamorphism had taken place, and at least showed, as Mr. Jukes long ago pointed out, that molten granite might be associated with true volcanic action, though it did not reach the surface as granite.—On fossil cones from the Airdrie black-band ironstone, by G. A. Panton.—Notes on the geology of India, by Andrew Taylor.

MANCHESTER

Literary and Philosophical Society, Jan. 27.—R. Angus Smith, F.R.S., vice-president, in the chair.—"On a Source of Error in Mercurial Thermometers," by Thomas M. Morgan, Student in the Laboratory of Owens College. While engaged in distillation, the thermometer, which was placed in a Wurtz tube so that the column of mercury was entirely surrounded by the vapour of the distilling liquid, was found after some days to indicate three degrees too little—a discrepancy caused by volatilisation from the surface of the column of mercury and condensation on the upper part of the tube. By causing the mercury to flow to the end of the tube and back, the condensed portion was gathered up and the correct temperature indicated. It has since been observed that after each day of distillation, with liquids boiling between 60° and 100° C., a quantity of mercury equal to 1°·or 1°·5 volatilises.—"Notes on fossil Lithothamnia so-called Nullipore)," by Arthur Wm. Waters, F.G.S. These attain their greatest development in the Leithakalk, a miocene formation which is principally, in some cases almost entirely, composed of these algæ. But they are in no way confined to the Leithakalk, being also very abundant in the eocene, especially the upper division; the so-called granit-marmor, or Bavarian marble, a nummulitic formation, is very largely composed of this concretionary-looking body. In North Italy it abounds in the eocene formations which are so largely developed in the Veronese and Vicentin. In many places the formation is some hundred feet, much more than half composed of the Lithothamnium. It occurs abundantly in Hungary and Switzerland. The so-called pisolithic limestone of Paris is according to Gumbel about eight-tenths stone algæ; also M. Mario, Astrup; the pleiocene of Castel Arquato; and in fact it seems to be found in most of the tertiaries on the Continent; it is further found in the chalk at Maestricht, and in the jurassic sponge beds at Schwabenbergs. The object of this paper is to draw attention to the great masses of these bodies and the importance of always noticing their occurrence in geological formations, since it should be a very material help in regard to the climate, and the conditions of the coasts and currents, besides being of great stratigraphical assistance; nor is it of less importance to note carefully the growth of recent ones, for only through a knowledge of the present can we interpret the past.

PARIS

Academy of Sciences, Jan. 26.—M. Bertrand in the chair.—The following papers were read:—Note on magnetism, in answer to M. Gaugain, by M. J. Jamin.—Direct demonstration of the equation $\int \frac{dQ}{T} = 0$ by M. A. Ledieu. This was a continuation of the paper read at the last session of the Academy by the same author.—Note on the Rhone irrigation canal, by M. A. Dumont.—Several papers on the action of water on lead, were received.—Organogenesis compared with androgenesis (*l'androcée*), by M. Ad. Chatin. This part of the paper dealt with the *Saxifrageæ* and *Crassulaceæ*.—On the lateral solfataras of the Chili volcanoes, and on certain new minerals, by M. L. Domeyko.—On the history of the question as to the passage of birds through the air, by M. A. Penaud.—On the shocks of earthquake at Nice, by M. Prost.—Determination of the pluckerian numbers of envelopes, by M. H. G. Zeuthen.—On the apparent orbit and period of revolution of the double star ζ Herculis, by M. Flammarion.—On the variable state of voltaic currents, by M. P. Blaserna. This was an answer to M. Cazin's recent remarks on the subject.—On a new saccharometer and a new method of obtaining an absolutely monochromatic sodium flame, by M. Laurent. The latter object is attained by interposing a cleavage plate of a crystal of potassic dichromate between the polariser and the flame. This absorbs nearly everything but the yellow light of the flame.—Researches on the flow of liquids through capillary tubes, by M. A. Gueroult.—On a new laboratory balance, by M. Deleuil.—On ethylic oxalurate and oxamethane cyanurate, by M. E. Grimaux.—On the grafting of dental follicles and of their constituent organs, separately by M. Legros and Magitot.—Remarks on M. Martin's paper on the comparison of the anterior member of the "*Monotremes*," with those of birds and reptiles, by M. E. Alix.—Note on the ammoniacal fermentation of urine, by M. A. Lailler.—On the pretended emission of ozone by plants, by M. J. Bellucci. The author had made a number of comparative experiments on this subject. He found the colouration of the test paper to be due to the combined action of light and moisture.

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THURSDAY, FEBRUARY 19, 1874

PHYSIOLOGY AT CAMBRIDGE

WE are not of those who believe that the *quality* of the scientific work produced by any country as a whole, is dependent to any great extent on the facilities afforded for special study, though the *amount* yielded in any special direction varies directly as the opportunities and encouragement which are offered. All experience goes to show that the ability of the individual is a constant quantity, and that whatever direction his mind takes, as the result of the circumstances in which he is situated, he is sure to rise to a certain standard of excellence in the quality of his productions, and no higher; in other words, the same facts put before two men of different mental powers will be employed in producing results of different quality, dependent on those powers. The backward state of physiology, and it may be said, of biology generally in comparison with the more exact sciences, has recently become so conspicuous, that attempts are being made by many of the leading scientific men to attract into these comparatively untrodden paths some of those able minds which would otherwise have devoted their best energies to the mastery and further elucidation of points in a subject such as mathematics, which may be almost said to have reached the limit of human mental power, as far as the methods at present at its disposal are concerned. In biology and physiology, however, the case is very different; their students may be said to be suffering from a glut of facts and disconnected minor theories, which want the assistance of some master minds to weed and connect them, so that the road may be made more easy for other less gifted workers. That such is the case is rendered evident by the undecided and tentative way in which most biological problems are on all sides discussed. Opinions the most opposite are held on fundamental points by partisans of different schools, and discussion becomes more a question of which side can be most subtle in its language or most dogmatic in its statements, rather than which is the true exponent of the subject under consideration. In such cases the precise statement of the problem by a master-mind would set the question at rest once and for all.

There are, however, many difficulties in the way of getting men suitable for this higher work, and for more than one reason. One of these is that there are very few who can be made to undertake the thorough training in more than a single subject, that is necessary for it. A student of ability at the University of Cambridge, for instance, takes up mathematics, and too soon finds that he has every reason to expect considerable pecuniary reward if he devotes the whole of his period of studentship to working for his tripos; he cannot but devote the whole of his time to the single subject, for otherwise those of equal powers who did so would beat him in the race and prevent his appearing in the tripos list in that position which insures him a fellowship, and therefore a competency. He keeps to his subject and reaps the reward; but by that time other duties, generally of a social nature, together with the narrowing effect of his one-sided education, have removed all his inclination to strike out a fresh line of thought, and he commences the routine of life, acquiring by every-day experience those facts which so many others of equal ability have learned before, and which he cannot therefore turn to any good account. The great defect of the Cambridge mathematical tripos is that it is too ultimate, and too complete in itself. The day on which the list comes out is that on which most think that mathematics has done as much good to them as it can do, and on that day most throw over for ever that genuine method of working which has occupied so much of their time and thought during the three or more previous years, never to return to it.

Things being so, all must have felt intense satisfaction at the establishment at last of laboratories in Cambridge, such as that for Practical Physiology by Trinity College, under the able superintendence of Dr. Michael Foster, and that for Practical Physics by the Chancellor, under Prof. Clerk-Maxwell, whose return to Cambridge has itself been a great stimulus to the advance of the subject in which he so greatly and so justly shines.

The first instalment of original papers from the former of these newly-founded institutions has recently been published. From the manner in which the researches have been conducted, from the thoroughly scientific and careful method of work adopted, the great discretion and experience of the Professors, as well as the excellent quality of the minds, with the assistance of which he has to deal, are evident. No teacher can help having a feeling of satisfaction at such work as that of Mr. Balfour and Mr. Liversidge, which shows signs of excellent mental training as well as a thorough love of the subject. Dr. Foster's standard is evidently a high one, and from the papers before us it is certain that on future occasions only thorough work, based on well-verified facts, arrived at by the most approved modern methods, and checked by the researches of previous authors, are to be expected from his laboratory.

Besides the papers on the development of the blastoderm and blood-vessels of the chick, and on the amyloid ferment of the pancreas, by the two above-mentioned authors, Dr. Martin gives some short notes on the structure of the olfactory mucous membrane in connection with the observations of Max Schultze and Exner. Mr. Dew-Smith records the results of observations—made with the assistance of that beautiful instrument the pendulum myographion—on double nerve stimulation, or the simultaneous stimulation, by two pairs of electrodes, of a single nerve, with well-marked and very instructive results. Mr. Yule also has a paper on the mechanism of opening and closing the Eustachian tube, in which, besides clearing up some points connected with their physiological function, he throws fresh light on the correct anatomy of their pharyngeal orifices.

In one of the papers, that by Dr. Foster himself, which is referred to by Mr. Lewes in this Journal (NATURE, vol. ix. p. 83), Mr. T. O. Harding, senior wrangler in 1872, is mentioned as one of those who have been working in the laboratory. This is a most promising sign; for, as previously remarked, nothing is more wanted than the assistance of such men, in order to show the bearing and value of the various facts laid stress on by pure physiologists. We hope that

Dr. Foster will be successful in attracting other advanced mathematicians to the study of his subject; and better still, that he will be able to persuade those who are in the beginning of their undergraduate mathematical education to devote some of their spare time—quite a recreation as it would be—to learning the first principles and the methods of physiological research, under his able supervision.

Truly Dr. Foster and Dr. Clerk-Maxwell have a noble work before them, and we may hope that by their example and precept Cambridge may after a lapse of thirty or forty years, in the matter of physical and physiological research, be on a level with a second-rate German University.

ATHENIAN TEXT-BOOKS OF SCIENCE

Εγχειριδιον της Χημειας, κατα τας νεωτερας της επιστημης προοδους. (Υπο Αναστ. Κ. Χρηστομανου. Εν Αθηναις. 1871.)

Επιστημονικα Παραδοξα. (Υπο Δ. Σ. Στρουμπος. . . . Αθηνησι, 1864.)

Περι Αερος και των Ενεργειων Αυτου. (Υπο Δ. Σ. Στρουμπος, 1869.)

Περι των Γνωσεων και των Δοξασιων των τε ἀρχαιων και των νεωτερων ὡς προς τα φυσικα φαινομενα εν γενει, και των μεθοδων των ερευνων αυτα. (Υπο Δ. Σ. Στρούμπος, 1858.)

THE University of Athens has existed for no more than thirty-seven years. Two of its four Faculties,—the Faculty of Medicine and the Faculty of Philosophy, require a knowledge of natural philosophy and chemistry. It is difficult to understand how these subjects could have been taught at first, for the students by no means often understand French, and no Greek books on science then existed. No doubt the professors taught as Plato and Aristotle taught; and the note-book of the student had to be his text-book. But matters have changed since then: the demand for text-books in Greek has caused them to appear; slowly indeed, for we have seen but few books on science, but we may hope that the original text-books which are now beginning to appear are the first of a continuous series. Do not let it be imagined that the works whose titles are given above are the only works on science we could find in all Athens. There is a big book on Physics by M. Damaskenos, who has also written on trigonometry and meteorology; there are various memoirs by M. Stroumpos on the refraction of light; on the internal constitution of flame; on the fundamental principles of hydrostatics, &c. The University is tolerably well supplied with physical and chemical apparatus, and in good time, we hope, some good student-work will be done there.

Many of the professors have studied in Paris, and we see evidence in the text-books of French science and of French thought. Prof. Chrestomanos appears, however, in the compilation of his Chemistry, to have consulted most of the recent books and memoirs. We are glad to see Canizzaro often quoted as an authority. The work does not present any specially noteworthy features, but it is sound and eminently clear. The phraseology is at times somewhat strange to a western student; thus we do not employ such words as "Physiography" and "Phutology." . . . After some prefatory remarks concerning the division of the sciences, we have a few pages given to the history of chemistry. The period of Alchemy is wrongly stated to extend from 400 to 1500 A.D. Then Iatrochemistry from 1500 to 1650; Phlogistic chemistry from 1650 to 1783; the new chemistry of Lavoisier and Davy, and so on to the chemistry of Kekulé and Canizzaro. This is followed by a short account of physical chemistry; then an account of crystallography with good figures of crystals. Although many of the names of our elements are derived from the Greek, the table of elements looks rather puzzling: lead is of course *μόλυβδος*, while molybdenum becomes *μολυβδαίνιον*; platinum is λευκόχρυσος; tungsten (or Wolfram) is *βολφράμιον*; nitrogen is at the beginning of the alphabetical list; copper near the end. Again, as to compounds the names of the oxides of nitrogen read as *ὑποξείδιον Ἀζώτου*; *ὀξείδιον Ἀζώτου*; *νιτρῶδες ὀξὺ*; *ὑπονιτρικὸν ὀξὺ*; *νιτρικὸν ὀξύ*. The theory of atomicities is well developed: niobium and tantalum are the only pentatomic elements; while molybdenum and tungsten are the only hexads. The peculiar atomicities of nitrogen and iron are not noticed. The building up of compounds on the type respectively of one, two, and three molecules of water is fully discussed (*μόριον* is the term used in place of our low-Latin *molecula*). Full tables of grouped elements appear; and the naming of compounds is considered. After this considerable and important introduction the work begins with hydrogen in the usual manner, and the account of the other element follows in due course.

The "Scientific Paradoxes" of Prof. Stroumpos is a volume of essays on physics and physiology; including magnetism, electricity, illusions, alchemy. Here too we find paradoxes of another kind; would Mr. Glaisher recognise his name as *ὁ Γλαισχερος*, or Mr. Coxwell as ὁ Κοξουέλλος? The treatise on the Air, by the same author, is a tolerably complete treatise on pneumatics, illustrated by very crude, but original and sufficient woodcuts. The discourse on the history of Science is very interesting, and full of excerpts from Plato, Aristotle, and other ancient writers. For them we think Prof. Stroumpos has claimed too much; we cannot with any degree of certainty assert that Aristotle discovered that the air possesses weight. His experiment at the outset is altogether faulty, for he tells us that an inflated skin (*ὁ πεφυσημένος ἀσκὸς*) weighs more when filled with air than when empty, that is, not inflated. This of course we know from the law of Archimedes is false; a bladder full of air weighed in air can weigh no more than the uninflated bladder.

These works constitute the commencement of Athenian science. The city, while its art, and literature, and philosophy, have unhappily long passed their culminating point, is more scientific than it has ever been before. Not far from the place in which the Peripatetic made his experiment with a crude statera and an empty wine skin with Theophrastus as demonstrator, Stroumpos now weighs his really vacuous vessel, and Chrestomanos explodes oxygen and hydrogen. Thirty years of science in a remote city, out of the highways of European intelligence, cannot effect much; but we hope in the course of the century original workers will multiply in Athens, and as much will be done to promote chemistry and physics, as has been done by Dr. Schmidt in the service of astronomy on the Hill of the Nymphs, over against the Acropolis.

G. F. RODWELL

THE ACRIDIDÆ OF NORTH AMERICA

Synopsis of the Acrididæ of North America. By Cyrus Thomas, Ph.D. Published in Vol. v. of Report of the United States Geological Survey of the Territories. Pp. 1-262. (Washington, 1873.)

IN a prefatory note to this volume the United States Geologist, F. V. Hayden, tells us that Prof. Thomas's work on the Acrididæ of North America is published "in the belief that it is a substantial contribution to natural history;" and certainly it is impossible, on a perusal of the work, not to share in this belief; it is, moreover, another proof of the great boon conferred upon natural science by public surveys and Government expeditions. Serious and extended works on natural history (except, perhaps, those relating to some few very popular branches of it) would seldom be produced, or in many cases their materials be collected, if it were not for the assistance of natural history societies, public surveys, and expeditions; organisations of these kinds can afford to disregard the commercial aspect of the question, and are able to bestow upon the public, works which private enterprise would seldom venture upon. Among insects, the Orthoptera (of which order the Acrididæ are a well-defined family group) are certainly not the most popular among entomologists, though, for many reasons, of great interest to others. Few persons but have some cherished association with, for instance, the persistent chirp of the cricket on the hearth, or the shrill stridulations of some of the grasshoppers; there are, again, few more wonderful sights in the insect world than a flight of locusts; and few natural scourges are more terrible than those inflicted by the devastations of these rapacious creatures; the walking-leaf and stick insects (Mantidæ and Phasmidæ) are also very popular objects for sightseers in natural history museums. The lack of general popularity among collectors and students arises probably in great measure from the Orthoptera being commonly less sightly as cabinet objects than some other orders of the Insecta, though perhaps it arises as much or more from the paucity of works combining both a general and special treatment of the whole, or of well-defined groups, of the order under consideration. Dr. Thomas's work is undoubtedly calculated to encourage the study of the large group included under the *Acrididæ*, and to be peculiarly acceptable to American entomologists, for it not only describes a large number of North American species (both known species as well as new ones), but it gives, in an "Introduction," pp. 9-45, a concise view of the general classification of the Orthoptera, with the relation of the Acrididæ to the other sub-ordinal groups, their structure, internal and external, and the distribution of genera and species over North America. This introductory part of the work is illustrated by two remarkably clear and good woodcuts, showing all the different portions of external structure, with the name of each part.

The remarks of Prof. Thomas, in the chapter on Classification, bring strongly before us the difficulties and imperfections involved in a linear arrangement of any portion of the animal kingdom; but if a real genealogical relationship be that which exists between all living creatures, then it is apparent at once how comparatively unimportant is (generally speaking) the mere linear arrangement of the series; it is, indeed, the only possible one on paper, but in reality some of the most important relationships do not run in one unbroken line, but in lines diverging at many different angles, and in many different planes. Dr. Thomas considers the Orthoptera as arising from the *Crustacea*, and, after reviewing the various extant arrangements of their families, divides that under consideration—ACRIDIDÆ (*i.e.* the saltatorial Orthoptera, or Grasshoppers with comparatively short antennæ), into two *sub-families*—ACRIDINÆ and TETTIGINÆ; the former of these is sub-divided into three divisions: — CONOCEPHALIDES, ORTHOCERIDES, and XIPHOCERIDES, forming (in the order in which these are here given) seven groups:—1. PROSCOPINI; 2. TRYXALINI; 3. TRIGONOPTERYGINI; 4. ŒDIPODINI; 3. ACRIDINI; 6. XIPHOCERINI; 7. PHYMATINI (?). The three first of these groups belong to the *Conocephalides*, the two next to the *Orthocerides*, and the two last to the *Xiphocerides*.

The sub-family TETTIGINÆ is undivided, and consists of a single group, TETTIGINI.

A useful and concise Synoptical Table gives the leading characters of the author's sub-families and subordinate groups; and another Synoptical Table of the United States genera (p. 49), as well as an excellent plate containing seventeen figures, will give great assistance to the student of the American species. Pp. 55—245 are wholly occupied by scientific descriptions of species, genera, and other larger groups. This portion of the work is divided into two parts, the first treating of the Acrididæ of the United States (pp. 55—190); the second (pp. 195—245) of those of North America, not found in the United States. The number of genera characterised as North American is 45; that of species 227. In the United States (exclusive of *Tettiginæ*, which contain 3 genera and 12 species) are, at present known, 125 species of 25 genera; of the former, *forty*, and of the latter, *four* are described as new; and six others also are described as not hitherto known to be represented in that more restricted locality. At pp. 3—6 will be found a boon to the American orthopterologist in the shape of a list of those authorities which contain descriptions of the genera and species of Acrididæ belonging to the North American fauna. Another exceedingly useful feature of Dr. Thomas's work is a glossary at the end explaining the technical terms (to the number of upwards of 200) used in the scientific descriptions; the utility of this glossary will, no doubt, be felt far beyond the circle of students and collectors of the Acrididæ.

OUR BOOK SHELF

Daily Bulletin of Weather Reports, Signal Service, United States Army, taken at 7.35 A.M., 4.35 P.M., *and* 11 P.M. *Washington Mean Time, with the Synopses, Probabilities, and Facts for the Month of September* 1872. (Washington, Government Printing Office, 1873.)

THIS is a quarto volume of upwards of 180 pp., containing besides 90 weather-charts—three for each day of the month of September 1872. The volume is published for the purpose of showing the method of working of this division of the U.S. Signal Service, the "Division of Telegrams and Reports for the benefit of Commerce and Agriculture." The system appears to us to be thorough and careful, and calculated to lead to valuable scientific results in the department of meteorology. For each of the three daily times mentioned in the title, there is first a tabulated meteorological record from 72 stations in the United States and British N. America, showing the state of the barometer, thermometer, humidity, wind, clouds, rainfall, weather. This is followed by a weather-map constructed on the preceding record, on which, by clearly distinguishable marks, the state of the weather at all the stations is shown, whether clear, cloudy, snow, rain, &c., the direction and velocity of the wind, and the average elevation of the locations. Following this is a synopsis of the record, in which the general results of a comparison of the particular observations are briefly stated. This synopsis is succeeded by a statement of "probabilities," which are the deductions made from the conditions exhibited in the chart, considered in their sequence, as to the meteoric changes probably to follow within the twenty-four hours next ensuing. Then come the "facts" by which the "probabilities" may be tested, these facts being a classified statement of the state of the weather at the various stations at the next succeeding time of observation, with "general remarks" showing how far the probabilities have been realised. This is done, as we have said, three times every day of the month for which this Bulletin is published, and the value of the publication to students of meteorology is evident. "As a contribution of data, at least, to meteoric science," the introductory statement justly says, "and a demonstration that it needs only that governments should will and act through proper organisation to make meteoric knowledge of daily and practical use to the people, the publication must have its value." The Government of the United States deserves the highest credit for the wisdom it displays in perceiving what the true interests of the country are, and for its liberality in supporting a scientific department such as the one from which this Bulletin issues, whose business it is, by publishing the result of scientific research, to "benefit commerce and agriculture." By a patient pursuit of the system exhibited in this Bulletin, and by adopting what improvements may from time to time suggest themselves, we have no doubt that results of great value to science will follow.

The Treasury of Botany: a Popular Dictionary of the Vegetable Kingdom, with which is incorporated a Glossary of Botanical Terms. Edited by J. Lindley, M.D., and Thos. Moore; assisted by numerous contributors. New and revised edition, with Supplement. (London: Longmans, Green, and Co., 1874.)

THERE is no more difficult task than that of editing re-issues of scientific works published some years since. The progress of science is so rapid, the number of new facts accumulated year by year so enormous, that the most satisfactory and exhaustive treatise on any subject written by a specialist in that subject, becomes to a certain extent obsolete or imperfect in ten years. And yet, where can our scientific men be found with leisure to write or edit new dictionaries of science every ten years? The re-editing of old dictionaries seems, therefore, the inevitable alternative, though one attended with many disadvantages, which disadvantages are greatly increased when the objectionable plan has been adopted, as in the present case, of stereotyping the plates of the original work. The new facts can then only be placed before the reader in the form of a supplement, which may often seem at variance with the work itself, while errors or imperfect descriptions cannot fail to be reproduced. Lindley and Moore's "Treasury of Botany" was so admirable a work in its day, containing such an enormous mass of information, that a new edition must necessarily be welcome, although botanical science has made such rapid strides since its first publication in 1866; and the welcome will be more hearty when it is found that the new matter has been entrusted to such competent authorities as Dr. Masters, Prof. Thiselton-Dyer, Mr. Britten of the British Museum, Mr. Jackson of the Kew Museum, and the surviving editor. The only fault we have to find with the supplement is that it occupies five times too little space; under 100 pages out of 1,350 is clearly entirely insufficient for even a brief account of the main additions to botanical knowledge made during the last eight years. Had the new contributors been allowed a larger space, the book would have been a far more satisfactory one. It is to be regretted that at a time when so much attention is being paid to vegetable histology, a description of the vegetable cell should be republished without comment, not only so inadequate, but so misleading in our present state of knowledge, as the following:—"Cavities in the interior of a plant; the cells of tissue are those which form the interior of the elementary vesicles;" or that no description whatever should be given of the structure or mode of formation of starch-grains. As a dictionary of botanical nomenclature and classification the work is most ample; and on this ground only the "Treasury of Botany" is one which no botanical student can afford to be without.

A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Simultaneous Meteorological Observations

IT is doubtless familiar to most of your readers that at the Meteorological Congress at Vienna a proposal was adopted which was made by the War Department of the United States regarding the institution of a system of simultaneous daily observations all over the globe.

I have recently received the subjoined letter from the chief signal officer at Washington on the subject.

It may be of interest to your readers to know that invitations have been issued by this Office to a large number of observing meteorologists in the United Kingdom, on whose co-operation I considered I might count, and that I have received returns from sixty-one stations for the first fortnight of the year, and from sixty-four for the second, so that we may consider that the plan has met with general acceptance with the public.

I am ready to receive the names of any gentlemen who are willing to assist in the scheme, and who possess properly verified instruments, and shall be very happy to answer inquiries on the subject.

ROBERT H. SCOTT, Director

Meteorological Office, London, Feb. 17

"War Department, Washington, D.C.,
"Jan. 20, 1874

"SIR,—At the recent Meteorological Congress at Vienna a proposition was adopted to the effect that it is desirable that, with a view to their exchange, at least one uniform observation of such character as to be suitable for the preparation of synoptic charts be taken and recorded daily and simultaneously at as many stations as practicable throughout the world.

"The United States has an especial interest in reports and exchanges of this character, for the uses of the particular work in which it is engaged. It is hoped that when they are sufficiently extended, satisfactory solutions of many questions from time to time presenting themselves to this Office, and which now cannot be answered, will be arrived at.

"I have the honour, therefore, to request the establishment of a regular exchange between the Meteorological Office of which you are Director, and the Office of the Chief Signal Officer at

Washington, of uniform reports made from simultaneous observations taken daily at as many of the stations under your charge as it may be practicable for you to instruct or request to furnish such reports, or from other stations from which they may be voluntarily tendered, and of similar reports to be taken at the established stations of this Office throughout the United States. The reports to embrace, at least, pressure (reduced), temperature, wind, rain, relative humidity, and clouds, and to be made at 12.43 P.M., Greenwich mean time. The records to be printed or in manuscript, as you prefer, and to be mailed (as many of them as may be ready for exchange on the dates) in packages, on the 15th and last days of each month. Should circumstances render it inconvenient for your Office to furnish such reports without blanks for days on which they will necessarily fail to be taken, the records will be none the less gratefully received. Self-registered records will be very acceptable. In return exchange it is proposed to mail to your Office on the 15th and last days of each month the record of the simultaneous report prepared for that purpose in the form of which the enclosure herewith is a specimen for a single day.

"The data so to be exchanged are intended for any use either Office may wish to make of them.

"As an acknowledgment to those who may, upon your invitation, assist in a work so much wished for on the part of this Office, it is proposed to send to you monthly copies of the 'Official Monthly Weather Review,' with its Maps, for distribution to each of those so assisting, or other papers published by this Office, if so requested.

"In requesting this exchange as a part of a system to which it is hoped a very wide extension can be given, the Chief Signal Officer recurs with pleasure to the prompt encouragement received at your hands at the earlier steps for its adoption, and is gratified to announce that co-operation for similar exchanges of records, commencing on January 1, 1874, has been requested or is in progress with Prof. H. Wild, Director, Imperial Observatory, St. Petersburg; Prof. A. Coumbary, Director, Imperial Observatory, Constantinople, Turkey; Prof. Carl Jelinek, Director, Imperial Observatory, Vienna, Austria; Prof. Quetelet, Director, Meteorological Observatory, Brussels, Belgium; Prof. Buys Ballot, Director, Meteorological Institute of the Netherlands, Utrecht, Holland; and Prof. H. Mohn, Director, Meteorological Institute of Norway, Christiana, Norway. As time and its faculties will permit, this Office will seek additional aid. The advantages to accrue to the service in the United States are certain, and the hope is not unfounded that as the co-operation sought will be world-wide, so also will be the benefits resulting.—I am, &c.,

"ALBERT J. MYER
"R. H. Scott, Esq." "Brigadier-General, U.S.A.

Remuneration of the Contributors to Milne-Edwards' "Mission Scientifique au Mexique"

EN vous remerciant de l'envoi d'un article (vol. ix. p. 260) relatif aux singulières assertions contenus dans une note de M. Gray, je vous demanderai la permission d'ajouter que ni M. Duméril ni aucun des autres naturalistes qui prennent une part, soit directe, soit indirecte à la publication de l'ouvrage sur la Zoologie du Mexique ne recoivent pour ce travail une rémuneration pécuniaire quelconque. C'est gratuitement et dans l'intérêt de la Science seulement qu'ils s'en occupent; par consequent les renseignements fournis à mon estimable ami M. Gray, par je ne sais qui, sont faux.

Recevez, Monsieur, l'assurance de ma consideration tres distingué.

MILNE-EDWARDS

Paris, ce 13 fevr.

Membre de l'Institut de France, et
Associé Etranger de la Société
Royale de Londres

Animal Locomotion

IT is not my intention to go through the detailed proofs of the different statements in my review of his work to which Dr. Pettigrew objects, and which his letter of last week in no way falsifies, nor to show how he has quite missed the point of an observation of mine which he condemns as "utter nonsense," but simply to answer the question with which he ends his remarks. At first sight it might seem that the active dilatation of the heart during diastole did depend on an inherent power in the muscular fibres of the ventricles to elongate, but the peculiarities of the coronary circulation are quite sufficient to explain the phenomenon without the introduction of so unnecessary a theory as that of Dr. Pettigrew. For in the heart when removed from the body, as in the living body during diastole, the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur. This explanation was proposed by Brucke, and by myself some years later (Journal of Anat. and Phys.)

A. H. GARROD

WHILE admitting that Dr. Pettigrew appears to have made mistakes in his figures, and that he has not explained his views in the clearest manner, nevertheless it appears to me that, on the very important question of whether a bird's wing during onward flight moves downward and forward or downward and backward, he is right in asserting the former to be the fact.

The arguments of Mr. Garrod and Mr. Ward against this view seem to be founded on two assumptions—that the wing during its down-stroke is an inflexible plane, and that during its upward motion the quills open so perfectly that there is neither vertical nor horizontal resistance. But every feather of a wing is highly flexible towards its extremity, so that during the down-stroke the whole posterior margin of the wing must be curved up by the pressure of the air, thus forming a highly effective propelling surface owing to the rapid motion of this part of the wing. During the upward stroke the feathers open freely so as greatly to diminish, though not wholly to prevent, downward reaction; but the broad soft web of each quill will be bent down by the rapid escape of air between the quills, and this will necessarily give a forward motion, probably equal to that attained during the down-stroke, in which the small curved surface has a greater resistance and more rapid motion. If then the up- and the down-stroke both produce onward motion, the resultant of this motion will be in the direction of the mean position of the wings, which we may take to be about that of the body of the bird; but if the down-stroke were directed *backward* and the up-stroke *forward*, the resultant onward motion would be obliquely downward, and this downward angle of motion would tend to be so much increased by the continual gravitation of the body that the surplus vertical reaction of the down-stroke over the up-stroke would not be able to overcome it. A slight upward angle of the mean position of the wing-plane seems therefore to be essential to secure horizontal forward motion as a general resultant of the upward and downward action of the wings under the influence of gravitation; and to Dr. Pettigrew belongs the merit of showing that this is one of the most important characteristics of the flight of birds, and, probably in a still greater degree, of that of insects. A bird's wing is a highly complex apparatus, subject to a variety of flexures and motions in every feather; and it is only by a careful consideration of the action of the resisting medium on these variously curved elastic surfaces, both during the upward and downward motion of the wings, that we can arrive at any definite notion of their supporting and propelling effect. The experiments of Prof. Marey do not seem to contradict the theory of Dr. Pettigrew, as far as I can make out from an abstract of these given in the "Ibis" for 1870, p. 267; though, as his apparatus only gave the motion of the wing relatively to the body of the bird, they are not of very much value in determining the absolute angular position of the wings, which is what we want to arrive at. The highly-inclined position of a hovering bird is more to the point, as any less degree of inclination would lead to onward motion.

ALFRED R. WALLACE

On the Variability of the Node in Organ-pipes

THE variability of the node is an unrecognised phrase. Something similar in kind relating to the node will be remembered as having been mentioned by scientific writers in a cursory manner, then set aside as evidence of too doubtful interpretation to call for more extended comment.

From the time of Savart it has been known that the nodal division of the open organ-pipe does not take place at the exact half of the length, that the half nearest the embouchure is the shorter of these "unequal halves"—a contradictory term apologised for yet sanctioned, I believe, by the late Prof. Donkin.

The displacement of the node is perhaps the most significant fact that in the natural history of organ-pipes presents itself to the attention of the investigator, be he student or teacher. Why it should have been passed by as though its meaning were not worth wrestling for is incomprehensible. Since Savart wrote no light has been thrown on this singular phenomenon, for the explanation which has been afforded (presently to be quoted), cannot be called in any degree satisfactory. In the illustrations of nodal division given in various scientific works there is a puzzling contrariety hardly to be accounted for except on the supposition that our engravers are as niggardly conservative in design as the buried Egyptians, or that the engravings themselves are the cherished heirlooms of our publishers. In one work a representation of the manometrical nodal division after Kœnig will be given, but *carefully* corrected and revised by the aid of a pair of compasses; in another a beautifully accurate copy of the original, so lopped to suit the size of the page that much mental effort and distortion of reasoning are incumbent on the reader in vain attempts to bring the engraving into harmony with the accompanying text, young eyes are mystified, it needs cold "well-worn eyes" to appreciate these fine economics of the publishing art; in another the manometrical nodal positions will be properly defined and, by some negligent inconsistency, on an opposite page an organ-pipe be depicted, admirably exact to theoretical localisation, in direct contradiction of knowledge. Faults of this kind should not be allowed to pass, they weaken faith in the teacher and are harassing to the inquirer.

Kœnig in his own illustrations represents the displacement of the node as it is indicated under experiment, for this one condition of truth to nature had been too often before him in his manometric-flames to allow of his disregarding its faithful portraiture. The difference he shows to exist as to position corresponds very closely with that we arrive at by other means, by calculation of scales and by the practical teachings of experimental study of the relations and arrangements of organ-pipes. Of the cause of the displacement Kœnig offers no elucidation.

The following explanation is quoted from Prof. Airy's treatise on "Sound and Atmospheric Vibrations." In the section on open organ-pipes he says:—

"It was found by Mr. Hopkins that the node next the open mouth of the pipe was somewhat less distant from it than that given by theory, or, which amounts to the same thing, that the place where the air has always the same density as the external air is not exactly at the pipe's mouth but somewhat exterior to it."

The extent of the disparity would be but very imperfectly comprehended under this vague delineation. Other authors have attempted explanation, in substance the same as the above, to account for the disparity; the summary of the whole is, that science brings forward no better plea than the surmise of a probable place, somewhat exterior to the mouth, which the air-wave of the *lower half* of the pipe has to attain before it can be properly said to be completed in length. Truly an illogical conclusion if this line of reasoning is carried out. In common fairness the *upper half* of the pipe may claim to be credited with a reasonable amount of wave-prolongation, seeing that at the higher orifice the internal column of air pulsates the atmosphere with far greater vigour than at the mouth, and consequently that for a similar attainment of density the due addition of wave-length would only serve to increase the disparity in relation to the *half below* the node.

A displacement of some sort thus receives acknowledgment although as yet the variability of the node is unsuspected.

The actual extent of the disparity between the "unequal halves" can be ascertained. It is subject to laws of relation of as definite a character as are found in other dynamical problems when the elements of calculation are delicately defined. An approximate estimate will be sufficient for the present purpose. For avoidance of the inconvenient "unequal halves" it will be permitted me to coin two simple terms as more distinctively representative, and to speak of them as super-nodal and sub-nodal.

If a standard open diapason pipe be made for some designed pitch, whatever that pitch may be, it may safely be predicted that the pipe will stand considerably short of the full theoretical length; æsthetically judged for musical quality, it ought to be about one-eighth less, a difference much affecting the veracity of scientific argument.

Doubtless it would be somewhat a novelty for a scientific lecturer to tell his audience that one-eighth of the whole wave-length was lost by conversion into organ-pipe vibrations, yet, unless he innocently accepts the ironical reply of Galileo on the pump question, that "perhaps Nature is indifferent to a few feet," he is strictly in this dilemma: if the pipe is a natural standard of wave-length, the velocity of sound in air computed on the basis of the pipe's length falls very far short indeed of the philosophical estimate, 1,125 feet per second; on this ruling the latter should be pronounced to be irreconcileably wrong, *or else* the frank admission made that there is no "necessity of relation" that the wave-length in an organ-pipe, giving a defined pitch, and the wave-length in the free air corresponding to that note should be identical.

Taking the several classes of pipes, from the Diapason to the Vox Angelica, ranging from the pipes of the most vigorous to those of the softest intonation, the amount of difference from full measure varies from one-eighth to one-twelfth *less* than that which theory demands. The loss is mainly due to the cause which enforces nodal displacement.

Our immediate inquiry is, what is the extent of displacement of the node, and what its variability? Divide the length of the already reduced pipe into seven equal parts, and the unequal halves will be in the ratio of 4 to 3 Four parts belonging to the super-nodal half, and three parts to the sub-nodal half, subject to a relative variability, according to the position of the pipe in the range of octaves, and subject to a fluctuating variability determined by force of wind, diameter of pipe, character of scale, relative size of mouth, mode of voicing, and other details, changing the proportion, perhaps, to 6 : 5, or even to 7 : 6. Whatever the extent of the variability, change in result rigidly follows change in details, with a calculable value. When, instead of the fundamental note, the pipe vibrates in harmonic nodal divisions, the lowest half-segment takes upon itself almost the whole difference, and not merely a proportional share in comparison with its segmental relation to the whole pipe. A remarkable fact, but one fully accounted for in that which I have termed the aëro-plastic reed theory (NATURE, vol. viii. p. 25) for it is easy to me visibly to demonstrate that the harmonic-independent and the harmonic-concomitant are originated in the pipe by totally different natural processes.

The nodal difference detected by Mr. W. Hopkins was much smaller in extent, but there is an important distinction not to be overlooked: his experiments (recorded in the Transactions of the Cambridge Philosophical Society, vol. 5) were not made with organ-pipes, but with glass tubes supported in position over a glass plate, the plate being set in vibration by friction. He expressly rejected organ-pipes by reason of their intractibility and of the difficulty of obtaining results from them of the nature desired.

In like manner we continually find experimentalists rejecting organ-pipes as insubordinate pupils; they prefer dumb pipes and the artificial speech by tuning-forks, and having obtained such negative evidence, make a clean transfer of their conclusions to all argumentative reasonings and expositions of the nature and functions of the original, living, speaking organ-pipes. The Hon. M. Strutt, in his paper on the Theory of Resonance, printed in Phil. Trans. Nov. 1870, says:—

"Independently of these difficulties, the theory of pipes or other resonators made to speak by a stream of air directed against a sharp edge is not sufficiently understood to make this method of investigation satisfactory. For this reason I have entirely abandoned this method of causing the resonators to speak in my experiments, and have relied on other indications to fix the pitch."

Prof. Airy is as evidently dissatisfied with the state of theory and experiment, using such phrases as these: "the matter, however, demands more complete explanation;" "that obscure subject, the production of musical vibrations in a pipe by a simple blast of air;" "possibly when the mathematical calculus is farther advanced, this may be shown," &c. Beyond the province of mathematical analysis his survey is keen, and with foresight of the results of possible experiments.

At the present date our best authorities are in effect repeating the assertion of Biot that "the particular properties of the vibrations of confined air in tubes are not yet sufficiently explained." The disturbing influence of some unknown agency may be discerned in Dulong's experiments of filling organ-pipes with various gases, and estimating the velocity of sound in these gases by the pitch produced. Similar experiments on this method are referred to by Herschel, and he, noticing how the results gave for hydrogen gas a velocity differing by one fourth from that which theoretically had been calculated, could only account for it by supposing an impurity in the gas used for the experiments. There is little need to resort to the supposition of an impurity; it is quite sufficient to know that an agreement in length of organ-pipe and aerial wave-length was assumed which does not exist, and that, moreover, the mechanical nature of the organ-pipe, and its delicate apparatus so wonderfully balanced for the attainment of its ends, had escaped observation. The admirable method of experiment for ascertaining the velocity of sound in gases, devised by M. Kundt, by means of glass tubes and lycopodium seed, is free from the same source of error; and, as might be anticipated, comparison shows a marked difference in estimates. In respect of carbonic acid gas and hydrogen gas, for instance, Dulong differs from Kundt, his estimate in the one case being less by one-fifth of the whole, and the other more by one-fourth; the divergence interprets itself, indicating the relation of their densities to the compelling force, the unseen mechanical action at the mouth of the organ-pipe. This will be clear when the "air-moulded reed" is fully understood in its nature and functions. When the magnetism of the earth is perceived, the dip of the needle to the north or south of the equator in accord with its localisation is explained.

The confession of "obscurity" amounts to a concession that the old theory has been found wanting, that it is inadequate to deal with facts. Whether in dealing with the larger questions here brought into discussion, or with the simpler class, the mere modifications of structure, it is equally incapable. If, for instance, a stopped pipe is pierced through the stopper and a short open pipe inserted, say a third or fourth the diameter and a third or fourth the length, what will be the effect of this on the pitch? The old theory would reply, the added length would cause a flattening of pitch, and then will come a proviso for safety's sake, that if the change converted it into an open organ pipe then the pitch would be raised in accordance with the open length. We go to Nature for her say in the matter, and find that the pitch is raised not flattened, and that the extent is about a quarter of a tone, and that *further lengthening* of the smaller pipe takes back the pitch again just its quarter tone. If another stopped pipe is drilled at the back with a hole of a diameter a third or fourth of that of the pipe, but so that it shall be at a higher level than the lip or edge of the mouth, in effect shortening the air column by admission of external air at a higher point, what will be the result? On the old theory we should expect the pitch to be higher in consequence. Appealing to the ear we know that, on the contrary, it is flattened. These results cease to be anomalies when viewed under the new theory, and indeed they would be predicted with confidence as the necessary outcome of the conditions.

The proposition that in an organ-pipe there is no constant wave-length for an ascertained pitch, will no doubt be discountenanced as novel and revolutionary, but it is true and will have to be acknowledged. A further proposition that in an open organ-pipe there are three different velocities speeding at different rates, concurring in every vibration, and essential to the synchronic time of its note, has a still more aggressive aspect defiant of law. Not so. It is because law—"known law," does not cover the facts, is unstable in its applications, and is deficient in prevision, that there is room for new hypothesis which does not play fast and loose with nature; the utmost exactitude of length in an organ-pipe is as indispensable in this as in the older theory, but the relation is one of proportion to a system, and the least and every variation will make imperative suitable or corresponding modifications in other portions of the structure. Only a whistle, yet with more to marvel at for delicacy of organisation and beauty of adaptation "than is dreamt of in philosophy."

As regards "fixity of wave-length," that characteristic reappears in a new relation, and we shall find that, allowing for retardation by friction, the super-nodal half-wave of the pipe corresponds very closely with length in atmosphere. The cause of the displacement of the node is involved in the physical action taking place at the mouth of organ pipes, the consideration of which is reserved for a further communication.

HERMANN SMITH

Auroral Display

As a few remarks on the aurora of the 4th may be of interest to some of your meteorological readers I append the following notes:—

At 6.15 P.M. on Wednesday, the 4th inst., an aurora commenced in the northern part of the sky which gradually went down towards the south.

7.15.—Semicircle from W. to E., streamers shooting up from it.

7.25.—Light more diffused, a few streamers at N W.

7.30.—A semicircle of diffused light from W S.W to E

7.35.—Bright line of light from W S.W to E.; no streamers

7.40.—A very faint irregular line of light from W.S.W. to E.

7.45.—Diffused light.

7.50.—Same as at 7.45.

7.55.—Streamers shooting down from zenith all round. Very fine

8.—Bright at N.N.E. Streamers N and N N.E. A sharp S.E. breeze.

8.5.—Bright light at N.W. No streamers.

8.10.—Streamers at N.E

8.15.—Streamers at S S.E.

9.—No aurora perceptible.

From the above, we note one peculiarity, namely, that the aurora was chiefly in W. + E. or W.S W. and S S E.

WILLIAM HY. WATSON

Braystones, near Whitehaven, Feb 9

[We have received letters concerning this aurora from several other parts of the country.]

THE PHOTOGRAPHIC SOCIETY

ALTHOUGH we published last week a letter from Mr. Baden Pritchard, Hon. Sec. of the Photographic Society, impugning the justice or accuracy of our strictures on that Society, our esteemed correspondent has not caused us to change our opinion.

We have now before us the Journal of the Society for the past year (a summer vacation of three months excepted), and certainly it furnishes *primâ facie* evidence of the most apathetic and inefficient condition which is consistent with continuous existence. The numbers contain eight pages each, the page little more than half the size of that of NATURE, and in the whole year's proceedings there are twelve pages devoted to science, half of this being a lecture by Prof. Stokes; three or four papers of considerable value on technical points of photographic interest, and much which the charity of any semi-learned society would be largely strained in giving paper and ink to.

There is no mention of scientific or other committees, no provision for them in the laws, no reports of investigations made or to be made, no notice of scientific discovery abroad or recognition of discovery at home. Mr. Pritchard has no need to assure us that the body "does not profess to be a purely scientific one"—the scientific element in it, so far as its own record shows, is purely fortuitous.

But without demanding scientific labours from a body not "purely scientific," we do not even find evidence of common activity in the research of practical problems, and if any of its members are, as Mr. Pritchard suggests, engaged in researches on the process and nature of film best suited for transit of Venus observations, they have not had faith enough in the countenance of their Society to place their labours before it, or ask its assistance in performing them.

Since our article appeared, the revolution alluded to has taken place, and that part of the Society in favour of reform having a majority at the meeting appointed for the discussion of the question, have carried an amendment to the laws providing that henceforward the Society at large shall select its council, and that the majority of the actual council shall not have the power to select for retirement such members as it sees fit and to decide who shall replace them, as has actually been the case hitherto; it has also been decided that the presidency shall rotate. These measures were, as we learn from the photographic papers, strongly opposed by the council, and upon being carried by a majority of 30 to 23 (the council itself voting in the minority) the entire body resigned.

As the meeting at which this stroke of singular policy was made, was that for the election of the new members of council, these were enabled to assume the reins of government and prevent the, otherwise inevitable, total dissolution of the Society. And now that the reformers have its affairs in their own hands, it is to be hoped that it will begin a new life of efficiency, and, remembering that it owes the cause of its existence to the labours of scientific men, give its most efficient aid to those scientific researches in which it has become an important element of investigation, as well as to those of a more technical nature which have given photography so great a commercial and industrial value. And on the other hand we bespeak for it the aid and countenance of all scientific men whose researches are in any way dependent on photography, and give it, in its reformation, our best wishes for that complete success and efficiency which will make it as useful to Science as honourable to itself and its members.

NOTES FROM THE "CHALLENGER"

THE following contributions to the literature of the *Challenger* Expedition appear in the *Cape Monthly*. The first contribution consists of a few notes from Commander Maclear, written on the day of the *Challenger's* departure from Simon's Bay, and will give our readers an idea of the work still before the Expedition :—

On leaving Simon's Bay, if the weather permits, dredgings and temperature soundings will be taken on the Agulhas bank; then sail made for Marion Island. This and the Crozetts will be examined ; the last may be occupied by the French as an observing station for the Transit of Venus. Then for Kerguelen Island. It is not likely that the weather will allow a regular series of soundings to be taken as hitherto, but some doubtless will be taken on the passage.

Kerguelen's, or Island of Desolation, will be a fertile field of exploration in every department of science, and as it is to be one of the stations for watching the Transit of Venus, special information will be collected for the use of the astronomers who will go there towards the close of next [this] year. The longitude of the island will be determined by chronometrical measurement from the Cape, and again to Melbourne, and with the great number of chronometers (16) that the *Challenger* has on board, the longitude should be determined very accurately.

After leaving Kerguelen, Macdonald Island will be examined, and search made for a harbour there; and then a stretch will be made to the Ice Barrier. The investigations in the neighbourhood of the ice are very important, but great care will have to be taken not to get entangled in the ice. With steam power, and the clear weather there is likely to be in February, little danger need be apprehended. If the season should be fine, some considerable time will be occupied in this region, but if not, after a short stay, sail will be made for Melbourne, which will probably be reached in the end of March. After a few days there, to obtain the rates of the chronometers, we go on to Sydney to refit and, if necessary, dock. This terminates the second stage of our voyage.

Leaving Sydney about the middle of May 1874, and carrying a line of soundings to New Zealand, we next examine the islands about the Coral Sea and Torres Straits in August 1874: New Caledonia, New Guinea, Arofura Sea, Kaepang in Timor, Java Sea, Macassar, Celebes, and reach Manilla in November. We next look up the doubtful islands of the Western Pacific; visit New Ireland, the Solomon Islands, and Pellew, and Japan will be reached in March 1875. From Japan we cross to Vancouver's, and then to Valparaiso, examining Eastern Island and Sulay group in our course. Leaving Valparaiso in the end of 1875, we go through the Straits of Magellan to Falkland Isles, Rio de Janeiro, Ascension, and England in the middle of 1876.

The other communication, of a different order, comes from a gallant Blue Jacket, who speaks for himself and the *Challengers* and their labours somewhat irreverently thus :—

FROM JACK SKYLIGHT TO HIS OLD SHIPMATE

A Letter without much Rhyme and with a little Reason

We've crossed the Line a many times in craft both great and small,
And of them 'ere fish that's thereabouts I've caught 'em nearly all.
It aint becos I wants to boast I says as "it is so,"
But 'cos I think that wot is wot I'm just the bloke to know.
I'll first acquaint you, topmate, with the natur of my dooty,
And show you what I've larned since last we met, my beauty.
I jined this craft last winter, got rated on her ledger
A swabber, jobber, scrubber, a sounder, and a druger.
I know, old ship, when this you see you'll say I'm flyin' hi,
But it's true as Polly-Arris is above us in the sky.
At sea we sounds—no matter, Bill, if every blessed thread
Aloft or low of canvas before the wind is spread,
In it comes ! And down there goes, I've really quite forgotten
How many fathoms (half-inch), Bill, until we touches bottom.
Sometimes the timmey-noggie that holds the weights don't G
And then a fog* arises as is horrible to see.
We flies in all directions, like cats on houses sportin',
The luff cries out, the donkey shies, and makes a dreadful snortin'—
It aint a regular ass, Bill, but one of them inventions
They puts aboard a man-of-war with various intentions,
To wit, it nicks the complement, and gives the honest Jacks
More time to study politics and read their Sunday tracks.
The donkey does the hauling in, which is no doubt a blessin',
For if it had to come by hand, oh ! lord, 'twould be distressin'.

We've a many curious ratins, a lot of long shore tallies
For scientifick genelmen, their servants, and their valleys.
Don't yer see these learned bosses have come to search the ocean,
But for what, old son, 'twixt you and I, I'm blow'd if I've a notion.
I've 'eard 'em talk of Artic drift and walleys under water,
And specs next week to find they've nab'd old Davy and his darter.
Of course you know they've got to find the link atween the species,
Some say as there's a coon aboard as liks it all to pieces ;
I cannot tell, for well you know it aint the likes o' me
That's got a chance like swells abaft the curus sight to see.
The scientifick swells, old chap, are mad on mud, and great
On getting things like what we used in Chiney for our bait.
You know them squids and stuff we tried for catching them there conger?
Well, it's the same; but then the name is many a fathom longer.
They seems to me to make a deal and show a great surprise
At things we've seen, Bill, many times, when first they meet their eyes.
Perhaps its 'cos the thing's alive their fancies somewhat tickle,
They only having seen them home screwed up in brine or pickle.

I've told yer how we sounded, now I'll tell yer how we druge,
And if my life's a angel's I'll leave yer for to judge.
We hangs the drege at the yard-arm to a sort o' kind of buffer—
At explanation, Bill, yer no I always was a duffer—
It aint a bad doge neither ; for when its pulled it streches
And gives a kind of surge when the dredge at summat kotches ;
It's like a koncertina, Bill, but where the wind is squoze,
From end to end a set of stays like Inde rubber goes :
A block is tacked at bottom and through it runs the line—
Which is the werry bane of life to this old pal of thine ;
I've burnt my hands, I've spiled my close, I torn my underneath,
I bark'd my shins and nik'd my back, and loosened all my teeth—
All through that blessed line, Bill, which, trifling as it seems,
Is wuss nor all the nightmares that ever hunts in dreams.
The care that is required for to keep that line from breakin'
If your stationed near the donkey is a awful undertakin' ;
The thing flies thro' your fingers, and if stationed near the drum,
Its safe to nab you somehow by a finger or a thumb ;
Then there's the pipe and others, Bill, that raise a shout, and call

---

* Row.

Till you'd almost think they'd been and caught the devil in the trawl;
The trawl's for fancy drugin' and the work's about the same,
The only diff'rance I can see is that wot's in the name.

A scientifick genelman, our Genius on the cruise,
Explained to us the hanimals, their habits, and their use;
I don't tumble to it much; but, Bill, he spun a yarn
About the objeck of the cruise which I was glad to larn.
He said 'twas for the good of man to raise him summat higher,
Since it was proved by some one that a monkey was his sire;
I don't see how it follers—but he sed from wat he found
There was fields of blazing sea weed below upon the ground;
And every little blessed thing we druge out of the sea
Was for the good of all mankind, including u and me.
He likewise said, and bid us all partikilarly remark,
That at the bottom also 'twas most exceedin' dark,
Cause from twenty million fathoms once we got a curus prize
(He didn't want 'em in the dark) a fish with many eyes.
He told us that we'd all be dooks when this 'ere cruise is done;
I think he was mistaken, or he meant he would be one.

Thare goes the pipe, my hearty; so I'll no more at present write
But ax you to believe yours most faithful

JACK SKYLIGHT

THE COMMON FROG*

X.

The Nervous System of the Frog.

THE nervous system consists of the brain, spinal marrow, and nerves.

The whole consists of a soft, white substance, ultimately composed of minute threads, termed *nerve-fibres*, and minute round bodies called "ganglionic corpuscles."

The brain is contained in the cavity of the skull, and consists of a rounded mass made up of corpuscles and fibres, and itself contains a cavity which is a remnant of the original canal formed by the upgrowth and overclosure of the walls of the primitive groove of the embryo.

The spinal marrow (as has been said earlier), traverses the canal formed by the successive neural arches of the vertebræ being directly continuous with the brain which it, as it were, continues on down the back. Like the brain, it is largely composed of corpuscles, as well as fibres, and itself contains a longitudinal cavity (continuous with that in the brain), which is also the ultimate condition of the canal formed from the primitive embryonic groove.

The nerves generally (which are made up of fibres) proceed forth from the brain and spinal marrow, which therefore are called the *central*, or (from their position along the dorsal axis of the body), the *axial* portion of the nervous system.

All the nerves which so proceed together constitute what is called the *peripheral*, or (because going to the limbs which are appendages of the trunk), the *appendicular* portion of the nervous system.

From the brain proceed the nerves of special sense: a pair, one on each side, going to the nostrils (1, the *olfactory nerves*), another pair going to the eyes (2, the *optic nerves*), and a third pair going to the ears within the skull (3, the *auditory nerves*). Other nerves go to the tongue and palate, ministering to taste, and again others to the little muscles (orbital muscles), which move the eyeball in various directions, and to different parts of the face.

The nerves which come forth from the spinal marrow are called spinal nerves. They proceed out in pairs (one on each side), and are distributed to the limbs and trunk.

Each nerve consists of fibres, of the sorts proceeding respectively from the ventral (in man anterior), and the dorsal (in man posterior) aspects of the spinal marrow. But these two kinds of fibres are distributed side by side in the ramifications and distributions of each nerve.

The fibres which come ultimately from the dorsal aspect of the spinal marrow are those which carry inwards the effect of a stimulus applied towards their ultimate termination, and are therefore called *afferent*, or *sensory*.

The fibres which come ultimately from the ventral aspect of the spinal marrow, are those which carry an influence outwards, and produce a contraction in the muscles, and are therefore called *efferent* or *motor*.

It is the nervous system of the Frog, rather than any other set of its organs, which has especially excited interest and attention. It is especially to the relations *inter se*, of the parts of this system that inquiry has been directed. The relations, that is, of its central or axial portion (the brain and spinal column) to its peripheral or appendicular portion (the nerves of the body and limbs).

In the ever memorable year 1789, Galvani accidentally discovered in the separated legs of certain Frogs, prepared for broth, those motions produced by irritation of the exposed great nerve of the thigh, now so familiar to most. This action was long called galvanism, after this observer, not, however, that he was absolutely the first to notice a fact of which he was but a re-discoverer—Swammerdam as long ago as 1658 having observed such motions.

They are generally considered as demonstrating the purely "reflex action" of the nervous system—the responsive action, that is, upon muscles, of nervous centres acted on by external stimuli without the intervention of sensation.

It is affirmed that not only will a decapitated frog endeavour to remove an irritating instrument by means of its hind legs and feet; but that if a caustic fluid be applied to a spot easily reached by one foot, the decapitated frog will apply that foot to the spot. More than this, if that foot be cut off it will move the stump as before, seeking to reach the spot, and failing so to do, will then apply the other foot to the irritated locality.

These, and such experiments, are of course conclusive, if the common assumption be conceded that the brain is the indispensable nervous instrument of sensation.

It may be, however, that the faculty of sensation may be subserved by the spinal cord without the brain, and if so, all these much vaunted experiments are valueless as regards the proof of pure reflex action, not but that they are of extreme interest, as showing what may be done in lower animals without the intervention of any b action whatever.

Mr. G. H. Lewes has long contended agai
bution of sensation to the brain exclusively, a
tian has recently supported and enforced simi

The latter remarks in his "Beginnings of
stead of accepting the popular view, that the
organ of mind, I believe it would be nearer the
upon the whole nervous system as the organ

Dr. Bastian here uses the word "mind," no
a rational intellect but as a generic term
psychical activity.

It may be remarked in passing that t
Messrs. Lewes and Bastian closely approx
they go, to that most rational belief that th
creature is whole and entire in every at
structure so long as the latter preserves
vital activity.

The brain of the frog consists of the s
as does the brain of all the vertebrate
man. In the form and in the proport
however, it differs extremely from the
above all from man) and resembles t
brain of the frog (and of Batrachians
much closer resemblance to that of
a mammal.

The brain of man consists of the
parts:

1. A pair (one on each side) o
each connected, by a long stalk,

---

* Continued from p. 266.

and each shaped somewhat like a life preserver. These are the "*olfactory* lobes," and from the swollen head of each proceed the delicate nerves of smell.

2. An enormous pair of folded masses which form the great bulk of the human brain and are called the *cerebral lobes* or hemispheres. These are so large and preponderant in man, as to hide every other part of the human brain when that organ is viewed from above.

3. A relatively very small portion, but one easily recognised since it supports two conspicuous little bodies. One of these (Figs. 69, 70, 71, *pl*) is called the *pineal gland*, and projects more or less upwards; the other (Figs. 69, 70, 71, *pt*) projects downwards and is called the *pituitary body*.

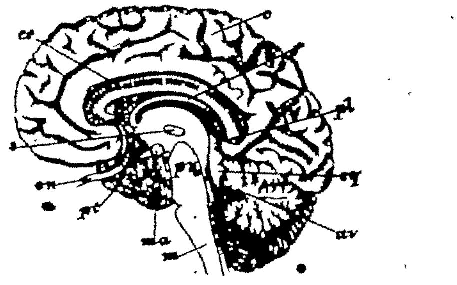
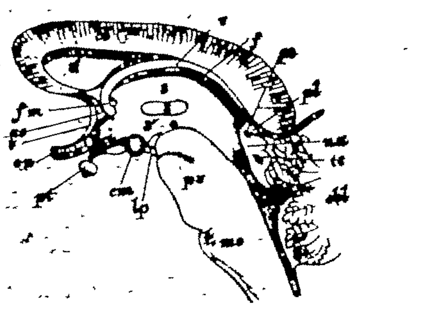


FIG. 69.—The Brain as seen when a Vertical Longitudinal Section has been made through its middle. *Av*, arbor vitæ of the cerebellum; *c*, cerebrum; *cc*, corpus callosum; *cq*, corpora quadrigemina; *f*, fornix (between the fornix and the corpus callosum is the septum lucidum); *m*, medulla oblongata; *ma*, corpus mammillare; *on*, optic nerve; *pl*, pineal gland; *pt*, pituitary body; *pv*, pons Varolii; *s*, soft, or middle commissure.

4. An also very small portion relatively, is distinguished by bearing certain small prominences (Fig. 69, *cq*, and Fig. 70, *na* and *ts*) placed behind the pineal gland, and called *corpora quadrigemina*.

5. A rounded mass of finely folded brain-substance, placed at the lower part of the back of the head beneath the hinder portion of the cerebral hemispheres. This is



—Enlarged and Diagrammatic View of a Vertical Section carried ugh the Corpus Callosum and the parts below. *ac*, anterior com- ure; *cc*, corpus callosum; *cbl*, cerebellum; *cm*, corpus mammillare; nix; *fm*, foramen of Monro; *i*, infundibulum; *lp*, locus; perfora- media, *mo*, medulla oblongata; *na*, nates, *on*, optic nerve; *pc*, rior commissure; *pv*, pons Varolii; *pl*, pineal gland; *pt*, pituitary ; *s*, soft, or middle commissure; *sl*, septum lucidum; *t*, lamina alis; *ts*, testes; *v*, velum interpositum (between it and the fornix is enclosed by the folding over of the cerebrum upon the roof of rd ventricle), 3, upper, and 3', lower part of third ventricle; ventricle—between them is the *iter a tertio ad quartum ven-* .

cerebellum, and when cut through exhibits singular, tree-like markings, due to the infoldings ce of the organ, and called the *arbor vitæ* (Fig.

rt which directly continues the brain into the [illegible] (Fig. 71, *m*). It is overlapped by the cere- [illegible] of the remnant of the [illegible] the fourth ven- [illegible] rain is

On turning to the brain of the frog from that of man it is at first sight difficult to find out the resemblances, and to determine which portions of the one answer to definite regions of the other.

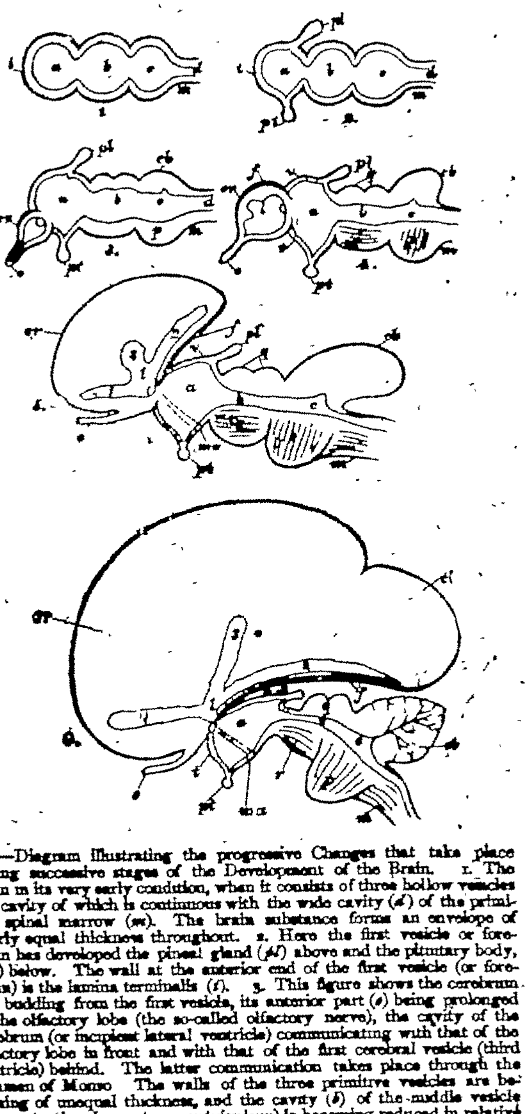


FIG. 71.—Diagram Illustrating the progressive Changes that take place during successive stages of the Development of the Brain. 1. The brain in its very early condition, when it consists of three hollow vesicles the cavity of which is continuous with the wide cavity (*d*) of the primitive spinal marrow (*m*). The brain substance forms an envelope of nearly equal thickness throughout. 2. Here the first vesicle or fore-brain has developed the pineal gland (*pl*) above and the pituitary body, (*pt*) below. The wall at the anterior end of the first vesicle (or fore-brain) is the lamina terminalis (*t*). 3. This figure shows the cerebrum (*cr*) budding from the first vesicle, its anterior part (*o*) being prolonged as the olfactory lobe (the so-called olfactory nerve), the cavity of the cerebrum (or incipient lateral ventricle) communicating with that of the olfactory lobe in front and with that of the first cerebral vesicle (third ventricle) behind. The latter communication takes place through the foramen of Monro The walls of the three primitive vesicles are becoming of unequal thickness, and the cavity (*b*) of the middle vesicle (*iter a tertio ad quartum ventriculum*) is becoming reduced in relative size 4. The cerebrum is here enlarged, and the inequality in thickness of the wall of the primitive vesicle is increased The thickened upper part of the wall of the cerebrum is the fornix (*f*). 5. This figure shows the cerebrum still more enlarged, and with a triradiate cavity (*l*, 1, 2, 3). The fornix has now come to look slightly downwards, dotted lines, indicate the downward extension of its anterior part, into the corpora mammillaria. 6. Here the cerebrum is still more enlarged and backwardly extended The fornix is shown bordering the descending cornu and extending into the temporal lobe (*tl*) of the cerebrum, which lobe is destined to descend (when the brain is fully developed) so much more that it comes to advance forwards The fornix borders the margin of the very thin outer wall of the descending cornu, which when torn forms the fissure of Bichat. The bending back of the cerebrum has now almost enclosed (between the fornix and the velum) the space (*x*) which in Fig. 4 is widely open, making what is morphologically called the outside of the brain come practically to be in its very centre. *a*, fore-brain; *b*, mid-brain; *c*, hind-brain; *cb*, cerebellum; *cr*, cerebrum; *d*, cavity of the medulla; *f*, fornix; *l*, lateral ventricle; *m*, medulla oblongata; *ma*, corpora mammillaria; *o*, olfactory lobe; *p*, pons Varolii; *pl*, pineal gland; *pt*, pituitary body; *q*, corpora quadrigemina; *r*, crura cerebri; *t*, lamina terminalis; *tl*, temporal lobe of the cerebrum; *x*, space, enclosed by the extension backwards of the cerebrum; 1, anterior cornu of lateral ventricle; 2, its middle or descending cornu; 3, its posterior cornu.

The earliest conditions of the human brain the resemblance much more marked and obvious; it is later that the correspondence between the brain of the frog and that of man becomes so disguised through the unequal growth of different portions of the organ in the human brain as it advances in its growth and development. The same six successive portions, however, exist in each.

1. In the frog the olfactory lobes acquire a much larger relative size, and they retain permanently an internal cavity which exists only transitorily in man.

2. The cerebral lobes (or hemispheres) exceed those just noticed but are insignificant indeed, when compared with the corresponding human structures. They may, however, be more insignificant than in the frog, as, for example, in the lamprey, where they are actually smaller than the olfactory lobes. In that the cerebral lobes of the frog each contain a cavity (the lateral ventricles) they have a character which is constant in all animals above fishes, they open by a common aperture (foramen of Monro) into the cavity of the next brain segment behind.

3. This third segment retains a great relative magnitude compared with that of man.

4. The fourth segment, however, consisting of the optic lobes, attains a still further relative development, though consisting only of two bodies instead of four, but these contain a cavity not found in the corpora quadrigemina of the human brain.

5. The fifth segment, the cerebellum, is very small, and smaller than the same part in animals both higher and lower in the scale; indeed, in the frog class, this organ may be said to be at its minimum. When cut it exhibits no trace of an *arbor vitæ*.

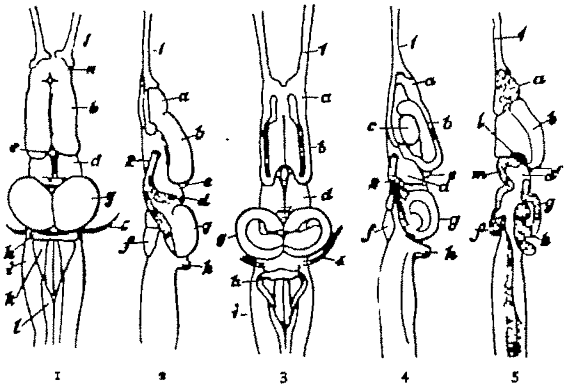


FIG 72.—Brain of Bull Frog in various views 1, Dorsal view 2, Lateral view; 3, Transverse horizontal section showing the cavities of the olfactory cerebral and optic lobes 4, Longitudinal section a little to the left of the median line. 5, Longitudinal section in median line. The corpus striatum, *c*, is here exposed to view and also a body, *g*, within the optic lobes 5, Longitudinal section in median line. In all five figures:—1, Olfactory nerve; 2, optic nerve; 4, auditory nerve; *a*, olfactory lobe, *b*, cerebral lobe, *c*, corpus striatum; *d*, optic thalamus; *e*, pineal gland; *f*, pituitary body; *g*, optic lobes, *h*, cerebellum.

This fact has a special interest as bearing on alleged functions of this portion of the brain.

It has been asserted by some that the cerebellum ministers to the sexual functions, by others that this part co-ordinates and directs locomotive movements, and, quite lately that it is related to movements of the eyes.

The first two of these hypotheses seem to be completely overthrown by our frog. In the first matter there is anything but a deficiency of energy and activity, and as to the second, many reptiles are less active and continuous than the frog in their locomotive efforts. As to the third hypothesis, it should be remembered that the eyes of the Frog are large and very moveable, as also that they require a power of ready adjustment to enable the animal to secure its insect prey.

6. The sixth and last segment of the brain, the medulla oblongata, is also relatively large, and is exposed to view through the rudimentary development of the cerebellum which, as has been said, overlaps it in man.]

It has been already said, that in man and the higher animals there are nerves supplying the orbital muscles and different parts of the face.

The eyeball in man is moved by six little muscles, four straight, (the *recti*) and two *obliqui*, one being the upper, the other lower, oblique.

Now a nerve called the *third*, because it follows the first two (olfactory and optic) goes from the brain to all the orbital muscles except the upper oblique and the outer rectus.

Another nerve, the *fourth*, proceeds to the upper oblique muscle only.

The *fifth nerve* is a very large one, and supplies the nose, tear-gland, eyelids, upper and lower jaws, tongue and teeth.

The *sixth nerve* is a very small one indeed, being exclusively applied to the outer rectus muscle of the orbit.

The *seventh nerve* is, in part, the auditory nerve in part it sends fibres to the face.

The *eighth nerve* is a very complex structure, and consists of, at least, three nerves united together, all arising from the medulla oblongata. It sends branches to the parts about the throat, as well as to the organ of voice, to the lungs, the stomach and the heart.

The nerves of the frog exhibit certain intermediate conditions like those we have seen to exist in various other parts of its anatomy.

In the higher vertebrate animals, as in Man, the muscles which move the eye-ball are supplied by three distinct nerves termed respectively the 3rd, 4th, and 6th. The 5th nerve being a very large and complex one, sending branches to various parts of the head and its organs.

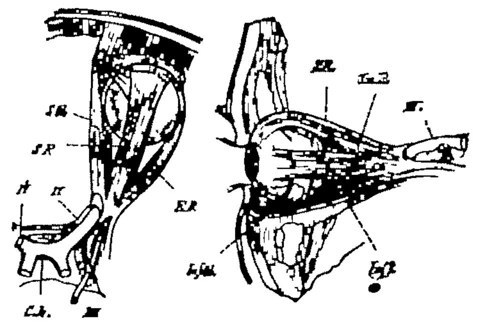


FIG. 73—The Muscles of the Eyeballs, viewed from above and from the outer side *R S*, the superior rectus, *Inf.R*, the inferior rectus, *E R*, the external rectus, *In R*, the internal rectus; *S Ob*, the superior oblique, *Inf.Ob*, the inferior oblique; *Ch*, the chiasma of the optic nerves (*II.*), *III.*, the third nerve, which supplies all the muscles except the superior oblique and the external rectus

Now in the frog there is no distinct 6th nerve, it being replaced by an extra branch of the 5th nerve. This modification, however, is but one step towards a condition which obtains in the Mud-fish (*Lepidosiren*), when all these three nerves are quite blended with one division (the Ophthalmic) of the fifth nerve.

Again in the higher Vertebrates, as in Man, the 8th nerve is a very large and complex one, and distributed as in him. It is also so distributed in the adult frog.

In the tadpole, however, this nerve shows a very different arrangement. After issuing from the skull this nerve sends a branch down the outer side of each branchial arch and then gives off a very long one, which extends laterally, *i.e.* along the side of the body and tail.

Nothing like this exists in any Beast, Bird or Reptile, but when we come to the class of Fishes we encounter a precisely similar state of things. Here we find the eighth nerve sending a branch to each branchial arch, and giving off a great nerve proceeding along the side of the body and tail, and on that account named the *nervus lateralis*.

ST. GEORGE MIVART

(*To be continued.*)

THE INDUCTION TUBE OF W. SIEMENS

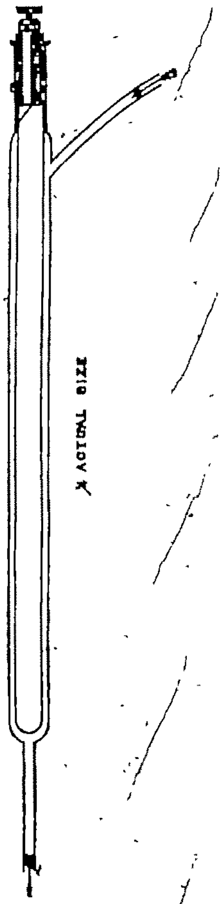
A TRANSLATION from a French periodical, *La Nature*, of an article on "Tubes for silent electrical discharges," appears in NATURE of Jan. 29 (vol. ix. p. 244). After referring to the action of the electric spark upon oxygen gas, the author of the article continues: "For the purpose of more easily obtaining ozone, M. Houzeau has recently constructed an apparatus worked by a Ruhmkorff coil, in which there are no longer sparks, but only dark discharges—*effluvia*—far more efficacious in the production of modified oxygen." Again, it is said, that M. Houzeau "has recently devised an apparatus for the preparation of ozone, which is spreading rapidly among the laboratories, and which has already yielded very remarkable results." A description of the apparatus is then given; further on, it is said, that "M. Houzeau is not the only one who has made use of the tubes whose structure he has made known, but that M. Boillot, a writer," it appears," well known to the readers of the *Moniteur*, "has made some further propositions about them; and lastly, that M. A. Thénard" (whose investigations constitute the main subject of the article) "has brought to bear on the construction of the tubes a further modification which makes them still more efficacious." A description and drawing of the apparatus of M. A. Thénard is given. Those who are unacquainted with the facts of the case will be surprised to learn that the invention thus publicly announced, although, doubtless, in principle deserving of the highest praise, was not made either by M. Houzeau, M. Boillot, or M. A. Thénard, but is simply a somewhat clumsy form of the Induction-tube devised by W. Siemens, which is described in his "Memoir on Electrostatic Induction," contained in *Poggendorff's Annalen*, for 1857 (vol. cii. p. 120).

This Induction-tube is one of the most remarkable, as well as simple instruments, of chemical research which has ever been devised; enabling us, by the action of electricity, to effect changes in the composition of gases which may be compared with the chemical changes effected in liquids by the agency of the voltaic battery. A few words in explanation of the instrument may interest the readers of NATURE.

The simplest form of induction-apparatus consists in two thin glass plates, of which one side is coated with tin-foil, and which are so arranged that the uncovered surfaces are parallel to one another, and separated by a uniform, narrow interval of about one or two millimetres filled, say, with air. If this apparatus be charged with electricity by a sufficiently charged Leyden jar, at the moment of the charge the air between the plates becomes luminous, and the same appearance is presented when the apparatus is discharged. To produce this effect, however, the apparatus must be charged beyond a certain limit, determined, in each case, by the special arrangement of the apparatus and the materials employed in its construction. Now, if the two plates of tin-foil be respectively connected with the terminals of a powerful Ruhmkorff's coil, the apparatus is successively charged with electricity and discharged; these operations being alternately repeated in such rapid succession that the air, in the interval between the plates, appears permanently luminous. We have, moreover, evidence of the occurrence in this interval of chemical changes determined by the electric action, in the odour and characteristic properties of ozone which may be recognised in a current of air or oxygen compelled to pass between the plates. The conclusion drawn by Siemens from this experiment is, that the electric polarisation of the particles of a dielectric cannot be carried beyond a certain point; and that if it be attempted to accumulate electricity in the apparatus beyond this point, the excess of this tension or polarisation appears in the form of the dynamical phenomena occurring between the plates, namely, light, heat, and chemical change. (*Poggendorff's Annalen*, *loc. cit.*, p. 119).

Now it is evident that in this arrangement the two sheets of glass may be replaced by two concentric cylinders of glass, the interior of the inner cylinder and the exterior of the outer cylinder being coated with tin-foil, as in the case of the plates. It is precisely this change which is effected in the induction-tube of Siemens, but with the additional advantage that in the induction-tube a regular flow of the gas to be operated upon may be maintained, that the experiment may be made at any required temperature, and the gaseous products of the experiment collected for examination. The construction of this induction-tube will be readily understood from the annexed drawing (taken from *Pogg. Ann.* loc. cit.), where the ring shows the horizontal section of the tube.

6.a



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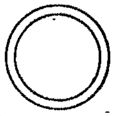

½ ACTUAL SIZE

If the reader will be at the trouble of comparing the description of the tube of M. Houzeau and the drawing of the tube of M. A. Thénard, with the description and drawing of the induction-tube of Siemens he will be satisfied of the substantial identity, both in principle and construction, of these pretended novelties with that invention. At the same time if the statement of these ridiculous pretensions were limited to those made in the article translated in NATURE, vol. ix. p. 244, they would hardly be worthy of notice, but this is not quite the case. The induction-tube of Siemens under the title of "the tube of M. Houzeau," is being rapidly acclimatised as a French discovery. In the article on ozone contained in a recent number of the "Dictionnaire de Chimie," which bears evidence of being the work of a highly competent writer, where we might expect to find a comprehensive treatment of the subject, a similar lapse occurs. We have there, too, a drawing of the tube of M. Houzeau which is described as "a happy modification of the tube of M. Babo," but not a word is said about Siemens, the inventor of the tube, whose name is simply dropped. Other similar instances might be brought forward which have afforded an opportunity of rectifying these mistakes, but of which no advantage has been taken. I have therefore ventured to make these remarks, not only I may say in the interest of justice, but also, having myself made many experiments with the induction-tube of Siemens, I have learned, perhaps, more than others to appreciate its value and feel myself under a special debt of gratitude to the inventor.

B. C. BRODIE

RECENT RESEARCHES ON TERMITES AND HONEY-BEES

THE accompanying letter, just received from Fritz Muller, in Southern Brazil, is so interesting that it appears to me well worth publishing in NATURE. His discovery of the two sexually mature forms of Termites, and of their habits, is now published in Germany; nevertheless few Englishmen will have as yet seen the account.

In the German paper he justly compares, as far as function is concerned, the winged males and females of the one form, and the wingless males and females of the second form, with those plants which produce flowers of two forms, serving different ends, of which so excellent an account has lately appeared in NATURE by his brother, Hermann Muller.

The facts, also, given by Fritz Muller with respect to the stingless bees of Brazil will surprise and interest entomologists. CHARLES DARWIN

Feb. 11

"For some years I have been engaged in studying the natural history of our Termites, of which I have had more than a dozen living species at my disposition. The several species differ much more in their habits and in their anatomy than is generally assumed. In most species there are two sets of neuters, viz., labourers and soldiers; but in some species (*Calotermes* Hg.) the labourers, and in others (*Anoplotermes* F. M.) the soldiers, are wanting. With respect to these neuters I have come to the same conclusion as that arrived at by Mr. Bates, viz. that, differently from what we see in social Hymenoptera, they are not modified imagos (sterile females), but modified larvæ, which undergo no further metamorphosis. This accounts for the fact first observed by Lespès, that both the sexes are represented among the sterile (or so-called neuter) Termites. In some species of *Calotermes* the male soldiers may even externally be distinguished from the female ones. I have been able to confirm, in almost all our species, the fact already observed by Mr. Smeathman a century ago, but doubted by most subsequent writers, that in the company of the queen there lives always a king. The most interesting fact in the natural history of these curious insects is the existence of two forms of sexual individuals, in some (if not in all) of the species. Besides the winged males and females, which are produced in vast numbers, and which, leaving the termitary in large swarms, may intercross with those produced in other communities, there are wingless males and females, which never leave the termitary where they are born, and which replace the winged males or females, whenever a community does not find in due time a true king or queen. Once I found a king (of a species of *Eutermes*) living in company with as many as thirty-one such complemental females, as they may be called, instead of with a single legitimate queen. Termites would, no doubt, save an extraordinary amount of labour if, instead of raising annually myriads of winged males and females, almost all of which (helpless creatures as they are) perish in the time of swarming without being able to find a new home, they raised solely a few wingless males and females, which, free from danger, might remain in their native termitary; and he who does not admit the paramount importance of intercrossing, must of course wonder why this latter manner of reproduction (by wingless individuals) has not long since taken the place through natural selection of the production of winged males and females. But the wingless individuals would of course have to pair always with their near relatives, whilst by the swarming of the winged Termites a chance is given to them for the intercrossing of individuals not nearly related. I sent to Germany, about a year ago, a paper on this subject, but do not know whether it has yet been published.

"From Termites I have lately turned my attention to a still more interesting group of social insects, viz., our stingless honey-bees (Melipona and Trigona). Though a high authority in this matter, Mr. Frederick Smith, has lately affirmed, that "we have now acquired almost a complete history of their economy," I still believe, that almost all remains to be done in this respect. I think that even their affinities are not yet well established, and that they are by no means intermediate between hive- and humble-bees, nor so nearly allied to them, as is now generally admitted. Wasps and hive-bees have no doubt independently acquired their social habits, as well as the habit of constructing combs of hexagonal cells, and so, I think, has Melipona. The genera Apis and Melipona may even have separated from a common progenitor, before wax was used in the construction of the cells; for in hive-bees, as is well known, wax is secreted on the ventral side: in Melipona on the contrary, as I have seen, on the dorsal side of the abdomen; now it is not probable, that the secretion of wax, when once established, should have migrated from the ventral to the dorsal side, or *vice versâ*.

"The queen of the hive-bee fixes her eggs on the bottom of the empty cells; the larvæ are fed by the labourers at first with semi-digested food, and afterwards with a mixture of pollen and honey, and only when the larvæ are full grown, the cells are closed. The Meliponæ and Trigonæ, on the contrary, fill the cells with semi-digested food before the eggs are laid, and they shut the cells immediately after the queen has dropped an egg on the food. With hive-bees the royal cells, in which the future queens have to be raised, differ in their direction from the other cells; this is not the case with Melipona and Trigona, where all the cells are vertical, with their orifices turned upward, forming horizontal (or rarely spirally ascending) combs. You know that honey is stored by our stingless bees in large, oval, irregularly clustered cells; and thus there are many more or less important differences in the structure, as well as in the economy, of Apis and Melipona.

"My brother, who is now examining carefully the external structure of our species, is surprised at the amount of variability, which the several species show in the structure of their hind legs, of their wings, &c., and not less are the differences they exhibit in their habits.

"I have hitherto observed here 14 species of Melipona and Trigona, the smallest of them scarcely exceeding 2 millimetres in length, the largest being about the size of the hive-bee. One of these species lives as a parasite within the nests of some other species. I have now, in my garden, hives of 4 of our species, in which I have observed the construction of the combs, the laying of the eggs, &c., and I hope I shall soon be able to obtain hives of some more species. Some of our species are so elegant and beautiful and so extremely interesting, that they would be a most precious acquisition for zoological gardens or large hot-houses; nor do I think that it would be very difficult to bring them to Europe and there to preserve them in a living state.

"If it be of some interest to you I shall be glad to give you from time to time an account of what I may observe in my Melipona apiary.

"Believe me, dear Sir, &c.,
"FRITZ MÜLLER"

MARS*

IN the previous article were mentioned some of Professor Kaiser's conclusions. We are induced to add a few further remarks, from their general applicability. The delineation of the heavenly bodies, he says, is always a very difficult task, especially when, as in the case of Mars, we have to deal with features more or less indistinct, delicately and gradually shaded. With the most powerful telescopes the disc is but small; and on it we find a mass of ill-defined and frequently very feeble spots, which require close attention for their disentanglement, and it is hard to obtain a clear conviction as to the outlines and shadings that have to be drawn. The difficulty is much increased by the incessant undulations of the air; and in the seldom-recurring moments of stillness so much under good circumstances is visible, that even the best artist cannot draw it all in half an hour, a period during which usually there are but a very few tranquil glimpses, and after which the planet will have materially changed its aspect from rotation. Even were it easier to distinguish what is actually visible, it requires great practice to represent it faithfully; and whoever has had personal experience of the difficulties of such designs will have but a limited confidence in the various portraits or the supposed changes that they represent. As a further illustration of these difficulties he refers to the representations of the Orion nebula by Rosse, Lassell, Secchi, and Liapounov (he could have added Herschel II.); or the portraits given by Bond, and others, of Donati's Comet. He might have cited, had he known of it, Prof. Young's remark as to the solar corona (where, however, these difficulties are heightened by the excitement of the moment), that "the drawings made by persons standing side by side differ to an extent that is sometimes really ludicrous, and has induced more than one astronomer who had not himself seen an eclipse, but judged only from the written accounts and sketches, to declare his belief that this whole outer corona is a mere subjective phenomenon."

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* Continued from p. 289.

The justice of Kaiser's remarks will readily make itself felt, but they do not exhaust the subject; something may perhaps be added as to the "personal equation" of vision. Independently of mechanical defects in the eye, there are inaccuracies of perception; and even if the rays have kept an uninterrupted and undeviating course to the retina, they do not always produce corresponding impressions on the mind. Whatever may be the cause, we frequently meet with defects in the sense of form, or proportion, or inclination, or even the presence of features which are not the immediate objects of attention. Comparisons of size are often very erroneous; craftsmen well know the meaning of "a true eye;" and the expression "I did not see it," is constantly employed with reference to a thousand objects whose representation on the retina is all the while unquestionable. It is in these respects that celestial photography is invaluable as recording everything and putting everything in its proper place; but photography, as Kaiser observes, is inapplicable to the light of Mars. Another point, too, might have admitted of notice. Although we may certainly, with him, be baffled in reconciling Rosse and Lassell, we may bear in mind, as regards the comparison of larger and smaller instruments, Dawes's important remark to the effect that a certain relative proportion of light and power may be essential to the visibility of some classes of difficult objects.

Without subscribing implicitly to the whole of Kaiser's views, some of which admit of doubt—as, for instance, when we contrast his assertion that the spots are never sharply defined, with the clearness and keenness of outline occasionally recorded by Lockyer and others—we may well admit their general accuracy. But we find it more difficult to accompany him in his inferences as to the planet's physical constitution.

The earth-light upon the moon having been found by Schröter more conspicuous when it proceeded from the hemisphere of our globe containing the largest amount of land, Kaiser implies that it has hence been inferred that (as it is difficult for us not to imagine other planets constituted like our own), the brighter and darker portions of Mars are equivalent to land and water. Whether such an opinion may have been arrived at in this circuitous way or not, it seems highly probable without any reference to lunar appearances. The eminently absorptive power of water is well known; even a thickness of seven feet will, it is said, diminish the incident light by one-half; and below 700 feet it is quenched in unbroken darkness; and the quantity of diffused light reflected from its surface would be inconsiderable, while the solar image at the distance of the Earth would be too minute, in all probability, to be visible. This reasoning would seem fairly to hold its ground against that of the Leiden astronomer, who does not believe that seas so looked upon would show such innumerable gradations in tone, or be so invariably ill-defined at their edges, while the same telescope gives perfect sharpness to the polar snows. He goes in fact so far as to say that if we may form any conclusion from their aspect, it is, that they cannot resemble seas such as our own. But as to distinctness of boundary, his experience is not accordant with that of other excellent observers, especially Lockyer, who remarks that "the effect of a cloudless and perfectly pure sky both here and on Mars appears to be, that the dark portions of the planet become darkest and most distinctly visible; the coast-lines (if I may so call them) being at such times so hard and sharp that (as has been mentioned by Mr. Lassell), it is quite impossible to represent the outlines faithfully." A more natural inference, it seems to the writer, would be that these fluid masses contain large areas of very slight depth, that the edges are in many places very shelving, and that possibly they may be the more transparent from the absence of salt. Other astronomers, Kaiser tells us, but without mentioning their names, have reversed the idea, and thought the bright parts to be seas, but they do not thus escape his objections on the score of definition, nor account for the dusky tracts which some of the great bright expanses contain. He has perhaps got hold of a more substantial difficulty in the aspect of the north polar region, where the white spot is often encompassed by a widely-extended dark zone with many gradations of tint. The width of this belt, very great when foreshortening is taken into account, is no doubt variable: Beer and Mädler ascribed it to the non-reflective power of the damp surface bared by the rapidly melting snow. On the whole, when Kaiser considers that nothing is established with certainty but the existence of an atmosphere and the connection of the polar spots with the seasons, we hesitate to follow him; and we should prefer the conclusion of Phillips, adopted by Lockyer, that "over a permanent basis of bright and dusky tracts, a variable envelope gathers and fluctuates, partially modifying the aspect of the fundamental features, and even in some degree disguising them under new lights and shades, which present no constancy, a thin vaporous atmosphere probably resting on a surface of land, snow, and water." A more protracted course of observation may possibly modify in some way this result, but so far as past investigations extend, we may say that nothing has been detected inconsistent with it. Could we be actually transported to that far distant surface, we should probably find much to astonish us that we cannot so much as conjecture here; it was a sound remark of Schröter's that unity in variety is the universal character of creation; and the spectroscope of Huggins has already in this instance confirmed it by the detection of absorption-lines the cause of which is utterly unknown. Our future inquiries should be conducted in that impartial spirit which is equally ready to admit the indications of discrepancy and of resemblance, and which is more anxious to ascertain facts than to seek their premature elucidation. We have as yet read but a part of the inscription on that golden shield: some of it has probably been deciphered correctly; how much of the remainder may give way we know not; but the whole, it will never be given to us to understand.

The extensive researches in which Dr. Terby of Louvain has for some time been engaged, and in which he has shown unwearied diligence and perseverance, if embodied, as we trust they will be, in one comprehensive result, will give material assistance in disentangling and concentrating our present scattered and discordant materials, and we may look forward with hope to the very promising opposition of 1877; when, if the seasons on both planets are as favourable as their mutual proximity, we may reasonably expect some advance to be in store for us. The great object will naturally be the identification of the dark spots, as well as a more careful delineation of their boundaries: attention will doubtless be paid towards obtaining a definitive value for the rotation; but in this direction progress is not very material, as we have already a sufficient approximation. Those who would see an extraordinary instance of the most painstaking and protracted efforts to get rid of a trifling uncertainty may apply themselves to the 23 pages of Kaiser, in which all kinds of varied combinations are tried to reconcile some conflicting decimals of a second, for to these the question is reduced at last. Cassini, as far back as 1666, had fixed the rotation at 24h. 40m. with surprising correctness for his day. Herschel I. brought it to 24h. 39m 21·67s. but, as Beer and Madler perceived, the omission of one rotation, and of the effects of phasis and aberration, vitiated the result. They in turn gave 24h. 37m. 23·7s. Kaiser, from many elaborate comparisons, deduced a mean of 22·62s., but Proctor having found a value of 22·735s. the former, who thought the English astronomer's coincidences illusory, went into the whole subject afresh with marvellous minuteness, and got out a final mean of 22·531s., discovering by the way some unexpected inaccuracies, convincing himself that the correctness of the best drawings has been greatly over-rated, and finally, in much mortified perplexity, leaving it to every one to choose his own combination. No computation, he says, can make us sure to the hundredth of a second; and unless observations become very much more precise, it will be several centuries before such a result will be obtained: how much the wiser mankind would be for it, is another question, which we need not discuss here. But there is, perhaps, no great difficulty in divining the cause of the Professor's troubles. Epochs of rotation could only be safely taken from drawings made with that special object, and few such probably exist; the designer usually either contenting himself with a general likeness, or being occupied about details, the study of which would of itself render him less attentive to mere position. In future, these objects might be better separated; and while the artist busies himself with the *minutiæ* of the picture, the rotation-seeker should employ himself exclusively in estimating the co-ordinates of some conspicuous points—a process which admits of a mean taken between many proportional valuations.

Several other desirable matters of inquiry will readily offer themselves. Measures of ellipticity have as yet yielded only contradictory results. The inclination of the axis, last deduced by Oudemans in 1852, may be susceptible of correction; and the excentric position of one or both of the snow-spots, and the unsymmetrical position of the isothermal poles, would be matters of interesting investigation. The amount of the latter deviation, first measured by the elder Herschel, has been given so very differently by different observers, even at the same opposition, that it evidently is open to fresh determination. The well-known colours will of course catch the eye; and attention may be paid to the question whether the green, or as others think blue, tint of the dark parts (which Kaiser saw as grey only) is really, as Herschel II. implies, the mere result of contrast. The effect may be possibly thus heightened; but no one who saw one of the great seas as the writer did with a 9in. silvered speculum on April 4, 1871, could doubt the independent existence of a beautiful clear blue-grey tint, the more certain as a shading on another part of the disc was of a brownish hue: nor does it seem to have been noticed that no effect of contrast has been traced in the polar snows. The luminous and occasionally coloured patches and segments on the limb should receive attention, and the position of "Dawes' ice-island" be scrutinised; such a brilliant speck I witnessed at the above epoch, but I believe in another situation. Black points should be looked for, as such have been detected by Mitchell and Dawes; and it should be noted at the time of any conspicuous feebleness of the markings, whether the sharpness of the limb indicates the cause to be further distant than our own atmosphere: and in general the "daily—nay, hourly—changes in the detail and in the tones of the different parts of the planet, both light and dark," described by Lockyer, should be carefully watched and recorded;

"In tenui labor;"

nevertheless, none of these little matters will be considered insignificant by those who love to behold in such things the footsteps of Creative and Upholding Power.

T. W. WEBB

NOTES

WE have received some interesting notes of the work done by the eminent Russian explorer, Dr. von Miclucho-Maclay, which we hope to publish next week. Contrary to the advice of everyone, this intrepid traveller and true devotee of Science is determined upon again visiting the east coast of Papua. When his researches here are complete he intends to visit some of the islands of Polynesia and certain parts of the coast of Australia. This, he calculates, will take up five or six years. The Governor of the Dutch East Indies, like a true man of Science, had given Dr. Maclay, for the last six months, roomy and comfortable quarters in his palace at Buitonrovg. It would be well if all in high position would imitate this kind of "patronage."

THE Meteorological Committee of the Board of Trade have resolved to commence the issue of lithographed copies of the twenty-four hourly tabulated readings, taken at their seven observatories, for every element which is observed continuously, commencing with January 1, 1874. The sheets will be issued quarterly, and the issue will be a limited one. The subscription for a copy is 1*l.* per annum, to cover a portion of the expense of production. The sheets will not be distributed with the publications of the office.

M. L. QUETELET, the founder and director of the Brussels Observatory, died in Brussels on Monday night, aged 77. He leaves a son, M. Ernest Quételet, who inherits the scientific enthusiasm of his father.

THE letter which has been received from Consul Prideaux, and the extract from Cameron's letter published in the *Academy*, adds but little to the details we gave some time ago concerning Livingstone's reported death. Lake Bemba is identified by Consul Prideaux as Lake Bangweolo, and a letter from the Arab Governor of Unyanyembe fixes the spot where the great traveller died at Lobisa. A letter to Dr. Petermann from the German African traveller and Austrian Consul at Zanzibar, Mr. Richard Brenner, merely repeats the statements already known. Dr. Kirk, under date Feb. 12, writes to the *Academy* as follows:—"This morning I have heard indirectly from Zanzibar, and find people there who could judge, still question the truth of the story ot Livingstone's death. Like us, they see nothing but native report to base it on." Let us hope that this is the real state of the case. As Zanzibar and Ujiji are at present at peace it is expected that there will be no difficulty in getting the Doctor's valuable journals. It is gratifying to see from Mr. Markham's letter in yesterday's *Times* that through Sir Samuel Baker's determined energy, the route to Zanzibar has been virtually opened up from the north.

THE letter above referred to from Consul Brenner, states that a German botanist, M. Hildebrand, has been preparing, for a year past, to undertake a journey into the interior of the Galla country and Somali Land.

A SHORT course of Lectures on the Growth of Physical Science during the last twenty-five years, is to be given at the request of a number of gentlemen in Edinburgh, in St. George's Hall, by Prof. Tait, of the University of Edinburgh. The first lecture is to be given to-day.

AT the Annual General Meeting of the Glasgow Geological Society, on Thursday last, Sir William Thomson gave an address on "The Influence of Geological Changes on the Earth's Rotation." We hope to be able, very soon to give an abstract of this address.

THE *Times* announces that the following arrangements have been made in consequence of Mr. Henry Cole's retirement last year from the post of Secretary of the Science and Art Department and Director of the South Kensington Museum:—Sir Francis Sandford, Secretary of the Education Department, will also be Secretary of the Science and Art Department; Major Donnelly, R.E., Official Inspector for Science, will be Director of Schools of Science and Art and affiliated institutions; Mr. Norman MacLeod will remain Assistant Secretary of the Science and Art Department; and Mr. Philip Cunliffe Owen, Assistant Director of the South Kensington Museum, will be the Director of that Museum.

PROF. HELMHOLTZ has communicated to the Academy of Sciences of Berlin a paper on "The Direction of Balloons," in which he enters into a number of elaborate calculations. In his calculations he directs attention only to the relation between the force and the weight, and supposes that the means at our disposal will enable us to construct the envelope of the balloon and its motive power. But, Prof. Helmholtz says, "it appears to me that here there exists a great difficulty in the way of execution, for the solid parts of the machine do not preserve the necessary solidity when they are much enlarged, although they continue to be geometrically similar; they then must be made thicker, and consequently heavier. To obtain the same effect with small motors at great speed, there is a loss of work. We can only work economically then with motors of large surface urged by a motion relatively slow. One of the great practical difficulties will then be to obtain the necessary dimensions without overloading the balloon."

PROF. H. A. NEWTON thus criticises the Report of the British Association Committee on Units in the March number of *Silliman's Journal*:—The *dyne* or unit of force which is proposed by the committee is to be a new unit of the same nature as a gram-weight, or the earth's attraction for a gram-mass, and having no commensurable ratio with it. Now our simplest and most useful ideas of force are derived at once from weight. It seems to us that, of necessity, this will always be the case. Probably the learned committee have no expectation that even among scientific men the new units will entirely replace what they call the vulgar ones. If, then, their recommendation is accepted, we shall create for certain departments of mechanical science new units of force and energy which are in no useful ratios to those used in other departments of science, and by people at large. Is there not some way of avoiding this great evil? Societies are formed and sustained whose main and most worthy object is to get rid of such confusions. We think the proposed units should be stoutly challenged to show a necessity for their being. We do need, it may be added, a new *name* for the earth's attraction upon a gram of matter at some fixed place. The words *gram*, *pound*, *ton*, &c., have had to do service in two different senses, that is, as mass, and as force. If any good word could come into use that shall express the earth's attractive force for a gram of matter at some place that may be agreed upon, it would meet a real want.

THE collection of Humming Birds of the late M. Jules Bourcier is to be sold by auction at Paris on March 2. M. Bourcier's collection of these birds was, a few years ago, the best and most complete in existence, embracing numerous types of the species described by the French naturalists, and specimens collected by himself during his residence in Ecuador as French Consul.

MR. A. S. NAPIER, of Owens College, Manchester, who has been elected to a Natural Science Scholarship at Exeter College, Oxford, received the first part of his science training at Rugby. Mr. W. E. Hoyle, of Owens College, has been elected to a Natural Science exhibition in the same College.

THE Japanese Government have, through their Legation in London, appointed Mr. R. Routledge, B. Sc., F.C.S., to the Professorship of Chemistry and Physics in the Imperial College at Yeddo. Mr. Routledge was formerly of the Owens College, Manchester, where he studied Chemistry under Dr. Roscoe, and afterwards took high honours at the University of London.

WE gladly call attention to the action taken by the British Association, "Boulder Committee," under its secretary, the Rev. H. W. Crosskey. A large printed form has been prepared, with a set of well-drawn up questions, and spaces for the answers of those who may be inclined to assist the Committee in their praiseworthy work. Copies of this form may be obtained by applying to Mr. Crosskey, 28, George Road, Birmingham.

A TELEGRAM from Cairo announces that Dr. Beke has succeeded in discovering the true Mount Sinai. It is said to be situated one day's journey west of Akaba, is called by the Arabs the Mountain of Light, and is 5,000 ft. high. On the summit were some sacrificial remains of animals.

MISS FRANCES STRICKLAND, of Appleby-court, Tewkesbury, has offered to found at the University of Cambridge a curatorship of the Ornithological collection formerly belonging to her brother, Mr. H. F. Strickland, F.R.S., and presented to the University in 1867 by his widow. Miss Strickland proposes to endow the office with a permanent stipend of 150*l.* per annum. The principal conditions attached to the gift are that the curator be appointed by the foundress during her lifetime, and afterwards by Mrs. Catherine Strickland, and, on the decease of these two ladies, by the superintendent of the University Museums of Zoology and Comparative Anatomy, but in each case with the consent of the Vice-Chancellor. That the curator is to be subject to the authority of the superintendent of the museums, and that his first duty be the proper custody and efficient preservation of the Strickland collection, making an accurate catalogue of it, so that the collection be of the greatest service to Science. He would be required to reside in the University; and in case of the abolition of the office of superintendent of the museums the curator shall be appointed and removed by the Professor of Zoology and Comparative Anatomy with the consent of the Vice-Chancellor. The Council of the Senate recommend the acceptance of Miss Strickland's liberal offer.

ON this day week there passed from among us a countryman whose power has been but too little appreciated, and far too little recompensed by ourselves or other European nations. Sir Francis Pettit Smith was, to all intents and purposes, the discoverer of the screw-propeller, a method of progression as practically advantageous as it is theoretically perfect; nevertheless, the benefits which he has derived from his indebted countrymen are but a paltry annuity and an equally insignificant decoration. Considering the little encouragement given by our Government for first-class work, it is really a matter of surprise that any should be produced at all.

AT the meeting of the Academy of Sciences at Paris, on Feb. 9, the candidates to be recommended to the Minister of Public Instruction for the chair of Comparative Embryogenesis at the College of France, were balloted for. The names of MM. Gerbe, Balbiani, and Dareste, were presented to the meeting, and the result of the voting was to select the two former gentlemen as the Academy's nominees for the post.

CAPTAIN S. P. OLIVER writes us concerning a meteor-cloud which he observed at Buncrana, Co. Donegal, on Feb. 5, at about 9.10 P.M. local time. He saw what he at first thought to be a comet's tail, viz. a broad band of silvery white and luminous cloud extending in an arc from S.E. by E. to N.W. by W., as near as he could jûdge, from horizon to horizon, but tapering towards the extremities. The apex of this arc, which was some four or five degrees in width, was as nearly as possible on the meridian at about 80° elevation from the horizon. The band passed within three or four degrees above the upper stars of Orion. Through this luminous cloud the stars shone brightly. The edges appeared well-marked, and there was no appearance of that serpentine track into which meteor-clouds frequently dissolve. Several "shooting-stars" were visible the same evening. One especially he noticed which seemed to come from a radiant point at the S.E. extremity of the above-named cloud.

MESSRS. SMITH, ELDER, and Co. have a new edition of Mr. Charles Darwin's work on the "Structure and Distribution of Coral Reefs" in preparation.

MICHELET, the celebrated historian, who died within the last few days at Cannes, aged 76 years, has written a few sensational books on natural history. "L'Oiseau" and "L'Insecte," had an immense circulation, although their real scientific value was very small.

M. REINWOLD, one of the largest Parisian scientific publishers, is just publishing a translation of Haeckel's "History of Creation." It is prefaced by M. Charles Martin, one of the most celebrated correspondent members of the French Institute. Consequently Darwinism is not to be considered as having been extinguished in France by the last rejection of Darwin by the Academy. Neither will the success of Haeckel's great work be paralysed by the cry of *no more Germanism* raised in certain quarters.

CAPT. MOUCHEZ, who has been appointed the chief astronomer for St. Paul's station on the Venus Transit Expedition, is publishing, at the expense of the French Admiralty, a map of the Algerian coast on the scale of $\frac{1}{100000}$. The survey was executed on a new plan and only lasted 18 months. Although Algeria extends about 750 miles east-west, M. Mouchez has determined all his stations by a series of triangulations executed on shore, independently of the situation of his boats or ships.

FOR some years past much interest has been excited in the United States in reference to the erection of a large telescope, and possibly a complete astronomical observatory on the high portion of the Rocky Mountains. As preliminary to this, a number of careful examinations have been made of the optical qualities of the atmosphere in various portions of the Western country. Of these special interest attaches to the expedition of Professor Davidson of the Coast Survey, whose report to the California Academy of Sciences, states that the meteorological tables kept at Summit Station, on the Sierra Nevada, 7,042 feet above the sea, during the year ending November 1867, show that out of 358 days and nights only eighty-eight were cloudy, nearly all of these occurring in the winter months, during which the snowfall was about forty-five feet, the winter not being unusually mild. The summer weather is very pleasant, the nights cool, and the atmosphere wonderfully clear. The mountain flanks are covered with verdure during the summer, and there is freedom from great clouds of dust. Prof. Davidson says that, owing to the steadiness of the atmosphere, observations at this elevated point would in one or two nights be of greater value than the results of six months' observations at lower stations. Higher and perhaps more desirable positions exist in the immediate neighbourhood of Summit Station; and the interest excited by Prof. Davidson's report probably has, to a considerable extent, influenced the determination announced in a recent letter of Mr. J. Lick, the well-known millionaire of San Francisco. This gentleman has indicated in a letter to the California Academy of Sciences, and again in a letter to Prof. Joseph Henry, his desire to establish an observatory in the best possible location, and provide it with the largest and finest astronomical instruments. He proposes to this end to set aside one million dollars as a permanent endowment fund. This is a monument and a renown which few are rich and wise enough to achieve for themselves, and it is greatly to be hoped that the founder of the Lick Observatory may live to enjoy the congratulations of his State and country.

PROF. JAMES ORTON, of Vassar College, N.Y., has just returned to the United States from South America, where he has been engaged in a second exploration of the Amazons. The general object of his recent travels in South America, was to supplement his expedition in 1867, when he crossed the continent from west to east, *viâ* Quito and the Nipo wilderness. His route in 1873 was up the Amazons from Para to Yurimaguas on the Hoallaga River; thence up to the Paravapura and its tributary, the Cachuyeen to Balsa Puerto; thence over the Icuto Vange on foot to Moyobamba; thence across and among the Andes to Chachapoyas and Cayamarca, crossing the Upper Mavañon, or Balsas, and striking the coast at Pacasmayo; thence to Lima and its immediate region; thence to Mollendo, Areguipa and Puno on the shore of the Lake Titicaca. He was the first traveller to pass from the Pacific to the Lake by the railway just finished by Mr. Meiggs. The prime object of his explorations was to study the physical geography, geology and topography of the Amazons. On these points he obtained a vast amount of new and reliable information. He found that the Upper Amazon (Marañon), has been grossly misrepresented in all the more recent maps of Peru. He made everywhere, but especially in Northern Peru, large collections in natural history, to throw light upon the distribution of animal life. Prof. Orton will condense the results of his expedition in a work on the Physical Geography, Natural History, and Commercial Resources of the Valley of the Amazons.

FRESH advices to January 11, received from the German exploring expedition in the Lybian Desert, under Gerhard Rohlfs, announce that the expedition had reached the important oasis of Dachel, containing 17,000 inhabitants. Valuable geographical discoveries had been made, and six maps of the country had been taken.

THE metrical system has just been adopted in Germany for the measurement of distances. The official papers have published the order with decrees that henceforth the kilometre shall replace the Prussian mile.

THE additions to the Zoological Society's Gardens during the past week include a pair of Coatis (*Nasua narica*) from South America, presented by Mr. W. P. Chambers; an Egyptian Fox (*Canis niloticus*) from Port Said, presented by Mr. J. T. Keane; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. R. Wilkinson; a Great Kangaroo (*Macropus giganteus*), born in the Gardens; five Branched Sea Horses (*Hippocampus ramulosus*) from the coast of France, purchased; a Capybara (*Hydrochærus capybara*) from Rio Negro.

SCIENTIFIC SERIALS

In the *Journal of Botany* for January, there is no paper of special general interest, the illustration being that of a new British moss *Tortula inclinata*. Mr. J. G. Baker describes a number of new or little known capsular gamophyllous Liliaceæ; and Mr. F. E. Kitchener gives an "elementary proof of the rule for detecting spiral arrangement." The first article in the February No. is an illustration of how much yet remains to be done in completing the British flora, being a description with a plate of a British Dock, *Rumex maximus*, discovered by the Hon. J. L. Warren in the neighbourhood of Lewes, where it was recorded many years since, but not having been observed in the meantime, had been generally treated as an error. There is no other paper bearing specially on British Botany, but a very useful account of the Esparto-grass of commerce, by Mr. J. R. Jackson.

Astronomische Nachrichten, No. 1972. The elements of Henry's comet, 1873, by E. Weiss and Aug. Zielinsky, the following elements are given:—

	Weiss. Berlin Time.	Zielinsky. Paris Time.
T =	Oct. 1·80022	Oct. 1·765792
π =	50° 28′ 18″·5	50° 28′ 42″·96
☊ =	176° 43′ 14″	176° 43′ 21″·88
i =	121° 28′ 58″·8	121° 27′ 48″·19
$\log q$ =	9·585297	9·5866441

A list of fifteen new nebulæ is given, which M. Stephan observed at Marseilles.

Poggendorff's Annalen der Physik und Chemie, No. 10, 1873.—We may first notice, in this number, some observations relating to phenomena of light.—M. Behrens contributes a paper on the production of coloured light through elective reflection. The reflected and transmitted light of opal and other bodies was examined with a micro-spectroscope; and it is shown that certain substances may have colour without absorbing light, and that the two spectra (from reflection and transmission, respectively) are exactly complementary of each other. This, he says, occurs more frequently than one might suppose.—Dr. Nöggerrath draws attention to the production of light in grinding of hard stones, as witnessed in the agate-works at Oberstein and Idar. In the case of all hard stones (which the workmen press with their hands against large grindstones revolving thrice in a second), a strong red light appears between the object and the grindstone, with a red halo and emission of sparks. Transparent stones, however, are lit up throughout with a beautiful yellowish-red light, like that of glowing iron, so that it seems as if the workman must burn his hands (though the rise of temperature was not above 10° or 12° R.). The author invites research in this direction, for which the works named present good opportunity.—The concluding portion of M. von Bezold's paper on the law of colour mixture and the physiological primary colours, is given; the author is led to some valuable deductions which we cannot here stop to particularise.—M. Valerius, in a note on binocular, as compared with monocular, vision, comes to the conclusion that the proportion of brightness of an object, looked at successively with both eyes, and with one, is nearly independent of the absolute amount of illumination: and with ordinary candle or gas flame, does not exceed 1·15. He afterwards found that his left eye was less sensitive than the right; had he used the latter, the proportion in question would be somewhat less. The measurements were made with Foucault's photometer.—M. Kundt contributes a paper on the vibration of rectangular, and especially of square, *air plates*; meaning, by an air plate, a thin layer of air enclosed between two solid plane plates applied to each other (it may be either in communication with the external air, or closed all round). He makes the vibration-forms visible by means of cork powder; and the present communication chiefly shows that the vibration-numbers observed in the entirely-closed air plates agree with those deduced from theory, to less than 1 per cent.—Dr. Hübener gives an account of researches on transpiration of salt solutions through capillary tubes. The velocity of outflow is found to be inversely as the equivalent weights; which may be explained (the author thinks) by the fact, that in compounds with high equivalent weight the molecules are larger than in those with low. If, then, equal weights of two salts of different equivalent weight be dissolved in a liquid, there will be present in the solution of the heavier salt larger but fewer molecules than in the other solution. Hence, in the solution of the first salt, the molecular surface in contact with the solvent will be less than in the second liquid; and the internal friction will be less; thus (other conditions equal), there will be greater mobility.—A paper by Dr. Dibbits discusses, at some length, the dissociation of ammonium salts in aqueous solution; the results detailed being both qualitative and quantitative.—M. Rammelsberg communicates a second note on natural compounds of tantalum and niobium; and Dr. Bender describes an ingenious method of determining the time of vibration of a material pendulum. The remaining matter does not specially call for notice.

SOCIETIES AND ACADEMIES

Royal Society, Feb. 5.—"On the Anatomy and Habits of the genus *Phronima* (Latr.)." By Dr. John Denis Macdonald, F.R.S., Staff Surgeon R.N., Assistant Professor of Naval Hygiene, Netley Medical School.

Of all groups of Crustacea the Amphipoda would appear to exhibit the widest range, in the modification of their parts or organs, without obliterating the delicate lines of natural affinity running through them as a whole. This is well exemplified in the interesting paper of Dr. R. Willemoes-Suhm, naturalist to the *Challenger* Exploring Expedition, "On a new Genus of Amphipod Crustaceans" founded by him, and named *Thaumops*. This genus, although exhibiting many characters in common with *Phronima*, presents some striking points of difference traceable in the external jaw-feet, caudal appendages, the position of the generative bone, and certain particulars in its external anatomy.

During the exploratory voyage of H.M.S. *Herald*, in the S.W. Pacific, numerous species which I have always been in the habit of referring to the genus *Phronima*, were taken in the towing-net; and I might remark that the assumed parasitic habit of these creatures was never, at least, a prominent fact to me, they were so often taken either perfectly free, or tenanting a nidamental case. Those who, like Dr. Suhm, are acquainted with deep-sea dredging, are usually cautious how they refer the doubtful products to their proper habitat; whether it be the bottom that has been reached, or some zone of the watery space above. Indeed it is quite possible for the narrow area of the tallow-arming of the deep-sea lead to include fortuitously, and carry down *Phronima* or any other little crustacean naturally living near the surface; and contact with the bottom would finally press it into the tallow, so as to mislead the observer as to its true habitat. Conversely, in bringing up the dredge from a given depth, it may finally carry with it any more superficial objects casually lying in the track which it takes.

The author then describes a species of *Phronima* captured in lat. 30° 16′ S., long. 176° 27′ W.

The evidence of Dr. Willemoes-Suhm supports my own experience that there is no metamorphosis in this group; and as it is very probable that the history of the development of *Thaumops* would resemble that of *Phronima*, the following observations may be of some importance, as carrying the process a little further than it has perhaps yet been traced by him:—

In lat. 21° 0′ S. and long. 17° 45′ W. off the island of Ono, Fiji group, apparently the same species of *Phronima* as that above referred to was taken in the towing-net, but with the addition of a numerous progeny of young in a large gelatinous but tough nidamental case. This interesting nest was shaped like a barrel, but with both ends open, and the external surface was somewhat tuberculated and uneven. The wall of the tube presented numerous round and puckered openings, observing no very definite arrangement, but through which entering currents were observed to pass. These openings in general pierced the tuberculations, though not invariably.

An external membrane, with an internal lining, was distinctly visible, both seeming to be continuous at the rims of the tube. The space between these layers was filled up with a pulpy substance, in which scattered nucleiform bodies were detected with a higher power of the microscope.

In a subsequent commission on the North-American and West-Indian Station in H.M.S. *Icarus*, I have frequently captured "*Phronima* in its bay," as my messmates would say. In order to bring the swimmerets into full play, the animal protrudes its bony tail foremost from the case, only calling into use the fine tips of the third and fourth pairs of thoracic limbs to hold fast its charge. When it fully retires into the case, the claws of the two posterior pairs of legs are pressed backwards against the lining membrane, so as still more effectually to secure its hold on the approach of danger.

Royal Society, Feb. 12.—"Note on the Synthesis of Formic Aldehyde," by Sir B. C. Brodie, Bart., F.R.S.

In a former note I communicated to the society the result of an experiment in which a mixture of equal (or nearly equal) volumes of hydrogen and carbonic oxide had been submitted, in the induction-tube, to the electric action. My expectation in making the experiment had been that the synthesis of formic aldehyde would be thus effected according to the equation $CO + H_2 = COH_2$. The only permanent gas, however, other than the gases originally present in the induction-tube, which appeared in the result of the experiment was marsh-gas. When a mixture of hydrogen and carbonic acid gas were similarly operated upon, the same hydrocarbon, together with carbonic oxide, was formed. I have now, however, succeeded, by a modification in the conditions of the latter experiment, in attaining the object which I originally had in view. Evidence of this is afforded by the following analysis:—The gas analysed was the result of submitting to the electric action equal volumes of hydrogen and carbonic acid. After removal from the gas of carbonic acid and carbonic oxide, and also of a trace of oxygen, 191·2 volumes of gas remained, in which were found at the conclusion of the analysis 2·6 volumes of nitrogen. Deducting this amount of nitrogen, 188·6 volumes of gas remain, containing the residual hydrogen in the gas, together with any gases besides carbonic oxide formed in the experiment. This gas was analysed by the addition of oxygen and subsequent detonation by the electric spark, the absorption of the carbonic acid by potash, and the removal of the oxygen over by pyrogallate of potash. The results of the analysis entirely concur with the assumption that the 188·6 volumes of gas were constituted of hydrogen, marsh-gas, and formic aldehyde in the proportions given below.

Hydrogen	183·2
Marsh-gas	0·2
Formic aldehyde	5·2
	188·6

The composition of 100 volumes of the gas being,

Hydrogen	97·14
Marsh-gas	0·10
Formic aldehyde	2·76
	100·00

Another experiment was attended with similar results, only that the proportion of marsh-gas was somewhat greater.

The result of this experiment may be considered to be given in the equation $CO_2 + 2H_2 = COH_2 + H_2O$. I have reason to believe that formic aldehyde is also formed in the reaction of hydrogen and carbonic oxide; and that the marsh-gas found (in both experiments) results from the decomposition of this substance, possibly according to the equation $2COH_2 = CO_2 + CH_4$. I do not now dwell upon this subject, as it is my intention very speedily to lay before the Society, together with other matters, the details of the various experiments which I have made in reference to it.

Geological Society Feb. 4.—His Grace the Duke of Argyll, K.T., F.R.S., president, in the chair.—The following communications were read:—"The Physical History of the Valley of the Rhine," by Prof. A. C. Ramsay, LL.D., V.P.R.S., vice-president. The author first described the general physical characters of the valley of the Rhine, and discussed some of the hypotheses which have been put forward to explain them. His own opinion was that during portions of the Miocene epoch the drainage through the great valley between the Schwarzwald and the Vosges ran from the Devonian hills north of Mainz into the area now occupied by the Miocene rocks of Switzerland. Then after the physical disturbances which closed the Miocene epoch in these regions the direction of the drainage was reversed, so that after passing through the hill-country between the lake of Constance and Basel, the river flowed along an elevated plain formed of Miocene deposits, the remains of which still exist at the sides of the valley between Basel and Mainz. At the same time the Rhine flowed in a minor valley through the upland country formed of Devonian rocks, which now constitute the Taunus, the Hundsruck, and the highland lying towards Bonn, and by the ordinary erosive action of the great river the gorge was gradually formed and deepened to its present level. In proportion as the gorge deepened, the marly flat Miocene strata of the area between Mainz and Basel were also in great part worn away, leaving the existing plain, which presents a deceptive appearance of having once been occupied by a great lake.—"On the Correspondence between some Areas of Apparent Upheaval and the Thickening of Subjacent Beds," by W. Topley, Geological Survey of England. The author referred to many instances in which beds have unequal development, being much thicker in some places than in others; and the main object of his paper was to show that such thickening and thinning of beds has an important effect in producing the apparent dip of overlying beds. The thinning of any one bed may have an appreciable effect in producing or increasing its own apparent dip; but where a whole series of beds thin constantly in one direction, the amount of the dip of one of the higher beds, due to the *sum of the thinnings of the underlying beds*, is often very considerable. It is generally supposed that the dip of any bed is due to great movements of the earth's crust; from the facts mentioned the author argued that our inferences as to such movements will vary according to the beds which happen to be exposed at the surface. It is evident, from the faults intersecting strata, that upheavals and disturbances have taken place; but unless we assume every bed to have been deposited on a perfectly horizontal plane, we cannot infer the amount of such upheaval from the present position of the bed. In all cases we must take into account the actual or possible thinning of underlying beds. The beds which support geological basins frequently thin towards the centres of those basins, thus producing, wholly or in part, the basined form of the strata. It was, however, shown that the beds of the basins themselves frequently thicken towards the centre of the basins.

Anthropological Institute, Feb. 10.—Prof Busk, F.R.S., president, in the chair.—The second part of the paper "Explorations amongst ancient Burial Grounds, chiefly on the sea-coast valleys of Peru," was read by the author, Mr. Consul Thomas J. Hutchinson. The paper treated of the burial grounds from Lima northwards, as did the former part of the paper on those from Arica to Lima. Mr. Hutchinson described a burial place with the Aymara name of Parará on the Oroya railroad at a station called Chonca, and at an elevation of only 2,750 feet above the level of the sea, and so named from its grinding stones used for bruising corn, numbers of which he amongst the cenotaphs. Those were said by Prof. Forbes to be used for cooking purposes, because the Aymaras are stated to have occupied a part of Peru of which the minimum elevation is 10,000 ft., and therefore where the boiling of water is a difficult matter to accomplish. The flattened and elongated skulls mentioned by Dr. Tschudi and Prof. Forbes were touched upon—an illustration of one of these from an elevation of 10,000 ft. above the sea being given. Mr. Hutchinson recommended a further and more extensive exploration of the mounds and Huacas in Peru to illustrate the rich treasures of archæology with which that country abounds.—A joint paper by Mr. Tyrwhitt Drake and Mr. A. W. Franks was read, on skulls and implements from Palestine.

Photographic Society, Feb. 10.—James Glaisher, F.R.S., president, in the chair.—A special general meeting was held to decide whether two new laws, previously proposed, should be adopted, or whether the Council's amendment to appoint a committee to revise the laws generally be accepted. The Council's amendment was lost. The anniversary meeting of the Society was held afterwards, when the balance-sheet, showing an improved financial position, and the report of the Council, were read and adopted. The President and Council, interpreting the rejection of their amendment as a vote of want of confidence, then tendered their resignations, which were accepted.

MANCHESTER

Literary and Philosophical Society, Jan. 19.—Microscopical and Natural History Section.—Mr. Joseph Baxendell, F.R.A.S., vice-president of the Section, in the chair.—Mr. Joseph Sidebotham, F.R.A.S., read a paper on "The similarity of certain Crystallised substances to Vegetable forms." The author called attention to the formation of verdegris on insect pins, in old Entomological collections. This substance makes its appearance where the pins pass through the thorax of the insects, and in length of time grows into a considerable mass of flocculent matter, of a brilliant green colour, and often breaks up the insects and also destroys the pins. It consists mainly of acetate or formiate of copper in combination with fatty or oily matter. On examination of various specimens under the microscope, they were found to present a great variety of forms, filamentous and ribbon-like structure, often resembling various fungi, in some cases so nearly, that it was difficult to believe that the fibres and fruit-like forms are not really organic bodies. The author expressed his opinion that these bodies were simply crystals, modified in their formation by the oil contained in the insects, with which the crystals are in some way combined. Some of the specimens exhibited were taken from insects collected twenty-five years ago.

Liverpool

Geological Society, Feb. 12.—Mr. T. Mellard Reade, F.G.S. read a paper containing a series of novel investigations on the action of tides on the sea-bottom. Applying a formula used by civil engineers, the result of practical observation in tidal estuaries, to the observed currents at the surface at various points in the St. George's and English Channels, it was proved, by comparison with experiments instituted for ascertaining the moving powers of running water on materials of various specific gravity and bulk, that, conditions being otherwise favourable, tidal currents were capable of destructive erosive action on the sea-bottom. Mr. Reade then entered elaborately into the phenomena of the tides in the Irish Sea, in the English Channel, and surrounding seas, using Captain Beechey's admirable observations for this purpose. Mr. Reade infers from a consideration of a variety of circumstances that the materials of the Irish Sea bottom are principally composed of re-arranged glacial drift, either eroded off the bottom or off the coast by the sea itself, or poured into it by the many rivers in the north-west of England, south of Scotland, and west of Ireland, draining vast basins mostly covered by glacial clays and sands. These materials, notwithstanding the oscillatory character of the tidal streams, have in the main a slow, progressive motion down channel, and out as far into the Atlantic as the little Sole Banks. Clear cases of the erosive action of the water on the bottom were then given. It was shown that there are pits or gullies excavated in the bottom in both the English and Irish channels, and that these depressions have generally their major axes conformable in direction with the set of the stream tide; and that the contour lines of the bottom approximately follow the same direction. The most remarkable of these excavations is the North Channel Gulley, off the coast of Wigtonshire, twenty miles long, one mile wide, and from 400 ft. to 600 ft. deeper than the surrounding bottom, and which the strong tide existing there has either partially or wholly excavated, and now keeps open. In conclusion, Mr. Reade expressed his conviction that the diurnal and semi-diurnal movement of the tides, acting down to the profoundest depths of the ocean, accounts for the preponderance of life in it over that exhibited by the fauna of the Mediterranean.

Edinburgh

Royal Society, Feb. 16.—Sir William Thomson, president, in the chair.—The president read obituary notices of deceased Fellows of the Society.—The following communications were read:—On the Kinetic Theory of the Dissipation of Energy, by Sir W. Thomson.—On the Electric Conductivity of Iron at a Low Red Heat, by Prof. Tait.—On the Stresses due to Compound Strains, by Prof. C. Niven, communicated by Prof. Tait.

Glasgow

Geological Society, Jan. 15.—Mr. E. A. Wünsch, vice-president, in the chair.—Mr. R. L. Jack, of H.M. Geological Survey, read a paper on a Boulder-clay, with broken shells, in the lower valley of the River Endrick, near Lochlomond, and its relation to certain other glacial deposits in the same neighbourhood. The author stated that the elevation of Lochlomond above the sea is so trifling that there is no difficulty in classing it with the sea-lochs that indent the western Highlands. A depression of 20 feet, or the removal of the superficial deposits traversed by its short outlet, the Leven, on its way to the Clyde, would restore it to its former condition. He then called attention to a deposit which he had observed in the course of his work on the geological survey near the south-eastern angle of Lochlomond, and whose relation to the already-known members of the glacial series seemed to deserve particular attention. The deposit is a true typical till, in every respect similar to the old boulder-clay or till of the Lowlands of Scotland. In a matrix of stiff unstratified clay, brown in colour, like the subjacent Old Red sandstone rock, are scattered stones of various sizes, blunted, smoothed, and marked with striations in all directions, but most frequently in the direction of their longer axis. It presents, however, one remarkable peculiarity that distinguishes it from the common till—it contains worn and broken fragments of marine shells. Though he had not found any clear instance of an older deposit below this shelly till, he believed its place was above the old boulder-clay, and that it was also the product of land-ice. He believed the till to be the product of land-ice—the *moraines profondes* of a large glacier which filled up the lake, covered the islands, and climbed the rising ground between the Leven and the Endrick to the height of at least 320 feet. This glacier, in all probability, existed during the latter portion of the period which preceded the "great submergence" of the land.—Mr. John Young read a paper on the occurrence of a bed of highly indurated Sandstone, with water-worn quartzite pebbles, interstratified with the trap of the Campsie Hills. The bed is probably of lower carboniferous age, and indicates one of those periods of repose between the great outbursts of igneous rock matter of which the Campsie Fells are principally built up.

Paris

Academy of Sciences, Feb. 9.—M. Bertrand in the chair.—The following papers were read:—On *Balistique intérieure*, by General Morin. This was a paper on the various forces acting on a projectile whilst still in the bore of a gun.—On the devitrification of glass, by M. Eug. Peligot. The author decides that, contrary to the received opinion that this effect is due merely to a crystallisation of the glass, it is due to the formation of a definite silicate having a formula corresponding to that of a pyroxene.—On the action of water on lead, by M. Balard.—New clinical and experimental researches on the movements and repose of the heart, and on the mechanism of the passage of the blood through its cavities when in the normal state, by M. Bouillaud.—On the preservation of vines threatened by *Phylloxera*, by M. de la Vergne.—On the problem of three bodies, by M. E. Mathieu.—On the resistance of glass tubes to rupture, by M. L. Cailletet. The author finds that a tube stands pressure from the outside better than from the inside. The pressures, however, which a tube can stand from the inside are very great. One of 9 mm. internal diameter and 1 mm. thickness, containing 6·9 c.c., was submitted to an outside pressure of 460 atmospheres, without injury, and subsequently to an internal pressure of 104 atmospheres, when it burst.—On the use of a double refracting prism for determining the axes of ellipses, by M. Jannetaz.—On some new bands produced in the absorption spectrum of chlorophyll by reagents containing sulphur, by M. J. Chautard.—On a new process for preserving wood, by M. Hatzfeld.—On the hardness and density of carbon obtained from pure sugar, by M. F. Monier.—On the flight of birds, by M. E. Bertin.—On an electric fire-alarm, by MM. A. Joly, and P. Barbier.—On the measurement of heat, by M. G. West.—On a case of monstrosity, &c., by M. Clandot.—Theorems concerning algebraic equations, by M. F. Lucas.—On the impossibility of certain double equations, by M. A. Genocchi.—On the conditions necessary for a conic with a curve of any order to have a contact of the fifth order, by M. Painvin.—On the chemical characteristics of the uredo of maize, &c., by M. Hartsen.—On the consecutive effects of the removal of the mammæ in certain animals, by M. de Sinéty.

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THURSDAY, FEBRUARY 26, 1874

THE ROTHAMSTED AGRICULTURAL INVESTIGATIONS

IT has become a trite remark, that while both the progress and teaching of Science are fostered in most educated countries by the care of the State, they mainly depend in our own country on the exertions of private individuals; this fact is perhaps, however, more strikingly seen in the case of agriculture than in any other instance. The traveller in Germany will find scattered over the country, some forty Experimental Stations and Agricultural Academies, establishments which are devoted to the investigation and teaching of scientific agriculture and are maintained by their respective States. The German farmer has thus the means of becoming acquainted with the true science of his business, and provision is at the same time made for the investigation of the various problems with which his work abounds. In England the state of things is, alas, wholly different. We have just one college—that at Cirencester, devoted to the teaching of scientific agriculture, and one Experimental Station—that at Rothamsted. There is indeed some experimental work done by local Farmers' Clubs, but this is generally only with the object of comparing the effects of the various manures that are in the market, and with no scientific aim or result. Yet England preeminently needs the help of Science to direct economically her vast system of agriculture. The art of agriculture is here in a higher state of development than on the continent. More capital is here invested in the land; more attention has been paid to tillage, to artificial manures, and to the breeding and feeding of stock. The British farmer succeeds because he is a practical man, and has good common sense, and the enterprising spirit of his race; what might he not do if he thoroughly understood the principles which underlie his art?

If we have but one agricultural station in England we have at least reason to be proud of it. The work done at Mr. Lawes' estate at Rothamsted is not to be equalled by that of any of the foreign stations; indeed, in several departments of investigation it might safely challenge a comparison with their united efforts. This excellence has arisen from the systematic and thorough manner in which the subjects taken up have been treated. We cannot better illustrate this than by referring to the last contribution from Rothamsted, a report by Messrs. Lawes and Gilbert on the growth of barley.*

In one of the experimental fields barley has been grown for twenty years, and the experiment is still progressing. The field is divided into plots of about one-fifth of an acre; some of these have never received any manure during the twenty years; the others receive some one or more of the food constituents which barley requires. Thus one is manured with phosphates, a second with alkalies, a third with ammonia, a fourth with ammonia and phosphates, a fifth with ammonia, phosphates, and alkalies, &c. The same manures are always applied each year to the same plot. At harvest the crops are carefully weighed, and are then analysed in the laboratory under the superintendence of Dr. Gilbert, the amount of dry matter, ash, and nitrogen being determined. The advantages of this systematic mode of experimenting are very great. Carried on in the same manner for so many years, these experiments answer questions relating to the exhaustion of soil, to the permanent effect of manures, to the effect of season upon the produce. With the aid of the laboratory investigations they teach us what proportion of the various ingredients supplied in the manure is recovered in the crop, and how the composition of the plant is affected by the various conditions of the soil. In conjunction with analyses of the soil and of the drainage water, we learn what becomes of the manures applied, how deeply they have penetrated into the soil, what is the loss suffered through drainage, &c. A single field experiment thus thoroughly and patiently carried out touches half the domain of agricultural chemistry, and supplies information of the most solid and valuable kind.

The value of every trustworthy investigation is increased as others are completed which compare with it; the work at Rothamsted thus derives an additional value from its extent. During the last thirty years Messrs. Lawes and Gilbert have investigated in the manner described all the principal farm crops, experimenting both on each singly, and also on their behaviour when grown in rotation. As the results are gradually published, and we are able to compare the behaviour of different crops grown on the same soil, with the same manures, and in the same seasons, the special characteristics of each crop become plainly shown by contrast with its fellows, and we gradually learn the part which each is fitted to play in a scientific system of agriculture.

Nitrogenous manures are of primary importance if luxuriant cereal crops are to be raised, the natural supply of combined nitrogen from the atmosphere being very small, and the crops in question having little power for assimilating the forms of nitrogen chiefly present in the soil. Nitrogenous manures are, moreover, as every farmer knows, very expensive, and it is a matter of great importance to employ them in the most economical manner. Messrs. Lawes and Gilbert, knowing the composition of the manure that has gone on to their fields, and the composition of the crops that have been carted off, can tell exactly what proportion of the nitrogen applied has been assimilated by the plant. They find, on an average of twenty years, that wheat assimilates about 45 per cent. of the nitrogen in a spring dressing of nitrate of sodium, and about 33 per cent. in the case of an autumn dressing of sulphate of ammonium, and only 14¼ per cent. of the nitrogen supplied by farmyard manure. With barley, the proportion assimilated is rather greater, being 49 per cent. for a spring dressing of ammonium salts. The question as to what becomes of the large proportion of unused nitrogen is clearly of the highest importance. Analyses of the soils, and of the drainage water, throw much light on the subject. The soils of the wheat field have been analysed down to a depth of 27 inches. A considerable part of the missing nitrogen is found to be actually present in the soil, but since it has scarcely any effect on the crops, it is apparently in some state of combination unsuitable for the plant's use. A still larger portion of the nitrogen is, however, not to be found in the soil, but the examination of the drainage waters from the different plots exhibits so large a content of nitrates, that calculation leads to the belief that in the case of ammonium salts and nitrate of sodium the loss of nitrogen chiefly takes place in this manner. Chemists knowing that ammonia is readily absorbed and firmly held by soil, had never anticipated that so considerable a loss might occur by drainage. It plainly appears, however, from these results, that ammonia when applied to the soil is quickly converted into nitric acid, and in heavy rains may be easily washed out. During autumn and winter there is little evaporation from the soil, and no consumption of water by a growing crop; as soon therefore as the surface soil is saturated most of the subsequent rain-fall will pass into the subsoil, or find its exit through the drains. The authors calculate that if the drainage water contains 1 part of nitrogen in 100,000, and many of the waters analysed were much richer, there will be a loss of 2·26ths of nitrogen (equal to about 23 lbs. of guano) for every inch of rain that passes beyond the reach of the roots. It is evident, among other conclusions from these important facts, that ammonia should only be applied to the land in the spring, when the crop is able to make immediate use of it. It may also be found that on gravelly and sandy soils, which have little power of holding water, organic forms of nitrogen, as rape cake and farm-yard manure, may be more certain in their effects than ammonia or nitrates. The organic manures being only slowly rendered soluble in the soil, can suffer comparatively little loss from sudden rain. The subject of the economic application of nitrogen is being further investigated at Rothamsted.

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* "Report of Experiments on the Growth of Barley for twenty years in succession on the same land," by J. B. Lawes, F.R.S., F.C.S., and J. H. Gilbert, Ph.D., F.R.S., F.C.S.—*Journal of the Royal Agricultural Society*, 1873, 89 and 275.

We have no space to do more than allude to the researches which have been conducted at Rothamsted in the department of animal chemistry: the experiments on the fattening of stock, and on the composition of the carcase produced, have been equally important in their results with those field experiments we have referred to. Of the indebtedness of Science to Mr. Lawes' unique and costly experiments we need not speak, the facts are so plain that they speak for themselves. Nor need we state the moral. The addition to the national wealth which has accrued from the discoveries made by Mr. Lawes is already enormous. It must be borne in mind that this benefit has arisen from *accidental* researches, for Mr. Lawes was not compelled to take them up, nor is he bound to continue them. Now if such work is not national work, The Royal Observatory ought to be shut up to-morrow, for the work done there is not one jot or tittle more important.

DR. LIVINGSTONE

THE telegram, dated at Aden on the 23rd of this month, announces that Lieut. Murphy is bringing the body of Dr. Livingstone down to Zanzibar, while Lieut. Cameron has passed onwards to Ujiji to recover Dr. Livingstone's papers and to continue his work. The story of those faithful negroes having carried the body of their beloved chief over hundreds of miles is one of the most romantic in history, and is a fitting close to the noble life of the great explorer. Dean Stanley, we are informed, has proposed that the remains of Livingstone shall find a last resting-place in Westminster Abbey. Yesterday (the 25th) was the last day on which instructions could be sent out by telegram touching the disposal of the body. We cannot believe that the necessary orders have not been despatched; for the wishes of the country are well known, and have been sufficiently expressed. With the body will arrive all Livingstone's faithful followers, who were engaged on the understanding that they were to receive a certain fixed monthly payment. There were Chumah and the two or three other men who had been with him since 1865. There were Jacob Wainwright and the other Nassick boys, and the men sent up to Unyanyembe in the summer of 1872; and there were the men engaged in the interior by Dr. Livingstone himself. A sum of about 1,000*l.* will be required to pay off these loyal and faithful servants of Her Majesty's Consul. Yesterday was the last day on which an order for the payment could be sent out. Has this been done? The people of England have a right to an answer, and an immediate answer, for if the Government hesitates, the country will never allow this disgrace to come upon it.

As soon as the full details arrive by the next mail, it will be fitting that we should give our readers a memoir of the illustrious martyr to Science who has passed away. But now we desire to know—and all England will join with us in the inquiry—whether orders were sent out by telegram on the 25th, respecting the disposal of Dr. Livingstone's body, and the payment of his followers, the devoted servants of Her Majesty's Consul?

Lieut. Cameron, in conducting the search expedition, has suffered terribly from fever and ophthalmia, and has been obliged to incur heavy expenses. But he has displayed the best qualities of an explorer. He is a good manager of natives, an excellent walker, an accurate astronomical observer, a linguist, and a man of indomitable perseverance. He is now pressing forward on a perilous and important duty, and we trust he will carry with him the generous sympathy of the Geographical Society, and of the public generally.

POST-TERTIARY GEOLOGY

The Great Ice-Age and its relation to the Antiquity of Man. By James Geikie. (W. Isbister and Co. 1874.)

I.

EVERY field-geologist, who works in northern latitudes, soon comes to know what is meant by Drift. In his attempts to trace the superposition and run across the country of the solid rocks, he is always sooner or later brought up by coming across masses of stony clay, gravel, and sand, which bury them to greater or less depths, and more or less completely hide them from view. These superficial accumulations lie indifferently on all members of the bedded formations; they occur now in detached patches, and now spread like a pall over vast tracts of country.

In the latter case it is clear that they would soon make their presence felt by the way in which they effectually mask the geological structure of the ground they cover; it was impossible, therefore, that they could be for long ignored altogether, but they seem for a time to have been looked upon as something very inferior in interest and importance to the older and more regular formations, in some cases, perhaps, as little more than troublesome hindrances to the making of a good geological map. Thus it came about that deposits of this class were lumped together under the comprehensive title of Diluvium or Northern Drift: and that, in the few cases where any geologist thought them worthy of more than a simple recognition, the explanations offered of their origin were crude and unsatisfactory. Some of these explanations we may just glance at. In the days when evolution and continuity were doctrines yet undreamed of, it was imagined that between any two consecutive geological epochs there intervened a period of chaotic turmoil, one result of which was, that a clean sweep was made of the life of the epoch that had just closed, and the ground prepared for the introduction of the life of that which was to follow. Supposing this to have been the general course of past events, some referred diluvial accumulations to the series of convulsions which came in like a great gulf between the age of man and the last of geological eras. It could not be reasonably objected to such an hypothesis that it called to its aid agencies the like of which we had never seen, and the like of which, as far as our knowledge of the economy of nature went, it was most improbable had ever been in operation; there was the ready answer, that during periods which were essentially abnormal, anything was possible. This made such explanations easy to frame and easy to uphold, and they commended themselves readily to the indolence of mind and impatience of accurate thought from which few of us are altogether free. The same may be said of the notion suggested by, but we cannot say based upon, the phenomena of the great ocean wave of earthquakes, "that somehow and somewhere in the far north a series of gigantic waves was mysteriously propagated," which "were supposed to have precipitated themselves madly on over mountain and valley alike, carrying along with them a mighty burden of rocks, stones, and rubbish," and that by this means the piles of diluvium had been heaped up. Again, the name diluvium was founded on the idea that its deposits were the relics of Noah's flood; and the notion that we had in them a proof of the accuracy of the Biblical record was so very welcome, that it was accepted and stuck to in spite of the absence of evidence in its favour, and so contributed, perhaps as much as anything else, to postpone the true solution of the problem.

But by degrees light began to dawn on the subject. Playfair had attempted to turn the attention of geologists to the proper quarter, when he suggested that the most powerful agents which Nature employs for the moving of rocks are the glaciers; but his hint lay for a while unheeded. In 1837, Agassiz arrived at the conclusion that the glaciers of the Alps had been formerly far larger than at present. He had studied the smoothed and furrowed surfaces which occur everywhere below glaciers, and had found that the rocks displayed markings exactly identical with these far beyond the range of the present ice. He explained his views to Buckland, who then saw the meaning of certain surface features which he had observed, but had not previously understood, in the British Islands. The two geologists visited Scotland together in 1840; found over the length and breadth of the land scorings and polishings which ice—and, as far as their knowledge went, nothing but ice—could have made, and came to the conclusion that the whole country had been once swathed in one widespread ice-covering. About the same time Sir Charles Lyell attributed the formation of portions of the Scotch drift to the action of land-ice.

The right clue was now found, and it only remained for others to follow it up. A great step was made by Prof. Ramsay when, some ten years later, he deciphered the story written on the Drift-beds of North Wales, and determined the broad succession of physical changes that had led to their formation. He pointed out that there had been two periods of cold, the first of intense severity, and the second less rigorous, and that between the two there came a milder interval, during which depression brought the sea up the flanks of the mountains to a height of 2,300 feet above its present level.

Still however the importance of the Drift was far from being fully recognised. For many years no notice whatever was taken of it on the maps of the Government Survey; and when at last it met with a tardy recognition, Drift was still for a while Drift "and it was nothing more," a something agriculturally important and therefore not to be passed over by the economic geologist, but hardly a great formation with a story to tell as long, as varied, and as interesting, as any that geology had hitherto revealed to us. It is significant that ever so eminent a pioneer as Prof. Ramsay did not deem these deposits worthy of more than incidental notice in his otherwise exhaustive Memoir on the Geology of North Wales. The Drift in fact was somewhat on the position of a *nouveau riche*, who is trying to work his way into "society," and it had up-hill work before it was admitted into the exclusive circle of the old respectable formations.

But its turn came at last, and amid the band of geologists, who have helped in the work of securing for it the attention to which it is fairly entitled, the brothers Geikie occupy prominent places. The one gave us in 1863 his paper on the phenomena of the Glacial Drift of Scotland, in which he offered a masterly summary of all that was known on the subject up to the date of its publication, and settled for ever the claim of land-ice against ice-bergs to have been the agent that formed the Scotch Till; and now the other comes before us with the goodly volume, whose title stands at the head of this article, and which can be cordially recommended both to the geologist and the general reader. Its account of the labours and conclusions of previous workers is all but exhaustive, but it is far more than a mere *résumé*; a long practical acquaintance with Drift-deposits has enabled the author to add materially to our knowledge of the course of events that accompanied their formation, and in some cases has led him to demur to views hitherto all but universally accepted; and his own contributions and criticisms are as remarkable for the boldness of their originality as for the soundness of the reasoning by which they are upheld. At the same time the explanations are so full, and the method of handling so free from technicality, that with a moderate amount of attention the book may be understood, and its reasoning followed, by those who had previously little or no geological knowledge.

A large part of the work is taken up by a careful and detailed description of the Drift-beds of Scotland, which country the author has chosen as a typical area. A better selection could not have been made, for in no country perhaps are these deposits so largely developed, and nowhere have they been so elaborately worked out as by the distinguished band of geologists who have made Scotch glacial formations a special study.

The first seven chapters are devoted to a description of the Till, the lowest member of the Scotch Drift; and an explanation of the line of reasoning that has led geologists to acquiesce almost unanimously in the opinion that it was formed on land beneath a sheet of ice, which, during a period of intense cold, overspread the whole country, and pushed its way far out over the shallow bed of the surrounding sea.

So far the author has only been repeating and enforcing the conclusions of his predecessors, but in chaps. 11—14 he enters on ground which is all but his own. It has been long known that layers of well-bedded sand and gravel occur in the heart of the Till, and between it and the older rocks. These deposits however are local and of small extent, or had been detected only in borings or underground workings, and had had comparatively little attention paid to them. Mr. J. Geikie has for the first time pointed out that in spite of their small development they are full of meaning; and that, when this meaning is realised, the fragmentary nature of their occurrence is only what is to be expected, and that the wonder is, not that there is so little of them left, but that any of them should have survived to tell the tale which he has so ably extracted from them. And the story they tell us is this. They are evidently the products of running water, alluvial or lacustrine deposits mostly; now former observers had realised in a vague sort of way that they were a proof of changes of climate, which permitted water to flow over what had been before an ice-bound waste, but we have now clearly brought before us that the abatements of the intense cold, which these beds indicate, were not local and temporary, but wide-spread and of long duration, and that they recurred several times during the period of the first great glaciation. Thus we are led to see that the first subdivision of the great ice-age was not one dreary unbroken lapse of Polar winter, but that it included mild intervals, when the ice shrank back, possibly disappeared altogether, when vegetation reappeared, and when herds of the great mammals returned from the southern retreats into which they had been driven during the most intense phases of the cold. And these facts enable us to realise more vividly the immense lapse of time represented by one division alone of the Glacial Formation. For if "we consider that the succession of changes happened not once only, but again and again, we cannot fail to have some faint appreciation of the lapse of time required for the accumulation of the Till and the Interglacial Deposits." Lastly Mr. Geikie has pointed out that these alternations of intense glaciation and comparative mildness are fully in accordance with the theory so ably expounded by Mr. Croll, that changes in climate are due to the combined effect of the Precession of the Equinoxes and variation in the eccentricity of the earth's orbit, a theory which he has lucidly expounded in chaps. 8—10.

We next come to certain deposits, the meaning of which seems first to have been clearly read by the author of the present work. At last the conditions which gave rise to the Till began to pass away and the climate to improve slowly, and the great glaciers ceased to be confluent; a depression of the land ensued so that the sea followed the retreating margin of the ice; but after a while, perhaps owing to an upward movement, the glaciers terminated on dry land. Mountain peaks now began to rise above the ice, and showered down on to its surface loads of *débris* torn from their exposed faces by frost. As the burden was shot over the ends of the glaciers, it gave rise to huge heaps of morainic rubbish, which at first fell into the sea, and afterwards, as the ice drew back, was shed upon the land. In this way were formed the subdivisions of the glacial formation which the author has distinguished as Boulder Clay and Morainic Rubbish. During this period the author believes that many of the erratic blocks, which form so conspicuous a feature among glacial deposits, were stranded from the ice-sheet as it drew back; and he gives good reasons for preferring this explanation to the older notion, which supposed these travellers to have been dropped from ice-bergs during the submergence which came a little later on.

As the climate gradually improved, the melting of the ice swelled the rivers and gave rise to mighty floods, which thundered down the narrow mountain glens, sweeping before them portions of the Till and Morainic rubbish, and, when they emerged on the open valleys of the lowlands, spread out the worn and rounded materials in broad sheets of gravel. One point here we must pause specially to call attention to. Geologists had long been aware of the disappearance of the great ice-sheet and of a gradual submergence of the land which followed it, but we now learn that the first of these events had made considerable progress, perhaps had been completed, before the second had fairly set in.

The first act of the drama (we are looking at may be said to close here; the second opens with the commencement of the submergence just mentioned. The land began to sink and went down till the sea reached to some 1,200 feet, perhaps in some cases to nearly double that amount, above its present level; and as each of the previously formed Drifts, Till, Boulder Clay, Morainic Rubbish, and Gravel, was brought under the action of the waves, they sifted and sorted it, washing out the fine dirt, and rounding and reducing in size the pebbles; and in many cases the clean gravel and sand so formed were piled up along each successive coast line in mounds and long ridges, which still retain the distinctive outline originally impressed on them by wave- and tidal-action. These hummocky piles and ridges are known as Kames or Eskers. That many Kames owe their present shape in the main to the direct action of the sea alone there can scarcely be a doubt; we find them sometimes for instance enclosing hollows *without any outlet*, a little tarn or peat moss occupying in some cases the central depression, and in such a case they must have been piled up by shifting currents, for in no other way could the closed basin in the middle have been produced. But many so-called Kames are only the remnants of large sheets of gravel, the greater parts of which have been carried away by denudation. It would be better perhaps to restrict the term to those mounds which were piled up originally very much as they stand now. Even with this limitation, however, Kames are plentiful enough, and they are found in greatest abundance at the spots where the currents tending to their formation must have prevailed as the land went down; in cols, then straits connecting opposite firths from which tides would flow in on either side; at the openings of mountain valleys, where the stream with its gravel burden was then met by the incoming tide; on low plateaus lying between what were then estuaries, over which opposing currents would sweep each high tide; and in other similar situations.

But were the Kames formed as the land sunk, or during its subsequent emergence? One would be inclined to say during the latter period, for any heaps piled up by the incoming sea would be liable to be swept away when they again became exposed to wave-action as the sea retreated. Nevertheless, Mr. Geikie—and he is supported by other observers in his opinion—holds that it was during the period of depression that kame-building went on, for the following reasons. The material of the Kames is, for the most part, fine and well rounded, and it is a very rare thing to find a large angular boulder in the heart of a Kame; hence it is believed that during the formation of the Kames the climate had so far mended that glaciers no longer existed, and that therefore there were no icebergs to strew the sea-bottom with travelled blocks. Erratics are, however, common perched on the outside of the Kames, and hence it is concluded that at some point in the period of the submergence cold again began to come on, that glaciers reappeared and gave rise to icebergs which bore away these blocks and dropped them where they are now found. That there was a return to cold conditions we know from other evidence, and with the exception of the difficulty just mentioned, which after all is not very serious, for the Kames we have may be only the relics of a body originally much more numerous, the explanation accords well enough with our knowledge of the facts. But the thought crosses the mind that it is not very often that we have an opportunity of seeing into a Kame, or rather, that we see the outside much oftener than the inside, and that this possibly may be the reason why erratics seem to be more plentiful in the one place than the other.

A. H. GREEN

(*To be continued.*)

OUR BOOK SHELF

Mineralogische Mittheilungen, 4th Heft. (Vienna, 1873.)

THE present number of the *Mineralogische Mittheilungen* forms the 4th volume of the series, commenced in 1870. It is published under the auspices of the K. K. Geol. Reichsanstalt, and in connection with their quarterly Jahrbuch, but forms properly in itself an independent journal. It is probably the only periodical devoted exclusively to mineralogy, and thus already occupies an important place among German scientific serials. The list of papers which appear in this number will give a good idea of the position which the journal occupies:—Mineralogical observations in the Argentine Republic, by H. Stelzner (embracing—minerals occurring in the granite quartz masses of Cordoba; minerals associated with the granular limestone; also analyses and special description of Triplite, Jamesonite, Enargite, Linarite, &c.); eruptive rocks of Banat, Hungary, by Niedzwiedzki; crystallised magnesite from the Northern Alps, by J. Rumpf; mineral observations from the Bohemian Forest, by Helmhacker; brief notices of various minerals, Grunochite, an optically uni-axial diamond, native copper, Roselite, &c. The number opens with a short obituary notice of Naumann.

The Mittheilungen are under the charge of Prof. Tschermak, so well known through the scientific world for his many and valuable contributions to mineralogy and lithology.

Prof. Tschermak occupies the position of Director of the Royal Mineralogical Museum of Vienna, a collection which in beauty and completeness is not inferior to that of the British Museum, while it surpasses the latter in the arrangement of the objects exhibited, especially in reference to the good of the general public. The Museum has been much enriched within the last four months by the acquisition of a number of interesting things from the Vienna Exposition. The collections are opened two mornings in the week, for the benefit of the public, opportunities which are well made use of, while the Cabinet is accessible on every week-day to those who are there engaged in regular work. The results accomplished in the Mineralogical School thus formed in connection with Prof. Tschermak and Dr. Schrauf do a considerable part toward supporting the *Mineralogische Mittheilungen*.

The Geological Reichsanstalt, now under the direction of Franz Ritter von Hauer, has carried on its work for twenty-seven years. The results are not only of the greatest importance in developing the resources of the Empire itself, but, as the work engages the best talent, and is consequently carried on according to the highest standard of pure science, the yearly labours are adding vastly to the fund of geological knowledge, and helping to solve many of the difficult problems of the science. The building occupied by the Reichsanstalt contains the various working-rooms, and the extensive collections, complete naturally in all matters pertaining to Austrian Zoology. The immediate results of the survey are first made known in the Evening Sessions, which are held on the first and third Tuesday of each month from November to May. They are attended regularly by some thirty or forty of those interested in such subjects, and form a pleasant opportunity for those of common interests to meet together informally. The stranger is continually impressed with the active spirit, and especially with the community of feeling, among the scientific men of Vienna; the latter has undoubtedy great influence in giving the city the prominent position which it occupies among the different scientific centres.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Zoological Nomenclature

FROM time to time an idea is started that Zoology is breaking down under the weight of its synonymy. With entomologists I have frequently contended that so far from this being the case, there is, on the contrary, an almost marvellous agreement in the generic and specific names used, especially when we consider the extent of the bibliography and the vast number of the species. After reading in NATURE, vol. ix. p. 258, Mr. Wallace's review of Dr. Sharp's pamphlet, I bethought myself of comparing the two best known catalogues of European Coleoptera, viz., Schaum's, published at Berlin, in 1862, and De Marseul's, at Paris, in 1866. Perhaps the results of the examination of the first six families in the two works will suffice. It must be premised that Schaum's is strictly confined to European species, while De Marseul's embraces as well those of the "basin of the Mediterranean in Asia and Africa." As to the genera, in the Cicindelidæ there are two in each. In the Carabidæ, Schaum has 98 genera and de Marseul 118; of the latter four are not adopted by Schaum and the remainder are extra-European. In the Dytiscidæ Schaum has 15 genera, De Marseul 17. In the Gyrinidæ there are two genera in Schaum, and three in De Marseul, the third being extra-European. Hydrophilidæ not having the same limitation in the two works, I take the Palpicorn families in which that group and the Sphæridiidæ are included. There are 22 genera in each catalogue, but Schaum and De Marseul each ignore a genus adopted by the other, and a third name, Cyllidium, is preferred by the French author to the earlier one of Chætarthria. As to the species, seeing that Schaum has about 1,580 in the families mentioned above, and De Marseul 2,640, it would not be easy to compare them in a definite form; but taking Cicindela, the second genus of the two catalogues, the first having only one species, which is, I conceive, a fair example of the others, if indeed it has not had more than its share of varieties elevated to the rank of species, we find the 26 species in Schaum identical in names with the same species in De Marseul, except two varieties or species, and a synonym given with a ? by Schaum, which is the right name according to De Marseul.

I would venture to suggest that the synonyms which look so formidable to some of our friends, are principally due to the writers of local faunas, or in some cases to specialists, and that such names have, as a rule, never been adopted, and practically offer no hindrance whatever to the naturalist. A species may be described by an author who is ignorant that it has been previously described, but this is an evil which it is sometimes impossible to avoid, as in the case of almost simultaneous publication; but in due time the later name is relegated to the list of synonyms and gives little further trouble. It does not seem to me that any change or additions to the present rules of nomenclature are needed. Naturalists very soon decide on the relative value of names, but always with due regard to the law of priority; it is a misfortune, perhaps, that this law is sometimes pushed too far, as in the case either of forgotten authors, or of doubtful descriptions. The alteration of trivial names from two authors using the same word is a case of very rare occurrence.

FRANCIS P. PASCOE

The so-called "Meteor-cloud" of Feb. 5

YOUR correspondent, Captain S. P. Oliver, appears to have been mistaken as to the character of the phenomenon seen by him on February 5, and noticed in NATURE (vol. ix. p. 313). At the hour he has indicated, the somewhat rare phenomenon an auroral arch was formed, which remained visible for about half an hour, and is doubtless the luminous "meteor cloud" seen by him. The description Captain Oliver has given of it is sufficiently accurate, though he does not mention that it drifted slowly southward, a well-known characteristic of the phenomenon. Its direction was of course at right angles to the magnetic meridian, and its position in the heavens, as seen from this locality, was more northward than that observed by your correspondent. During the whole time that I observed it, the arch crossed some portion of the constellation Ursa Major, the star δ Ursæ Majoris being in its midst when first seen, and the entire arch having retreated southward as far as ζ Ursæ Majoris before it disappeared. It was of uniform breadth and intensity, and spanned the sky from west to east (magnetic), passing not much to the north of the zenith. Although I have been fortunate enough to have seen auroral arches upon several occasions, and once succeeded in obtaining the spectrum, I have never seen a brighter or more complete arch than this one; but what made it quite unique, at least as far as my experience goes, was the fact that the ordinary aurora with a well-defined "dark segment" was visible in the north-north-west at the same time, from which, at an earlier period, brilliant streamers had proceeded. There were therefore two parallel arches of light at an interval of perhaps 50° from one another, which the slow movement of the upper one gradually increased. The night was remarkably clear, and the zodiacal light had been plainly visible earlier in the evening.

JOHN J. PLUMMER

The Observatory, Durham, Feb. 21

Aboriginal Australian Artists

I NOTICED, in one of your latest papers, that some of your readers doubted the ability of Australian, or other low savages, to sketch in the manner of the Vezère people, and I made a copy of a few sketches still found in this neighbourhood engraved on rocks. They consist chiefly of fishes, whales, birds, and a few men; the execution is not so good as when the figures are scratched on blackened bark. I also send you a photograph of a carving in fossil coral from New Guinea. H.M.S. *Basilisk* has not long ago returned from New Guinea, and brought some splendid weapons, &c.; also one of the Papuan pigs, which they brought for our collection. It is the most intelligent pig I have ever seen, follows me like a dog, and goes up to the very top of the Museum building, which is about 80 feet high.

I noticed the, to me, wonderful remark about a scarcity of skeletons of large carnivora in European museums, and I am glad to say that we possess two tigers, two lions, wolf, hyæna, three grey seals, two large sperm whales, 70 and 35 feet in length, many small birds, dugongs, &c. &c. The sum total of our skeletons, all mounted, is more than 150; with few exceptions all articulated on the premises by one man, who has never been out of Sydney in his life. If our Government grant some extra money for cabinets, I think we shall be able to astonish the people on board the *Challenger* when they come here, because half our Australian fossils and minerals cannot be exhibited for want of the necessary cases.

GERARD KREFFT

P.S. The trustees have had so many applications for *Ceratodus* specimens, and they have been so often disappointed when exchanging them with other museums, that they have now determined to sell their duplicates in London to the highest bidders. Five of these fishes, in spirits (males and females) will be despatched to Messrs. P. W. Flower and Sons, and I hope that a good price will be obtained for them. Up to the present time all efforts to obtain more of the *Ceratodus* have been in vain, and I believe that they are not so common as some people think. Mr. George Masters has too much to do here; and besides, we have no funds, travelling being very expensive in the Wide Bay district, otherwise another Expedition would be sent by the Board. Mr. Masters knows *how* to catch them, and I hope that when the *Challenger* arrives he will be able to accompany a party from that ship to Gayndah.

Rainbow and its Reflexion

A FEW weeks ago I had the pleasure of seeing a rainbow and its reflexion, or at least a reflexion of one from the same shower at the same time, in smooth water.

The base of the bow in the cloud seemed but a few hundred yards from me, and the reflexion evidently did not belong to it, as the two bases did not correspond, the reflected bow lying inside the other, the red of the one commencing where the violet rays of the other disappeared.

Balbriggan, Ireland, Feb. 2 GEORGE DAWSON

Remarkable Fossils

THE letter by Mr. T. W. Cowan in NATURE, vol. ix. p. 241, confirms the truth of the statements contained in my "Appeal to our Provincial Scientific Societies" which appeared in NATURE, vol. ix. p. 162. Collections of the kind described by Mr. Cowan are "kicking" about the country in all directions, valued merely as temporary possessions by the owners, few of whom, as far as my experience goes, appear to possess sufficient public spirit or intelligence to realise their public and scientific importance; otherwise these collections would be more frequently localised and preserved for the district museum.

Jan. 31 S. G. P.

Volcanoes and the Earth's Crust

MR. HOWORTH, in NATURE, vol. ix. p. 201, advances the following opinions:—That volcanoes are found neither in regions of elevation nor of subsidence, but on the boundaries between them; that the great continents are on the whole rising, and the beds of the great oceans on the whole sinking; and that the centres of elevation are in the circumpolar regions.

It seems to me that the last two statements cannot be reconciled with each other. The southern hemisphere is for by far the greater part oceanic. According to Mr. Howorth, the ocean-beds are subsiding, and yet the southern circumpolar region contains a focus of elevation. Further: if volcanoes are not found in areas of elevation, and if the circumpolar regions are regions of elevation, what does he make of the volcanoes of Jan Mayen (between Norway and Spitzbergen), and of the Antarctic continent?

Were there any such laws of elevation and subsidence as Mr. Howorth maintains, the respective regions of elevation and of subsidence would have continued the same since the consolidation of the earth: but this is contradicted by the commonest facts of stratification, which show that elevation and subsidence have everywhere alternated with each other.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim

The Use of Terms in Cryptogamic Botany

As no specialist in Algology has replied to the inquiry of your correspondent "D. R.," in NATURE for January 15, I may be permitted to quote for his information the following from the article "Nucleus," in the "Treasury of Botany" from the pen of the author of the "Introduction to Cryptogamic Botany":—"In Algæ the term is applied to the fructifying mass of the Rhodosperms, whether contained in a single cell or in a compound cyst or conceptacle, the word *nucleoli* being used when there is a group of nuclei." The instance alluded to by your correspondent is, unfortunately, not the only one in which the terminology of cryptogams is in a state of most perplexing confusion.

ALFRED W. BENNETT

A Lecture Experiment

THE condensation of liquid in the form of vapour into minute globules, and the production of a shower of rain, may be very well illustrated for class purposes in the following manner:—

Place about an ounce of Canada balsam in a Florence flask, and let it boil. At the top of the flask clouds of globules of turpentine will be seen hovering about, altering in shape very much like sky-clouds, and the globules are large enough to be visible by the naked eye. If a cold glass rod be gradually introduced into the flask, these clouds may be made to descend in showers. By the adaptation of a lime-light the whole process could be shown on a screen.

LAWSON TAIT

TODHUNTER ON EXPERIMENTAL ILLUSTRATIONS

> Segnius irritant animos demissa per aurem,
> quam quæ sunt oculis subjecta fidelibus, et quæ
> ipse sibi tradit spectator.

THE following is, as nearly as I can recollect, the substance of a few remarks which I felt myself compelled to make to my class in a recent lecture. I had exhibited and described Hope's apparatus for showing the maximum density point of water, and proceeded to say:—

Now that the freezing mixture has been applied, my assistant will from time to time record on the black-board the simultaneous indications of the two thermometers, and will recall our attention to the experiment as the critical period approaches. You must, however, in this form of experiment take for granted his fidelity and accuracy in reading and recording. By means of a somewhat cumbrous application of optical processes, it would be easy to project upon a screen images of the thermometers, in such a way that each of you might see for himself the course of the phenomenon. But the thermo-electric method, whose principle I have already explained to you, is at once far easier of application, and in its indications more directly expressive. This I will show on another occasion. For the present you must rely on the observations to be made for you by my assistant. Yet I have no doubt that all of you will allow that the exhibition of the experiment, even in this imperfect manner, wonderfully assists you in understanding its nature.

This leads me to mention that a very decided opinion against the use of experimental illustration has been recently pronounced by one of the most erudite and voluminous of British mathematicians; my own former tutor, Mr. Todhunter, whose name and many of whose works must be familiar to most of you. Such a man speaks, deservedly with authority, on many points; and therefore his dicta upon a point with which he shows himself to be totally unacquainted are especially dangerous. And I feel that it is my duty to point out to you, and warn you against, errors or absurdities connected with physics, whenever they come from one whose statements are, on other grounds, worthy of attention. I shall not trouble you with the whole passage I refer to in Mr. Todhunter's "Conflict of Studies," but merely read to you a sentence or two of the most astounding part of it. I premise that though he is speaking of the teaching of physical science in schools, his observations apply (if they have any basis whatever) to science-teaching in general.

"It may be said that the fact makes a stronger impression on the boy through the medium of his sight, that he believes it the more confidently. I say that this ought not to be the case. If he does not believe the statements of his tutor—probably a clergyman of mature knowledge, recognised ability, and blameless character—his suspicion is irrational, and manifests a want of the power of appreciating evidence, a want fatal to his success in that branch of science which he is supposed to be cultivating."

Verbal comment on this would be altogether superfluous, and the only practical comment I am disposed now to make is to proceed at once to farther *experimental* illustrations of the subject before us.

P. G. TAIT

POLARISATION OF LIGHT *

V.

THE conversion of plane into circularly polarised light may also be effected by total reflexion. If plane-polarised light traversing glass be incident upon the inner side of the limiting surface at any angle at which total reflexion takes place, it may be considered as resolved into two plane-polarised rays, the vibrations of one being parallel and those of the other perpendicular to the plane of reflexion; and there is reason to believe that in every such case a difference of phase is brought about which for a particular angle in each substance (in St. Gobain glass it is 54° 30′) it has a maximum value of one-eighth of a wave-length. And if the original plane of vibration be inclined at an angle of 45° to that of reflexion the amplitudes of the two vibrations, into which the reflected vibrations are supposed to be resolved, will be equal. A full discussion of the mechanical causes which may be considered to effect this difference of phase would carry us deeper into the more difficult parts of the Wave Theory than would be suitable in this place. But if we accept the fact that the above-mentioned effects result, when polarised light (whose plane of vibration is inclined at 45° to that of reflexion) is reflected at a proper angle; then the following construction will be readily understood. Take a rhomb of glass, *a*, *b*, *c*, *d*, Fig. 14, whose acute angles are 54° 30′; a ray incident perpendicularly to either end will undergo two total internal reflexions at the sides, say at *p* and *s*, and will emerge perpendicularly to the other end. These two reflexions will together produce a retardation, as described above, of one-fourth of a wave-length. And if the ray be originally polarised and its plane of vibration be inclined

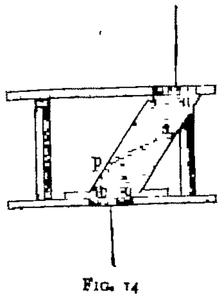

FIG. 14

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* Continued from p. 285.

at an angle of 45° to that of reflexion (that of the paper in the figure) the amplitudes of the two vibrations will be equal; and all the conditions will be fulfilled for the production of circular polarisation. Such an instrument was invented by Fresnel, and is called in consequence Fresnel's rhomb. On account of its length and its displacement of the ray, it is not so convenient as a quarter-undulation plate; but on the other hand it affects rays of all wave-lengths equally, while the quarter-undulation plate can strictly be adapted to rays of only one wave-length.

If either of these instruments be introduced and suitably placed between a selenite plate and the analyser, the chromatic effects will be similar to those due to a plate of quartz cut perpendicularly to the axis.

Another important property of these instruments consists in their effect upon circularly polarised light. Such light may be considered to arise from two plane-polarised rays whose vibrations are perpendicular to one another, and which present a difference of phase equal to a quarter of a wave-length. If, therefore, either a quarter-undulation plate or a Fresnel's rhomb be suitably placed, it will either increase or diminish the difference of phase by a quarter of a wave-length. In the one case the difference of phase will amount to a half wave-length, in the other it will vanish. And in either case the vibration will be converted into a rectilinear one; but the directions of vibration in the two cases will be perpendicular to one another.

Reflexion from a metallic surface may also be employed for converting plane into circular polarisation. If a ray of plane-polarised light fall upon a metallic reflector it is divided into two, whose vibrations are respectively parallel and perpendicular to the reflector; and the latter is retarded behind the former by a difference of phase depending upon the angle of incidence. If the plane of vibration of the incident ray be inclined to the plane of incidence at an angle (nearly 45°) which varies with the metal employed, but which is perfectly definite, the intensities become equal. And further, if the angle of incidence have a particular value dependent upon the nature of the metal (for silver 72°) the retardation will amount to a quarter of a wave-length. And the result will be a circularly polarised ray as in the case of total reflexion.

The apparatus (Fig. 15) best adapted for experiments based upon this principle is a modification of Norremberg's polariscope, suggested by Sir Charles Wheatsone, from whom the following description is quoted:—

"A plate of black glass, G, is fixed at an angle of 3° to the horizon. The film to be examined is to be placed on a diaphram, D, so that the light reflected at the polarising-angle from the glass plate shall pass through it at right angles, and, after reflexion at an angle of 18° from the surface of a polished silver plate S, shall proceed vertically upwards. N is a Nicol's prism, or any other analyser, placed in the path of the second reflexion. The diaphragm is furnished with a ring, moveable in its own plane, by which the crystallised plate to be examined may be placed in any azimuth. C is a small moveable stand, by means of which the film to be examined may be placed in any azimuth and at any inclination; for the usual experiments this is removed.

"If a lamina of quartz cut parallel to the axis, and sufficiently thin to show the colours of polarised light, be placed upon the diaphragm so that its principal section (*i.e.* the section containing the axis) shall be 45° to the *left* of the plane of reflexion, on turning the analyser from left to right, instead of the alternation of two complementary colours at each quadrant, which appear in the ordinary polarising apparatus, the phenomena of successive polarisation, exactly similar to those exhibited in the ordinary apparatus by a plate of quartz cut perpendicularly to the axis, will be exhibited; the colours follow in the order R, O, Y, G, B, P, V, or, in other words, ascend as in the case of a right-handed plate of quartz cut perpendicularly to the axis. If the lamina be now either inverted, or turned in its own plane 90°, so that the principal section shall be 45° to the right of the plane of reflexion, the succession of the colours will be reversed, while the analyser moves in the same direction as before, presenting the same phenomena as a left-handed plate of quartz cut perpendicularly to the axis. Quartz is a positive doubly refracting crystal; and in it consequently the ordinary index of refraction is smaller than the extraordinary index. But if we take lamina of a negative crystal, in which the extraordinary index is the least, as a film of Iceland spar split parallel to one of its natural cleavages, the phenomena are the reverse of those exhibited by quartz. when the principal section is on the *left* of the plane of reflexion the colours descend, and when it is on the *right* of the same plane the colours ascend, the analyser being turned from left to right.

"It has been determined that the ordinary ray, both in positive and negative crystals, is polarised in the principal section,* while the extraordinary ray is polarised in the section perpendicular thereto. It is also established that the index of refraction is inversely as the velocity of transmission. It follows from the above experimental results, therefore, that when the resolved ray whose plane of polarisation is to the left of the plane of reflexion is the quickest, the successive polarisation is right-handed, and when it is the slowest, the successive polarisation is left-handed—in the order R, O, Y, G, B, P, V and in the second case in the reverse order.

"The rule thus determined is equally applicable to laminæ of bi-axal crystals.

"As selenite (sulphate of lime) is an easily procurable crystal and readily cleavable into thin laminæ capable of showing the colours of polarised light, it is most frequently employed in experiments on chromatic polarisation. The laminæ into which this substance most readily splits contain in their planes the two optic axes; polarised light transmitted through such laminæ is resolved in two rectangular directions, which respectively bisect the angles formed by the two optic axes; the line which bisects the smallest angle is called the intermediate section; and the line perpendicular thereto which bisects the supplementary angle is called the supplementary section. These definitions being premised, if a film of selenite is placed on the diaphragm with its intermediate section to the left of the plane of reflexion, the successive polarisation is direct or right-handed; if, on the contrary, it is placed to the right of that plane, the successive polarisation is left-handed. The ray polarised in the intermediate section is therefore the most retarded; and as that section is considered to be equivalent to a single optic axis, the crystal is positive.

"In one kind of mica the optic axes are in a plane perpendicular to the laminæ. They are inclined 22½° on each side the perpendicular within the crystal, but, owing to the refraction, are seen respectively at an angle of 35°·3 therefrom. The principal section is that which contains the two optic axes. If the film is placed on the diaphragm with its principal section inclined 45° to the left of the plane of reflexion, the successive polarisation is right-handed. The ray, therefore, polarised in the section which contains the optic axes is the one transmitted with the greatest velocity.

"Films of uni-axal crystals, whether positive or negative, and of bi-axal crystals, all agree therefore in this respect:—that if the plane of polarisation of the quickest ray is to the left of the plane of reflexion, the successive polarisation is right-handed when the analyser moves from left to right; and if it is to the right of the plane of reflexion, other circumstances remaining the same, the successive polarisation is left-handed.

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* The plane of polarisation is, throughout these pages, taken to be perpendicular to that of vibration.

"It must be taken into consideration that the principal section of the film is inverted in the reflected image; so that if the plane of polarisation of the quickest ray in the film is to the left of the plane of reflexion, it is to the right of that plane in the reflected image.

"It may not be uninteresting to state a few obvious consequences of this successive polarisation in doubly refracting laminæ, right-handed and left-handed according to the position of the plane of polarisation of the quickest ray. They are very striking as experimental results, and will serve to impress the facts more vividly on the memory.

"1. A film of uniform thickness being placed on the diaphragm with its principal section 45° on either side the plane of reflexion, when the analyser is at 0° or 90° the colour of the film remains unchanged, whether the film be turned in its own plane 90°, or be turned over so that the back shall become the front surface; but if the analyser be fixed at 45°, 135°, 225°, or 315°, complementary colours will appear when the film is inverted from back to front, or rotated in its own plane either way 90°.

"2. If a uniform film be cut across and the divided portions be again placed together, after inverting one of them, a compound film is formed, which, when placed on the diaphragm, exhibits simultaneously both right-handed and left-handed successive polarisation. When the analyser is at 0° or 90° the colour of the entire film is uniform; as it is turned round the tints of one portion ascend, while those of the other descend; and when the analyser is at 45° or $n 90° + 45°$, they exhibit complementary colours.

"3. A film increasing in thickness from one edge to the other is well suited to exhibit at one glance the phenomena due to films of various thicknesses. It is well known that such a film placed between a polariser and an analyser will show, when the two planes are parallel or perpendicular to each other and the principal section of the film is intermediate to these two planes, a series of parallel coloured bands, the order of the colours in each band from the thick towards the thin edge being that of their refrangibilities, or R, O, Y, G, B, P, V. The bands seen when the planes are perpendicular are intermediate in position to those seen when the planes are parallel; on turning round the analyser these two systems of bands alternately appear at each quadrant, while in the intermediate positions they entirely disappear.

"Now let us attend to the appearances of these bands when the wedge-form film is placed on the diaphragm of the instrument, Fig. 15. As the analyser is moved round, the bands advance toward or recede from the thin edge of the wedge without any changes occurring in the colours or intensity of the light, the same tint occupying the same place at every half revolution of the analyser. If the bands advance toward the thin edge of the wedge, the successive polarisation of each point is left-handed; and if they recede from it the succession of colours is right-handed: every circumstance, therefore, that with respect to a uniform film changes right-handed into left-handed successive polarisation, in a wedge of the same substance transforms receding into advancing bands, and *vice versâ*. These phenomena are also beautifully shown by concave or convex films of selenite or rock-crystal, which exhibit concentric rings contracting or expanding in accordance with the conditions previously explained.

"4. Few experiments in physical optics are so beautiful and striking as the elegant pictures formed by cementing laminæ of selenite of different thicknesses (varying from $\frac{1}{2000}$ to $\frac{1}{50}$ of an inch) between two plates of glass. Invisible under ordinary circumstances, they exhibit, when examined in the usual polarising-apparatus, the most brilliant colours, which are complementary to each other in the two rectangular positions of the analyser. Regarded in the instrument, Fig. 13, the appearances are still more beautiful; for, instead of a single transition, each colour in the picture is successively replaced by every other colour. In preparing such pictures it is necessary to pay attention to the direction of the principal section of each lamina when different pieces of the same thickness are to be combined together to form a surface having the same uniform tint; otherwise in the intermediate transitions the colours will be irregularly disposed.

"5. A plate of rock-crystal cut perpendicular to the axis loses its successive polarisation, and behaves exactly as an ordinary crystallised film through which rectilinear polarised light is transmitted.

"By means of the phenomena of successive polarisation it is easy to determine which is the thicker of two films of the same crystalline substance. Place one of the films on the diaphragm E of the instrument (Fig. 15) in the position to show, say, right-handed polarisation, then cross it with the other film; if the former be the thicker, the successive polarisation will be still right-handed; if both be equal, there will be no polarisation; and if the cross film be the thicker, the successive polarisation will be left-handed. In this manner a series of films may be readily arranged in their proper order in the scale of tints.

"In the experiments I have previously described the planes of reflexion of the polarising mirror and of the silver plate were coincident; some of the results obtained when the azimuth of the plane of reflexion of the silver plate is changed are interesting.

"I will confine my attention here to what takes place when the plane of reflexion of the silver plate is 45° from that of the polarising reflector.

"When the principal sections of the film are parallel and perpendicular to the plane of reflexion of the polarising mirror, as the whole of the polarised light passes through one of the sections, no interference can take place, and no colour will be seen, whatever be the position of the analyser.

"When the principal sections of the film are parallel and perpendicular to the plane of reflexion on the silver plate, they are 45° from the plane of reflexion of the polarising mirror.

"The polarised ray is then resolved into two components polarised at right angles to each other; one component is polarised in the plane of reflexion of the silver plate, the other perpendicular thereto; and one is retarded upon the other by a quarter of an undulation.

"When the analyser is at 0° or 90° no colours are seen because there is no interference; but when it is placed at 45° or 135°, interference takes place, and the same colour is seen as if light circularly polarised had been passed through the film. The bisected and inverted film shows simultaneously the two complementary colours.

"But when the film is placed with one of its principal sections 22½° from the plane of reflexion of the polarising mirror, on turning round the analyser the appearances of successive polarisation are reproduced exactly as when the planes of reflexion of the silver plate and of the polarising mirror coincide. In this case the components of the light oppositely polarised in the two sections are unequal, being as cos 22½° to sin 22½°; these components respectively fall 22½° from the plane of reflexion of the silver plate and from the perpendicular plane, and are each resolved in the same proportion in these two planes. The weak component of the first, and the strong component of the second, are resolved into the normal plane, while the strong component of the first and the weak component of the second are resolved into the perpendicular plane.

"The apparatus (Fig. 15) affords also the means of obtaining large surfaces of uncoloured or coloured light in every state of polarisation—rectilinear, elliptical, or circular.

"It is for this purpose much more convenient than a Fresnel's rhomb, with which but a very small field of view can be obtained. It must, however, be borne in mind that the circular and elliptical undulations are inverted in the two methods: in the former case they undergo only a single, in the latter case a double reflexion.

"For the experiments which follow, the crystallised plate must be placed on the diaphragm E, between the silver plate and the analyser, instead of, as in the preceding experiments, between the polariser and the silver plate.

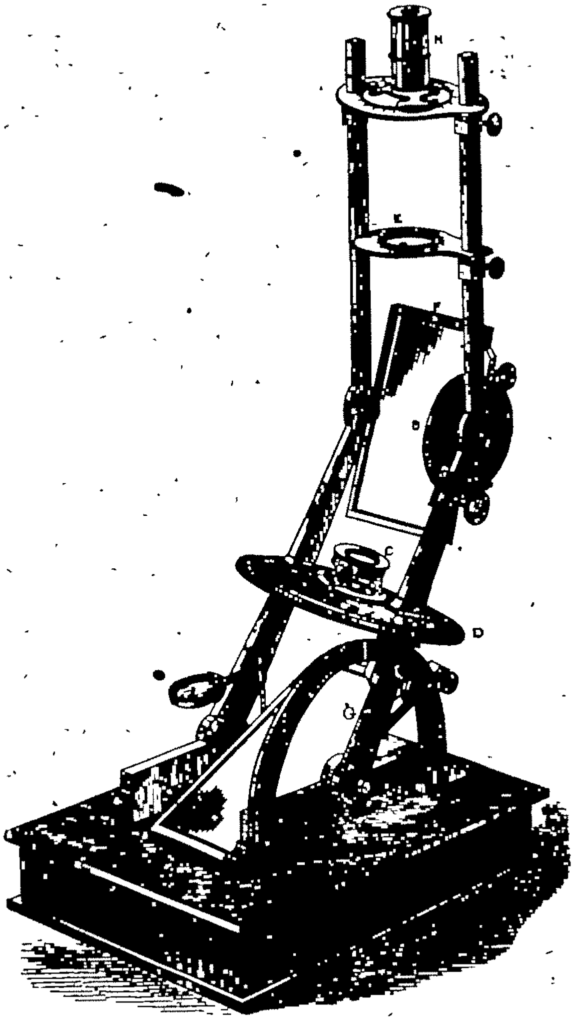


FIG. 15.—Wheatstone's modification of Norremberg's Polariscope.

"By means of a moving ring within the graduated circle D, the silver plate is caused to turn round the reflected ray, so that, while the plane of polarisation of the ray remains always in the plane of reflexion of the glass plate, it may assume every azimuthal position with respect to the plane of reflexion of the silver plate. The film to be examined and the analyser move consentaneously with the silver plate, while the polarising mirror remains fixed.

"In the normal position of the instrument the ray polarised by the mirror is reflected unaltered by the silver plate; but when the ring is turned to 45°, 135°, 225°, or 315°, the plane of polarisation of the ray falls 45° on one side of the plane of reflexion of the silver plate, and the ray is resolved into two others, polarised respectively in the plane of reflexion and the perpendicular plane, one of which is retarded on the other by a quarter of an undulation, and consequently gives rise to a circular ray, which is right-handed or left-handed according to whether the ring is turned 45° and 225°, or 135° and 315°. When the ring is turned so as to place the plane of polarisation in any intermediate position between those producing rectilinear and circular light, elliptical light is obtained, on account of the unequal resolution of the ray into its two rectangular components.

"Turning the ring of the graduated diaphragm from left to right, when the crystallised film is between the silver plate and the analyser, occasions the same succession of colours for the same angular rotation as rotating the analyser from right to left when the instrument is in its normal position and the film is between the polariser and the silver plate."

The same principles apply to the case of bi-axal crystals cut parallel to a plane containing the two optic axes. A ray of plane-polarised light transmitted through such a plate is divided into two, whose vibrations respectively bisect the angles formed by the two axes. As mentioned above, the line which bisects the smallest angle is called the intermediate section, and the line perpendicular to it the supplementary section; and the order of the colours depends upon the relative velocity of the two rays. In selenite, the ray whose vibrations lie in the supplementary section is the slowest; in mica it is the swiftest. Hence these two crystals, all other circumstances being alike, give the colours in opposite orders, and may be regarded as positive and negative, like quartz and Iceland spar. And a test similar to that indicated for uni-axal may be applied to bi-axal crystals.

Some interesting and varied experiments may be made by using two circularly polarising instruments, *e.g.*, two quarter undulation plates (say the plates A and B), one between the polariser proper and the crystal (C) under examination, the other between the crystal and the analyser. The light then undergoes the following processes. If the plate A be placed so that its axis is at 45° on one side or other of the original plane of vibration, and the plate B with its axis parallel or perpendicular to that of A, then on turning the analyser we shall have the phenomena of circular polarisation described above. Again, if, the plates A and B retaining the positions before indicated, the crystal C be turned round in its own plane; then, since the light emerging from A and B is circularly polarised, it has lost all trace of direction with reference to the positions of polariser and analyser, and consequently no change will be observed.

Again, if the plates A and B have their axes directed at 45° on either side of the axis of C, and the three plates be turned round as one piece, the colour will remain unchanged, while if the analyser be turned, the colours will follow in the regular order. If the plates A and B have their axes directed at 45° on the same side of the axis of C, and the pieces be turned round bodily as before, the colours change in the same order as above, and go through their cycle once in every right angle of rotation; and if the analyser be turned in the same direction, the colours change, but in the reverse order. The explanation of this is to be found in the fact that when the plates A and B are crossed, the retardation due to A is compensated by that due to B; so that the only effective retardation is that due to the crystal C. But upon the latter depends the rotation of the plane of vibration; if, therefore, the polariser and analyser remain fixed, the colour will remain unaltered. When the plates A and B have their axes parallel, there is no compensation, and the colour will consequently change. It should be added that the rotation of the plane of vibration, and consequently the sequence of colours, does not follow exactly the same law in these cases as in quartz.

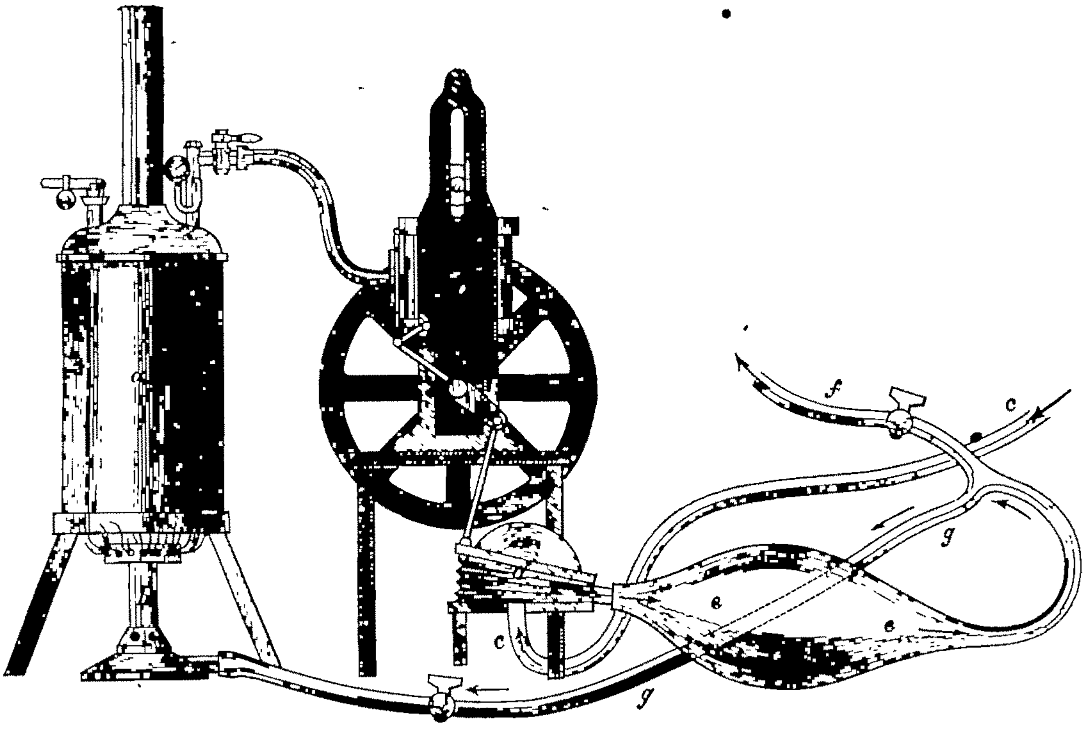
W. SPOTTISWOODE

(*To be continued.*)

THE HEART AND THE SPHYGMOGRAPH *

IN the same way that by the spectroscope much can be learned as to the chemical constitution and the physical changes going on in the sun, so by the sphygmograph applied to the artery at the wrist many of the most important phenomena occurring in the heart can be studied with a facility that cannot be otherwise attained. Till the introduction of the sphygmograph of Marey the pulse was considered to be little more than a simple up and down movement, because the instruments employed to register it, such as those of Hérisson, Ludwig, and Vierordt, developed so much momentum that the details of the true trace were disguised. In the instrument as at present employed, the substitution of counterbalancing springs instead of weights has so far improved its efficiency, that the pulse is now known to form a decidedly complicated curve if its movements are allowed to record themselves on a moving paper. The sphygmograph trace, as thus produced, gives indications in two directions; first, as to the action of the valves of the heart; and secondly, as to the manner in which the muscular walls of the ventricles perform their work. It is to the former of these subjects that most physiologists have directed their observations in employing the instrument; but it is to the latter, the more important of the two, that it is my intention to direct attention on the present occasion.

The heart being nothing more than a pump of a peculiar construction, much may be learned by comparing it with other artificially constructed machines for the same purpose. In most such machines the force which keeps the pump at work is constant in power, in other words it does not vary automatically in efficiency with the amount of work that is expected of it. In the locomotive engine, however, there is an arrangement by which the furnace becomes hotter as the speed at which it moves is increased, the waste steam pipe opening into the funnel and so varying the amount of the draught through the boiler tubes. With this arrangement it is nevertheless evident that there is a great waste of fuel in the construction of the furnace.



Automatically Working-engine, when supplied with coal-gas.

It is quite possible to construct a steam-engine on much more economical principles, and the accompanying figure illustrates the manner in which the small engine on the table is at present working (see Figure). The boiler (*a*) being sufficiently heated, drives the engine (*b*), which performs work by pumping *coal-gas* from the tube *c* through the pump *d*, into the elastic reservoir *e*. From this elastic bag most of the coal-gas escapes, through the tube *f*, into an ordinary gas bag, but a tube (*g*) carries some of it to supply the Bunsen's burner (*h*) which heats the boiler. It is evident that with this arrangement the size of the flame of the Bunsen's burner (*h*), and therefore the pressure of steam in the boiler, which is the same as saying the efficiency of the engine, varies with the amount of work required of that engine; for the greater the pressure in the elastic bag, the harder is it for the engine to perform the work required of it, and the greater is the burner-flame. With a certain proportion between the sizes of the orifices of the taps and the extensibility of the elastic bag and tubes, it would be possible to arrange this engine in such a manner that, within certain limits, the velocity of the fly-wheel would not vary with the pressure in the elastic bag; in other words, with the work to be done. That the heart is a pump constructed on the same principle as this engine is the teaching of the sphygmograph, as far as it is in my power to interpret its curves, the proof resting on the following considerations.

First, the analogy between the anatomical distribution of the arteries and the different parts in connection with the engine is not difficult to trace. The coal-gas corresponds to the blood, the boiler (*a*) together with the engine (*b*) to the muscular tissue of the heart, whose left ventricular cavity has its analogue in that of the bellows (*d*). The elastic reservoir, together with the tubes, corresponds to the systemic arteries, the gas-bag connected with the tube *f* (the capillaries) to the systemic veins; and the tube *g* to the coronary arteries, which supply the muscular tissue of the heart with nutrient blood, just as it does the boiler by means of the burner *k*. This, however, does not show that the pumping power of the heart varies directly as the blood-pressure; that such is the case depends on the opportunity offered by the sphygmograph-trace for the estimation of the length of the ventricular systole under different circumstances. Each beat or revotion of the heart is divided into two main parts—(1) the period of contraction or systole, and (2) the period of repose or diastole. The former of these occupies the interval between the commencement of the primary rise and that of the dicrotic rise in the sphygmograph trace; the latter from the dicrotic rise to the commencement of the succeeding primary rise. In all good tracings from healthy pulses these two points, the primary and dicrotic rises, are readily found; and their relative lengths can be estimated with great accuracy. A large number of measurements have enabled me to prove that *the relative lengths of the systolic and diastolic portions of the pulse-trace do not vary for any given pulse-rate.* But, as will be granted by most physiologists who have worked at the subject, *the blood-pressure in the arteries is quite independent of the pulse-rate.* Consequently the heart may be doing very different amounts of work without any variation in the pulse-rate, which is the same thing as saying, with the same length of systole; which makes it evident that, as in the above-described engine, *the force of the cardiac muscular contraction varies directly as the blood-pressure,* knowing what we do about the flow of fluids through capillary tubes, and the capacity of the arterial system under different degrees of blood-pressure.

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* Abstract of a lecture delivered by Mr. A. H. Garrod at the Royal Institution on the evening of Friday, Feb. 6.

The sphygmograph trace tells us more than this. Though the length of the systolic portion of the beat does not change with any given pulse-rate, nevertheless it does so greatly with different rapidities of pulse; my observations showing that the length of the systole varies as the cube root of the whole beat, being found from the equation $xy' = 47\sqrt[3]{x}$ when $x =$ the pulse-rate and $y' =$ the ratio borne by the systole to the whole beat. From this no other inference can be drawn than that the length of the diastole, or period of cardiac rest, during which fresh blood is circulating through the ventricular walls, must modify the contractile force of its muscular substance. The exact extent of this influence can be more readily estimated by a study of the cardiograph trace, which is obtained by applying the sphygmograph to the side of the chest at any spot where the pulsations of the heart are to be felt. It may, from the thus obtained curves, be demonstrated that if not exactly, approximately, at least, *the nutrition of the heart's walls must vary as the square-root of the length of the diastolic period.*[*]

There is much, therefore, as I hope I have been able to show, to be learned respecting the action of the heart from measurement of sphygmograph tracings, and it is scarcely too bold to extend the generalisation to the properties of muscular tissue generally; for the fact that each beat depends entirely for its efficiency on the peculiarities in the blood-pressure and the duration of the previous diastole, removes all complications as to incompleteness of exhaustion, and all doubts as to the exact amount of work done by the muscular fibres themselves of that most perfect of engines, whose extreme perfection enables it to complete in most of us something like 750,000 beats in a week, and nearly thirty thousand million revolutions in a person by the time he is seventy years of age.

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[*] *Journal of Anatomy and Physiology*, May and November, 1873.

DR. VON MIKLUCHO MACLAY'S RESEARCHES AMONG THE PAPUANS[*]

WHEN lately at Buitenzorg—the scarcely euphonious equivalent of the "Sans Souci" of a former English Governor of Java—we had the good fortune to make the acquaintance of the owner of a name, whose peculiarity, no less than fame, has rendered it familiar to every biologist.

The friends—and we are sure they must be numerous—of Dr. Miklucho Maclay will regret to hear that he is determined, in spite of an aguish fever which still clings to him, and of, it is feared, some serious implication of the liver, to start again in a few days for the scene of his recent labours—the east coast of New Guinea, where he had previously spent fifteen months in close intercourse with the natives.

On September 19, 1871, the Russian corvette *Vitias* cast anchor in Astrolabe Gulf, and Dr. Maclay landed with two servants, one a Polynesian, the other a Swede. After a hut of very modest dimensions[†] had been built for him by the ship's carpenter, the *Vitias* weighed anchor on the 26th, and departed.

Dr. Maclay was soon left practically alone, and dependent entirely upon himself, for the Polynesian servant died early in December of a chronic fever which he had when he started, and the Swede soon followed suit, with the exception, that he did not die, but by constant ailing was a source of much encumbrance to his master.

As the natives were very distrustful, scarcely answering any questions, Dr. Maclay did not make much progress with the essential task of learning the language. Not only, however, were they suspicious, but determined to discourage the presence of the stranger by shooting arrows close to his head and neck, and pressing their spears so hard against his teeth, that he was constrained to open his mouth. Finding that he did not only not take the least notice of these annoyances, but that all his actions toward them were good (for, he being a doctor, his utility in the economy of the community was soon discovered), they concluded that he was no ordinary mortal, but that he was the veritable man-in-the-moon ("Kâram-tâmo"), and paid him due respect accordingly.

As there were footpaths only in the neighbourhood of the villages, and as these latter were never found at a greater elevation than 1,500 feet, Dr. Maclay had some difficulty in making expeditions without guides, which at first were difficult to procure.

During his stay in New Guinea Dr. Maclay studied the inhabitants of the whole coast of Astrolabe Gulf; the people of the mountains round the gulf, and the dwellers on the islands near Cape Duperré, one of the boundaries of the gulf, who lived a life of such perfect peace that he called the islands "the Archipelago of Contentment."[‡] The inhabitants, too, of Dampier Island (Kar-kar) paid him visits. The inhabitants of "Maclay Coast," by which name Dr. Maclay proposes to call the coast skirting the edge of the Astrolabe Gulf, were of especial interest, as it seems that they have never been in intercourse with any civilised people, for not only were all their tools and weapons made out of stone, wood, or bone, but no trace of any European article could be found among them. These people treasured up, or exchanged as valuable, the smallest trifles which were given them, *e.g.* fragments of broken bottles, with which they shaved themselves, as a substitute for flint, or the sharp edges of grasses.

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[*] "Anthropologische Bemerkungen ueber die Papuas der Maclay Küste in Neu Guinea." Reprint from the *Natuurkundig Tydschrift voor Nederlandsch Indie Deel* xxxiii. *Mijn Verblijf aan de Oostkust van Nieuw Guinea. Ibid.* (Batavia, 1873).

[†] Only 7 feet broad, and 14 feet long, and divided by a screen of sailcloth into two rooms, one for his servants, the other for himself. The hut was situated on the south coast of Astrolabe Gulf, midway between the two capes, its boundaries.

[‡] "Archipel der zufriedenen Menschen."

Dr. Maclay told us of one curious custom which he does not mention in either of the two papers referred to in the note. The Papuans, though they know how to produce fire by rubbing together two pieces of wood, do not do this when they require this agent, but always carry their fire literally about with them, either trailing a lighted stick after them as they walk, or placing the same under their beds when they sleep.

Dr. Maclay, despite much pains, was only able to collect ten skulls, and only two out of these had the lower jaw, for the natives preserve this with great veneration, while the skull itself is thrown into the neighbouring jungle as a thing of no worth. The skull of the Papuans of Maclay coast is "dolicho-cephalic." The superciliary eminences are frequently very strongly developed The maxillary region is prognathous, so that the upper teeth project considerably beyond those of the mandible. The Papuans are of middle stature, the females being considerably smaller than the males, but are strong and well built.

Contrary to what has been written, there is no roughness of skin considerable enough to constitute a race characteristic; which may be largely accounted for by the custom of smearing the bodies with a kind of earth, and to the frequency of psoriasis ("masso"). The colour of the skin too is in general of a light chocolate brown, and not of a bluish-black colour as has been previously asserted. The inhabitants of New Ireland, an island not far distant, have, on the other hand, a comparatively dark skin. The scars of slight wounds, *e.g.* such as are made with a red-hot coal, are somewhat darker than the surrounding skin, while deep wounds, which are of not infrequent occurrence, leave behind them scars almost white in colour.

After a series of very careful observations, made as well upon shaven as upon well-covered scalps, Dr. Maclay concludes that the hair is not naturally disposed, as has been represented, in tufts or clumps, but grows just as it would upon the head of a European. The length of the hair, too, varies in different individuals, for while one man is fain to cover his bald pate with a cuscus,* another is proud to display a "gatessi," which luxuriantly covers his shoulders.†

The natural colour of the hair is dull black, but this is marked, after the period of childhood, by a black ("kuma") or red ("surru") dye. The hair of children is covered with a wash of ashes and water for protection against lice; this hardens into a thick crust. In the case of males this is continued till the time of circumcision, after which period much care is bestowed upon the coiffure. The women, oddly enough, expend no pains upon the arrangement of their hair. The eye-brows are generally shaven, and the hairs of the beard are either shaven or plucked out in the young men, but are permitted to grow among adults.

The general hair-growth upon the body seems to be more scanty than it is among the Caucasian races. Though hair is never seen on the back of the hands, it sometimes grows pretty thickly along the line of the vertebral column, and is sometimes so far extended as to cover the whole of the buttocks.

With regard to the physiognomy, the forehead is not high but small, and sometimes retreating; the nose is broadly flattened out, frequently with dilated nostrils; the mouth is broad, and has a projecting upper lip; the chin is retreating, while strongly projecting cheek-bones strikingly contrast with the smallness of the forehead in the temporal region.

The Papuans of Maclay coast bore a hole through the septum of the nostrils, in which a long fragment of stone or piece of shell is worn. The teeth are much worn through the almost exclusive use of a vegetable diet: Dr. Maclay noticed this in his own teeth after a stay of eight months in Papua. The lobules of the ears are pierced at an early age by means of the thorn of a *Dioscorea* and become much elongated by having to support heavy ear-rings.

If the back of a Papuan is seen in profile, there will be noticed a considerable concavity of curve in the lumbar region. This would seem to be a characteristic in which the Papuan differs from the Caucasian race. The Papuans make a greater use of the left hand and arm than of the right, and use the feet to pick up various objects—sometimes very small ones—from the earth. This is done, not by flexion of the toes, but by anadduction of the great toe, which is considerably separated from the rest of the toes. From this use of the toes, it frequently happens that the two feet are dissimilar in size.

Circumcision is performed at from the ages of 13 to 15 years, and, as Zipporah performed it, with a sharp flint. This custom is general among the Papuans of Maclay coast, and among most of the coast and some of the mountain inhabitants. Those—and among them are the New Irelanders and the inhabitants of one of the Islands of the Archipelago of Contentment—who do not use this rite are looked down upon by their circumcised brethren. The suckling of infants is carried on for a long period, sometimes to the age of four years.

The Papuans are very strict in their sexual relations. The men marry early, soon after circumcision, and have only one wife; concubinage is almost unknown. The women, probably on account of the hard work in which they are engaged, seldom bear many children.

In spite of the dark colour of their skin, Dr. Maclay was able to recognise a change of colour in the face among the Papuans. He does not, however, state whether blushing follows upon a sense of shame, but only notices that the features are darker when they are overjoyed, or have been making great efforts, *e.g.* in the dance.

The Papuan women, like their European sisters, cultivate the art of which Mr. Turveydrop was the distinguished professor. Readers of the "Arabian Nights" may remember how that the seductive wriggling of the sides of one of the damsels "shaped like the letter alif," caused the "world to turn black" before the eye of a susceptible hero, and will therefore fully appreciate the subtle influence of a peculiar and "killing" wriggle which the Papuan maid begins to have at even the tender age of seven years. The half-caste women whom one sees at Batavia seem to have adopted a similar though modified habit.

The favourite position of the Papuan men—as it seems to be among the Malays also—is resting the buttock upon the heels (*das Hocken*), while the whole surface of the soles of the feet is applied to the ground. Dr. Maclay found that he could keep his balance only when the toes alone were in contact with the earth. This position of the Papuan must not only be acquired, but must depend also upon a peculiar relation of proportion in the limbs. Nothing can be said with certainty as to the duration of life among the Papuans. Dr. Maclay never saw an old individual among them.

Dr. Maclay, from the observations which he has at present made, concludes that *the Papuan stock falls into numerous varieties, distinct from one another, which, however, have no sharp lines of demarcation.*

On December 19, 1872, some natives came to Dr. Maclay to inquire the cause of some smoke which had been seen rising from the sea between Vitias and Dampier Islands. This turned out to be the clipper *Isoumroud*, which had been sent out to look for the traveller (whose death, it seems, had been announced in the English journals), at the instance of the Grand Duke Constantine.

Early on the morning of the 24th the *Isoumroud* weighed anchor, and as she steamed away there could

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[*] A small marsupial found in Papua. It is figured in Wallace's "Malay Archipelago."

[†] The long hair worn at the back of the head is termed "gatessi."

be heard all along the coast the sound of the Bâroem, a great wooden gong, announcing to the islanders the departure of the "man-in-the-moon," who had taken up his abode for more than a year amongst them.

JOHN C. GALTON

MICROSCOPIC EXAMINATIONS OF AIR

A WORK* of the greatest importance on the above subject has just been published in Calcutta by Mr. Douglas Cunningham. The conclusions which he has reached as the result of the experiment are so valuable that we deem it right to give them as great publicity as possible. The following is Mr. Cunningham's description of the aeroscope with which he made his experiments:—

The apparatus employed in obtaining specimens was a slightly modified form of that devised by Dr. Maddox. It consisted of three thin brass tubes, two of which slipped over the third central one and came into contact with the opposite side of a projecting rim on its circumference. This rim was formed by the margin of its diaphragm which divided the centre tube into two chambers. It was of sufficient thickness to allow of a spindle passing up through it. The latter terminated in a pointed extremity, which came in contact with the upper end of the bearing, and provided for the free rotation of the system of tubes. Round the margin of the diaphragm there was a set of perforations, to allow of the passage of air through it, and, on the centre of its anterior surface, there was a square plate of glass with a slightly projecting rim on its lower margin. The anterior of the two lateral tubes was provided with an expanded orifice, and contained a small, finely-pointed funnel in its interior; the pointed extremity opening immediately in front of the centre of the diaphragm-plate. The posterior tube was quite simple, and had a good-sized fish-tail vane fitted into a slit in its extremity.

The following are Mr. Cunningham's conclusions:—

The most important conclusions to be derived from all the preceding experiments regarding the dust contained in the atmosphere in the vicinity of Calcutta appear to be the following:—

1. The aeroscope affords a very convenient method for obtaining specimens really representing the nature of the true atmospheric dust.

2. Specimens of dust washed from exposed surfaces cannot be regarded as fair indices of the constituents of atmospheric dust, since they are liable to contain bodies which may have reached the surface otherwise than by means of the air, as well as others which are the result of local development.

3. Specimens collected by gravitation also fail to indicate the nature and amount of organic cells contained in the atmosphere, as the heavier amorphous and inorganic constituents of the dust are deposited in relative excess due to the method of collection.

4. Dew also fails to afford a good means of investigating the subject, as it is impossible to secure that all the bodies really present in a specimen of it should be collected into a sufficiently small space, and, moreover, because it is liable to accidental contaminations, and also affords a medium in which rapid growth and development are likely to take place.

5. Distinct infusorial animalcules, their germs or ova, are almost entirely absent from atmospheric dust and even from many specimens of dust collected from exposed surfaces.

6. The cercomonads and amœbæ appearing in certain specimens of pure rain-water appear to be zoospores developed from the mycelial filaments arising from common atmospheric spores.

7. Distinct bacteria can hardly ever be detected among the constituents of atmospheric dust, but fine molecules of uncertain nature are almost always present in abundance; they frequently appear in specimens of rain-water collected with all precautions to secure purity, and appear in many cases to arise from the mycelium developed from atmospheric spores.

8. Distinct bacteria are frequently to be found amongst the particles deposited from the moist air of sewers, though almost entirely absent as constituents of common atmospheric dust.

9. The addition of dry dust, which has been exposed to tropical heat, to putrescible fluids is followed by a rapid development of fungi and bacteria, although recognisable specimens of the latter are very rarely to be found in it while dry.

10. Spores and other vegetable cells are constantly present in atmospheric dust, and usually occur in considerable numbers: the majority of them are living and capable of growth and development; the amount of them present in the air appears to be independent of conditions of velocity and direction of wind; and their numbers are not diminished by moisture.

11. No connection can be traced between the numbers of bacteria, spores, &c., present in the air and the occurrence of diarrhœa, dysentery, cholera, ague, or dengue; nor between the presence or abundance of any special form or forms of cells, and the prevalence of any of these diseases.

12. The amount of inorganic and amorphous particles and other *débris* suspended in the atmosphere is directly dependent on conditions of moisture and of velocity of wind.

If these results be compared with those obtained by other observers, and detailed in the first section of this report, it will be seen that they agree very closely with those of M. Robin, only differing from them in indicating the presence of a somewhat larger number of spores than appeared in his observations. They differ almost equally from those arrived at by Pouchet and Ehrenberg. It is somewhat difficult to understand how the former observer so constantly failed to detect the presence of spores in his experiments, but there is an apparent reason for Ehrenberg's observation of the predominance of animal forms in the atmosphere. His conclusions appear to have been almost entirely founded on the results of the examination of specimens of dust not directly obtained from the air, but from surfaces on which it had been previously deposited from the air, such as leaves, tufts of moss, &c. Now, as has already been indicated, it is certainly quite unwarrantable to assume that all organisms found in such specimens existed as such in the air, or were even derived from the air in any way. All such surfaces are more or less liable to contact-inoculation; leaves and moss, for example, are liable to this through the agency of insects or birds. Moreover, with regard to many of the organisms detected in such situations, it must be recollected that there is no reason why they should not have arrived there by means of active progression over the surface. When surfaces are wet with rain, there is no reason why Tardigrades, Rotifers, Anguillulæ, and many infusoria should not travel over them from one point to another. The journey accomplished at any one time may be small, and its progress soon arrested by defective moisture; but, unless they are deprived of vitality in the interval by desiccation, they are ready for a fresh start when favourable conditions are again presented to them.

It is hardly safe to venture on the vexed questions regarding the origin of bacteria, but it may, at all events, be stated that the results of the present experiments are certainly not opposed to the belief in the transmission of these organisms in some way or other by means of the atmosphere; for they were actually observed among the particles in moist air, the addition of dry dust to putre-

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[*] "Microscopic Examinations of Air," by D. Douglas Cunningham, M.B. Surgeon H.M. Indian Medical Service (Calcutta).

scible fluids was followed by their rapid development, and they appeared in specimens of pure rain water.

Although these observations may not appear to encourage the hope of success in discovering the presence of atmospheric particles connected with the origin of disease, it must not be forgotten that they only refer to bodies distinguishable from one another *whilst in the air*, the possibility remaining that many of the finer molecules present in it are really of different natures, and may yet be distinguished from one another by means of their actions or developments. Many interesting questions are suggested in connection with the fact of the presence of such considerable numbers of living cells in the air. What becomes of them when drawn into the respiratory cavities of animals? Is their vitality destroyed, and, if so, how are they got rid of? Are they ever capable of undergoing any development within the organism, and do they then exert any prejudicial influence on the recipient? These and similar questions can only be answered by means of patient and extended experiment, but even such imperfect and superficial observations as the present will, I trust, serve a useful purpose in clearing away a few of the preliminary obstacles from the path of investigation.

NOTES

A SPECIAL General Meeting of the Linnean Society is to be held on Thursday, March 5, at 8 P.M., "to consider alterations in the Bye-laws of the Society;" when it is expected a full explanation will be given of the reasons which induced the Council to make the alterations recently adopted by the Society, which met with such violent opposition from a small section of the Fellows. It is understood that Mr. Bentham, who has occupied the chair of the Linnean Society for the past eleven years, will not offer himself for re-election at the ensuing anniversary. The custom of the Society requires that the next president shall be a Zoologist, but students of both branches of Biology will be glad to learn that Prof. Allman has allowed himself to be nominated. Few naturalists would bring to the office a wider, and none a more sympathetic knowledge.

MR. HIND writes to the *Times* that he has received from Prof. Winnecke, Director of the Observatory at Strasburg, the following position of a comet discovered by him in the Constellation Vulpecula on the morning of Saturday last:—February 20, at $17^{h}\ 16^{m}\ 40^{s}$ mean time—right ascension, $20^{h}\ 35^{m}\ 34{\cdot}2^{s}$; north declination, 26 deg. $0^{m}\ 46^{s}$. The diurnal motion in right ascension is 9^{m} increasing, and in declination 1 deg. 30^{m} towards the south.

PROF. ASA GRAY has been appointed to fill the Chair in the Board of Regents of the Smithsonian Institution, previously occupied by the late Prof. Agassiz.

THE Rev. Dr. Thomas William Jex-Blake, Principal of Cheltenham College, has been elected Head-Master of Rugby School, in succession to Dr. Hayman.

A BARONETCY has been conferred upon Dr. George Burrows, F.R.S., President of the Royal College of Physicians.

WE would direct the attention of Palæontologists and others who are specially interested in the Cephalopoda, to a paper by M. Munier-Chalmas, in the *Comptes Rendus* for Dec. 29, 1873, which is translated in the current number of the *Annals and Magazine of Natural History*, in which, from a study of their earliest stages, the generally accepted systematic position of the Ammonitites and Goniatites is stated to be inaccurate, they being shown to be dibranchiate decapoda allied to Spirula, and not tetrabranchiata at all.

THERE are two islands named St. Paul in the ocean: one close to the Equator was visited lately by the *Challenger*; the other, south of the Cape of Good Hope, is to be visited by a French expedition under Capt. Mouchez, for observing the forthcoming transit of Venus, as we stated in our last number. The identity of name has created a singular confusion. The French administration having decided that no naturalist was needed for St. Paul, the *Challenger* having explored the island a few months since, M. Mouchez had some trouble, it is said, to get the decision reversed by the authorities. Both islands, southern and northern, are almost of the same microscopical size and equally barren. They are of volcanic formation, with no trace of vegetable earth, and consequently of vegetation.

A TELEGRAM from Melbourne, dated February 17, states that Colonel Egerton Warburton has reached Perth, in Western Australia, overland from Adelaide, having thus accomplished the object of the exploring expedition on which he left Tennant's Creek, north of Adelaide, in the centre of Australia, about twelve months ago. Colonel Warburton's explorations embrace a portion of the interior of Western Australia hitherto unknown. The distance traversed is over 1,000 miles of longitude, the expedition having been conducted by means of camels, and was fitted out by the munificent liberality of the Hon. Thomas Elder, M.L.C., and Mr. W. W. Hughes. Another expedition under Mr. Gosse, conducted with horses at the expense of the Government of South Australia, has not been so successful. Mr. Gosse, amid many difficulties caused by want of water and the barren nature of the country through which he passed, managed to reach as far as E. long. 129° 59′ in lat. 26° 32′ S., the total distance traversed irrespective of numerous turnings and windings, being not less than 600 miles. His most notable discovery was made in lat. 25° 21′, long. 131° 14′, being a hill consisting of one solid rock (fine conglomerate) or huge natural monolith two miles long, one wide, and 1,100 feet high, with a spring coming from its centre; Mr. Gosse named it "Ayres Rock." Both expeditions are highly creditable to the enterprise of South Australia, which, as our readers know, has succeeded in carrying a line of telegraphy right across the country, from Port Augusta to Port Darwin.

THE enterprise of the Australian Colonies is producing really valuable scientific results, as will be seen from the following telegram, dated Dec. 22, published in the *Brisbane Courier*, from Mr. G. Elphinstone Dalrymple, commander of the Queensland North-east Exploring Expedition:—"The coasts, harbours, inlets, navigable rivers, and creeks have been examined from latitude 18° 15′ to 15° 15′ S. The Bellenden Kerr mountain range has been successfully ascended, and found to be a complete 'razor back' of granite. Palms were found on the summit; but although the botanical discoveries were interesting, they have not borne out all that was anticipated from them; 144 miles of soundings and 371 compass cross bearings have been taken in 19 navigable rivers and creeks of which the North and South Johnstone, the Mulgrave and Russell, drain the Bellenden Kerr range; the Mossman and Daintree drain the Arthur Palmer range inside the Schnapper Island. This range is nearly as lofty as the Bellenden Kerr, and is 25 miles in length. New rivers have been discovered penetrating a jungle-clad country of thoroughly tropical character, covered with a new rich soil suitable for sugar and other tropical cultivation. The extent of this country is roughly estimated at, in the aggregate, half a million acres, thus at once placing Queensland on a par with other favoured tropical countries. Mr. Hill has collected 3,000 botanical specimens, roots, and blocks of timber; 130 shells of five genera and eight species; 42 specimen bags of soils. Mr. Johnstone has collected 30 specimens of interesting birds, insects, and reptiles; and I have obtained 93 geological specimens."

THE French Society of Geography and the Commission delegated by the Syndical Chambers of Commerce of Paris have instituted a "Commission of Commercial Geography." This Commission has for its object—1. To spread in France, either by education or by publications, information relating to commercial geography; 2. To pursue the organisation or development, from an industrial and commercial point of view, of explorations in all quarters of the globe; to take part in researches relative to existing routes or to create new ones; 3. To point out the natural riches and the manufacturing processes which may be utilised by commerce and industry; 4. To inquire into all questions relating not only to the development of French colonisation, but also to the colonial systems of the various civilised nations. The Commission is divided into four sections corresponding to the above, and in the interval between the general meetings these sections hold stated *séances* for discussing questions, their decisions being submitted to the approbation of the Commission.

THE Paris Jardin d'Acclimatisation has just received a flock of six magnificent male ostriches and twelve females presented to it by General Lacroix-Vaubois, who holds a high command in Algeria. All attempts to breed these birds have hitherto proved futile, but a new attempt is to be made under the sun of Provence. The six ostriches will not remain long in Paris, and are to leave soon for Hyeres, where the Acclimatisation Society possesses a large estate.

THE Meteorological Society of France has decided upon holding its next Biennial Exhibition at the Palais de l'Industrie, Champs Elysées: it is to be an International one. The expenses being paid by the Government, no charge will be made for exhibiting. A special circular will be sent to the English Society this year.

THE *New York Tribune* in calling attention to the unauthentic character of a story to the effect that the non-existence of the companion star of Procyon, and of all except two of the satellites of Uranus, had been determined by the new telescope at Washington, announces the first important result obtained from this instrument. The recent observations have resulted in the rediscovery of the two smallest moons of Uranus, which have been not only distinctly seen on several occasions, but have actually been measured by Prof. Newcomb and his assistant, Prof. E. L. Holden. The two larger moons of Uranus, first discovered by Sir Wm. Herschel, are well-known objects, and can be seen under favourable circumstances with any telescope of 12 in. aperture. The two smallest were first discovered by Lassell, about twenty years ago, through the fine instrument attached to his private observatory near Liverpool; but his observations were very unsatisfactory (scarcely, indeed, determining the exact number of moons), and it was not until he renewed his researches at Malta that he obtained any accurate indications. Since that time, and until this re-discovery, no one has seen these satellites, and their detection and accurate observation through the Washington instrument is gratifying evidence of its superior power.

MRS. MARY TREAT publishes in the *American Naturalist* for December 1873 a remarkable contribution to our knowledge of the sensitiveness of the leaves of the sundew, her experiments being chiefly made on the large American species *Drosera filiformis*, the leaves of which capture and kill moths and butterflies two inches across. Her observations are in accordance with those already recorded on English species, that the motion of the glands is excited only by organic substances, or if for a very short time by mineral substances, that the excitement passes off almost immediately. The most astonishing of her observations is, however, that when living flies are pinned at a distance of half an inch from the apex of the leaf, the leaf actually bends towards the insect until the glands reach it and suck its juices. In the *Naturalist* for January is an account of Roth's observations on the irritability of the sundew, made nearly a century ago.

WE have before us the first number of what seems to us likely to be a most useful work—"Insects of the Garden; their habits," &c., by Dr. A. S. Packard. The present number contains 32 pp. with woodcuts and a coloured plate, and is published at 25 cents. It forms part of a work called "Half hours with Insects," to be completed in 12 parts.

AN advance sheet of the forthcoming number of Petermann's *Mittheilungen* contains an official account of the voyage of Count Wiltschek in the summer of 1872 to Spitzbergen and Novaya Zemlya in the yacht *Isbjorn*, the chief object of which was to plant a provision depôt in the Arctic Sea for the Austro-Hungarian expedition under Weyprecht and Payer in the *Tegethoff*. The account contains some valuable observations on the ocean-currents, temperature, weather, wind, &c., of the region, and the geology of Novaya Zemlya; collections of the fauna and flora of that island were made, and photographic views were taken. Nothing is known at present of the Austro-Hungarian expedition in the *Tegethoff*, though it is probable that she may be wintering somewhere on the coast of Siberia.

THE Government of Peru have for some years been expending vast sums of money in exploring the little known portions of Peru which lie to the west of the Andes, and Señor Raimondi, a scientific man of the highest character, has, in the service of the Government, been also exploring the remote valleys between the Cordilleras, and at the head waters of many of the rivers which flow down the northern slopes to the eastern plains, a work in which he has been engaged for twenty years. The announcement is now made that the labours of Señor Raimondi are to be utilised in the publication, by the Government, of a magnificent illustrated work, which is to embrace a narrative of his explorations, and the result of all his researches upon the geography, natural history and climate of Peru.

IN a "Note Additionelle," by Mr. Albert Lancaster, of the Belgian Academy, to Mr. W. T. Brigham's memoir on "Volcanic Manifestations in New England, 1638—1870," published by the Boston Society of Natural History, the author records a number of earthquakes omitted in Mr. Brigham's memoir. Taking all the recorded earthquakes in New England during the last three centuries, the author finds that ·2 occurred yearly during the seventeenth century, 1·2 during the eighteenth, and 2·0 during the nineteenth, though on account of the imperfect data of the seventeenth and eighteenth centuries, he thinks that 2·0 per annum may be taken as the average annual number of earthquake phenomena in New England. If the number of earthquakes during the three centuries be examined in reference to the months in which they occurred, it will be found that there are two distinct maxima and minima, both showing an equality almost to a unit; the former fall in February and November, the latter in April and September, and they are to each other as 3·6 : 1. Dividing the number of earthquakes according to the seasons in which they occurred, it is found that eighty-seven occurred in winter, forty-three in spring, forty-three in summer, and ninety-one in autumn. Enough is not yet known of the geological constitution of New England to enable us to explain these remarkable results, though it is hoped that the researches at present carried on by the U.S. geological officials may ere long enable us to do so.

THE existence of gigantic Cephalopoda in American waters has long been suspected, and at last a large specimen of a "squid," or sepia, has been captured and preserved. The measurements, as given by the Rev. M. Harvey, of St. John's, Newfoundland, are, length of body, 7 ft.; circumference, 5 ft.;

length of two tentacular arms, 24 ft. each; eight pedal appendages, 6 ft. in length, and 9 in. in circumference nearest the head; the sucking-discs are denticulated, and in some instances measure 1¼ in. in diameter. This individual has been preserved, and its measurements are therefore authentic; but still larger specimens are believed to exist, and an account is given of an encounter between some fishermen and a huge creature which, on being struck by them, attacked their boat by twining its arms round the vessel. Two of the arms were cut off by a fisherman, when the squid moved off, ejecting a large quantity of inky fluid to cover its retreat. A portion of one of these arms, measuring 19 ft., has been preserved, but it is said that 6 ft. of it were destroyed, while the fishers estimate that they left 10 ft. more on the body of the squid. This would bring its length to 35 ft. It is to be hoped that more care will in future be taken to prevent the mutilation of specimens; and the capture of the first-mentioned one will, no doubt, excite the fishermen and others to greater care and exertions in looking out for still larger examples. The first squid was caught in Logy Bay, Newfoundland. The encounter with the second took place off Portugal Cove, Conception Bay, about 9 miles from St. John's.

AT a recent meeting of the Essex Institute (Salem, Mass.), Mr. Byron Groce of Peabody read a paper on "The Study of Natural History in Schools," in which he advocated its introduction by substituting it for some of the less useful studies now pursued. He also gave an account of the method he had followed in the High School of Peabody, stating that during the summer he took his school into the woods and fields for a half day each week, taught the scholars to collect specimens and preserve them properly; then had the specimens arranged in the school cabinet, and on unpleasant days in the winter they were used for instruction. In this way a lively interest had been created in the school, and a Natural History Club had been formed among the scholars for the purpose of carrying on the study.

A NEW illustrated weekly newspaper is announced for first appearance on March 7. The title is the *Pictorial World*. We trust the projectors will be wise enough—to take the word in its largest sense—to let its readers know something of what is being done in the world of Science.

WE have received a map (published by the U.S. Geological Survey) of the sources of the Snake River, with its tributaries, together with portions of the head waters of the Madison and Yellowstone, from surveys and observations of the Snake River Expedition, by G. R. Bechler, Chief Topographer, and James Stevenson, Director. The scale is five miles to an inch, and all the remarkable features of the extensive district, which includes the Yellowstone National Park, and the nature and products of the ground, are clearly indicated.

WE have received the third volume, for 1873, and the commencement of the fourth volume of the "Procès-verbaux des Séances de la Société Malacologique de Belgique," showing the activity with which this department of Natural History is pursued in Belgium.

THE additions to the Zoological Society's Gardens during the past week include a Crested Agouti (*Dasyprocta cristata*), from Mexico, presented by Mr. C. H. M. de Lichtacbel; a Pennant's Parakeet (*Platycercus pennanti*), and a Cockateel (*Calopsitta novæ-hollandiæ*), from Australia, presented by Dr. H. Wheeler; a Common Gull (*Larus canus*), British, presented by Mr. W. K. Stanley; a Malayan Hornbill (*Buceros malayanus*), from Malacca, purchased; and a hybrid Pheasant (between *Thaumalia amherstiæ* and *T. picta*), received in exchange.

SCIENTIFIC SERIALS

Justus Liebig's Annalen der Chemie u. Pharmacie, Band 170, Heft 1 und 2. This number contains the following papers:—On the chlorides and oxy-chlorides of sulphur, by A. Michaelis. The author describes the compound SCl_4 and other chlorides, also an oxy-chloride of the formula $S_2O_3Cl_4$. He gives tables and curves showing the dissociation of SCl_4 which is very rapid between – 20° when 100 per cent. of that body exists and + 6°·2 when only 2·43 per cent. exists as SCl_4, the rest consisting of SCl_2 and chlorine. SCl_2 dissociates much less rapidly, 5·44 per cent. existing at a temperature of + 120. Researches on the nature and constitution of gallic acid, by H. Schiff. A correction of the formula of carbazolin, by C. Grabe.—On capronic acid contained in the crude butyric acid of fermentation, by A. Lieben—On the salts of caproic acid derived from fermentation, by F. Kottal.—On a condensation product of oxybenzoic acid, by L. Barth and C. Senhofer. The author's obtained a body of the formula $C_{14}H_8O_4$; two of the hydrogens of this formula are replaceable by metals. The Ba Ca, K and Na salts thus formed are described. On treating the original body with zinc, anthracene was obtained. They propose to call this body Anthraflavon; it is useless as a dye-stuff.—On phenol-trisulphuric acid, by C. Senhofer. The acid is prepared by acting on sulphuric acid with phosphoric anhydride in the presence of phenol; it is a tribasic acid of the formula $C_6H_6S_3O_{10}$ crystallising with 3½ molecules of water. Several of its salts are described.—On orthoxylol prepared from liquid brom-toluol, by P. Jannasch and H. Hubner.—On the action of ozone on carburetted hydrogen, by A. Houzeau and A. Renard. This is a translation from a paper in the *Comptes Rendus*, vol lxxvi., 572.—On dichlorglycide, by A. Claus. This body is prepared by the action of potassic hydrate on trichlorhydrin. On the action of potassic cyanide on dichlorglycide, by the same author.—On œnanthylic acid and normal heptyl alcohol, by Harry Grimshaw and C. Schorlemmer.—On trimethyl acetic acid, by A. Butlerow.—On the dichlor propionic ether from glyceric acid, by Messrs. Werigo and Werner.—Contributions to our knowledge of citric acid; and On baric citraconate, by H. Kammerer.—On dissociation, by A. Horstmann.—The number concludes with a short reply to Butlerow's paper on trimethyl carbinol, by E. Linnemann.

Bulletin de la Société Impériale des Naturalistes de Moscou. No. 2. 1873.—This number commences with a paper by Dr. Koch on malformations in the embryos of species of salmon and Coregonus; the various monstrosities being treated under the headings of (1) Dicephaly; (2) Diplomyelia (duplicity of spinal cord, total or partial; (3) Divergences of body from its long axis; (4) Defects in organs of locomotion; (5) Anomalies in the vegetative sphere; (6) Defects in organs of sense. As to the disputed point how double-monsters arise, Dr. Koch finds confirmation of I. Muller's view, according to which they are produced through union of two organised embryos arising from an imperfect fission. He also asserts that double monsters, where both bodies are formed alike, never live after leaving the egg, and when the yolk has been absorbed; other monsters may, if the form permits of food being procured. From experiments made on production of monsters, it seemed well established, that unfavourable conditions, such as shaking, were peculiarly apt to cause them; even double formations, but only of a certain kind, viz. *Diplomyelia partialis*, not *D. totalis*. The influence of temperature was also seen in the fact, that, with embryos in these unfavourable conditions, a difference of three degrees R (above the normal cold temperature), made a difference of twelve days in the time of development, which was to this extent retarded. The paper is accompanied with numerous illustrations.—M. Wolkenstein continues his account of anthropological researches on the ancient cemeteries of Waldai, named "Jalnikis;" giving here detailed measurements of the skeletons found in these peculiar tombs.—M. Becker describes a journey he made in 1872 to several places in the neighbourhood of the Caspian and in South Daghestan; apppending a list of plants and animals found there.—M. Trautschold furnishes some measurements of *Elasmotherium sibericum*; M. Motschoulsky a list of new species of Coleoptera; and there are one or two notes, in Russ, on botanical subjects.

Astronomische Nachrichten, No. 1973. In this number Dr. Stein gives an account of an apparatus for astronomical photography, with which the negatives are taken without the use of a dark room or tent, and if useful in practice, it justly deserves credit. It consists practically of a sort of flat box of glass, one side of which is the collodionised plate which fits water-tight against the other sides by means of india-rubber packing. There is a tube passing into this box through which first the ordinary silver solution is poured, and then by laying the collodionised plate downwards it is covered by the solution and sensitised; this is then drawn off, and the box, which is contained in a suitable holder, placed on the telescope and exposed by drawing away the non-actinic glass cover in front. After exposure the coloured glass is replaced, the box removed and developed by pouring in the solution in the same manner as the silver, in the meantime watching the plate through the coloured glass; the washing is then proceeded with in the same manner.—Dr. Stein proposes to use this method for photographing the transit of Venus.—Prof. Schmidt contributes a paper on the rotation of Jupiter, in which he discusses all the old observations of Cassini and others. From his list we gain that these observers differed to the amount of 6^m, the minimum being $9^h\ 50^m$, and the maximum $9^h\ 56^m$. From Prof. Schmidt's observations in 1873, he obtains a period of $9^h\ 56^m\ 7{\cdot}2^s$.

Archives des Sciences Physiques et Naturelles, Dec. 15. 1873.—In this number a short opening notice of M. de la Rive is followed by an article by M. Wiedemann, being an extract from his recent work on elliptic polarisation of light, and its relations with the superficial colours of bodies. The author shows that superficial colours change considerably with the nature (indices of refraction) of the substances in contact with which they are produced; and colours the most strongly reflected present generally the most intense elliptic polarisation, provided the reflection occurs in air or in vacuo. The principal angles of incidence undergo the most rapid modifications for wave-lengths corresponding nearly to the bands of absorption. M. Wiedemann's work elucidates the connection between the phenomena of bodies with superficial colours and the principal angles of incidence and relations of amplitude.—Dr. Hermann Muller's recent interesting work on fertilisation of flowers by insects is reviewed in a paper which gives a succinct *résumé* of the principal results.—M. Plantamour furnishes an account of the proceedings of the Meteorological Congress held at Vienna in 1873, and the circumstances which led to it.—There is also a note on the early development of Geryonides, by M. Fol; and this is followed by the usual scientific summary.

Ocean Highways, February.—About one-third of this number is occupied by a paper by Captain R. F. Burton, describing "Two Trips on the Gold Coast," the first being to the Beulah Gardens and Agrimanti Hills, and the second along the shore to the Volta River. The paper, which is written in Captain Burton's characteristic and attractive style, and illustrated by two maps, is full of information, and will no doubt prove interesting to many at the present time. An article on the Bengal Famine recounts the principal Indian famines from 1661 to the present time, and shows how much could be done to foresee and obviate the consequences of famine by a more scientific investigation of the laws which regulate meteorological phenomena. The article is accompanied by a map showing the extent of the famine districts. In a short article on "Wyche's Land," called by the Germans after King Karl of Wurtemburg, it is shown satisfactorily, we think, that the honour of the discovery, by right, belongs to Edge's expedition in 1617, and that the name then imposed should remain unchanged. Some interesting details are given of Richard Wyche or Wiche, the London merchant, who did much to encourage early discovery. Other articles are on "European Emigration to the Argentine Republic," and on the "Provindah Trade," or trade of the Lohani merchants, who are the channels of communication between India and Central Asia.

SOCIETIES AND ACADEMIES

Royal Society, Feb. 12.—"On the Division of Sound by a layer of flame or heated gas, into a reflected and a transmitted portion," by John Cottrell, Assistant in the Physical Laboratory of the Royal Institution; communicated by Prof. Tyndall, F.R.S.

A vibrating bell contained in a padded box was directed so as to propagate a sound-wave through a tin tube and its action rendered manifest by its causing a sensitive flame placed at a distance in the direction of the sound-wave to become violently agitated.

The invisible heated layer immediately above the luminous portion of an ignited coal-gas flame issuing from an ordinary bat's-wing burner was allowed to stream upwards across the end of the tin tube, from which the sound-wave issues. A portion of the sound-wave, issuing from the latter, was reflected at the limiting surfaces of the heated layer; and a part being transmitted through it, was now only competent to slightly agitate the sensitive flame.

The heated layer was then placed at such an angle that the reflected portion of the sound-wave was sent through a second tin tube (of the same dimensions as the above), and its action rendered visible by its causing a second sensitive flame placed at the end of the tube to become violently affected. This action continued so long as the heated layer intervened; but upon its withdrawal the first-mentioned sensitive flame, receiving the whole of the direct pulse, became again violently agitated, and at the same moment the second sensitive flame, ceasing to be affected, resumed its former tranquillity.

Feb. 19.—"On the Number of Figures in the Period of the Reciprocal of every Prime Number below 17,000," by William Shanks, Houghton-le-Spring, Durham.

"On an Instrument for the Composition of the Harmonic Curves," by E. A. Donkin, Fellow of Exeter College, Oxford.

"On the Absorption of Carbonic Acid by Saline Solutions," by J. Y. Buchanan, chemist on board H.M.S. *Challenger*.

Linnean Society, Feb. 19.—J. Gwyn Jeffreys in the chair.—The chairman announced that a Special General Meeting of the Society would be held on Thursday, March 5, at 8 P.M., to consider alterations in the Bye-laws of the Society." The following papers were read:—Systematic list of the Spiders at present known to inhabit Great Britain and Ireland, by the Rev. O. P. Cambridge.—Some observations on the vegetable productions and rural economy of the province of Baghdad, by Surgeon-major W. H. Colvill.—Note on the Bracts of Crucifers, by Dr. M. T. Masters.

Zoological Society, Feb. 17.—George Busk, F.R.S. vice-president, in the chair. Mr. Busk exhibited some skulls of the tiger and leopard from China, procured by Mr. R. Swinhoe, and showed that those from the northern and southern provinces did not appear to be specifically distinct.—A communication was read from Mr. L. Taczanowski, Conservator of the Museum of Warsaw, containing the descriptions of twenty-four new birds, obtained by Mr. Constantine Jelski in Central Peru. Amongst these was a new Cotingine form, proposed to be called *Doliornis sclateri*, and four new humming-birds named respectively *Metallura hedvigæ*, *Heliantha dichroura*, *Eriocnemis sapphiropygia* and *Lampraster branickii*.—A communication was read from Sir Victor Brooke, Bart., describing a new species of Gazelle, founded on two specimens living in the Society's Menagerie, which he proposed to call *Gazella muscatensis*.—A communication was read from Dr. T. Schomburgh, Director of the Botanic Gardens, Adelaide, containing an account of the habits of the Australian Coote (*Fulica australis*) as observed in the Gardens under his charge.—Mr. E. Ward exhibited the head of a supposed new species of Wild Sheep, from Ladak, which he proposed to name *Ovis brookei*, after Sir Victor Brooke.—Dr. J. E. Gray, F.R.S., communicated some notes on the Crocodile of Madagascar, which he proposed to distinguish from *Crocodilus vulgaris* of Continental Africa, and to call *Crocodilus madagascariensis*.—A communication was read from Mr. W. N. Lockington, of Humboldt County, California, containing some notes on the mammals and birds met with in that part of the State of California.

Mathematical Society, Feb. 12.—Dr. Hirst, F.R.S., president, in the chair.—Prof. Clifford gave in some detail a statement of the views advanced in his paper on the foundations of dynamics.—A discussion ensued, in which Messrs. Wilkinson, Moulton, Cayley, Roberts, and G. H. Lewes took part.—Mr. Clifford having answered questions and replied to objections, proceeded next to give an account of a paper on the free motion of a solid in elliptic space.—Owing to the lateness of the hour a paper by Mr. C. J. Monro, entitled "Note on the Inversion of Bernoulli's Theorem in Probabilities," was taken as read. Under the name of Bernoulli's Theorem are comprehended two theorems which, with a little licence, we may distinguish as the deductive and the inductive. The deductive theorem assumes the constant probability p of a given result on a single trial, and determines the probability P that on m trials the result will be produced from $mp - l$ to $mp + l$ times, or from $x - l$ to $x + l$, if x is the greatest integer in $mp + p$. The inductive theorem assumes that the given result is produced mp times on m trials, which give a constant facility for its production (that is, are made under definable circumstances, which, if defined, would give a constant probability for the same), and determines P′, the probability that this facility lies between $p \pm \frac{l}{m}$. In the deductive theorem it is supposed that $\frac{1}{m}$ may be neglected; and in the inductive $\frac{1}{\sqrt{m}}$: (The author here refers to Mr. Todhunter's "History," p. 555, and to Mr. De Morgan's treatise in the "Enc. Metr.," § 77.) The object of the paper was to show, first that there is an oversight in Laplace's statement of the inversion (see Todhr., § 997), the correction of which removes the inconsistency of the results; and secondly, that upon the hypothesis of equally probable values within equal ranges, the inversion is so far legitimate that either theorem may be inferred from the other with little calculation, and in particular without the approximate evaluation of a general integral, and accordingly that the two solutions are identical in principle.

Chemical Society, Feb. 19.—Prof. Odling, F.R.S., president, in the chair.—Mr. James Bell delivered his lecture "On the Detection and Estimation of Adulteration in Articles of Food and Drink." The lecturer, after some preliminary remarks on the fiscal regulations with regard to adulteration, began with a description of the microscopic appearance of the various kinds of starch, as many of them, from their cheapness, are largely employed for the purposes of adulteration; he then considered the characters of pure coffee and of the various substances used to adulterate it, pointing out the most convenient methods for their detection. Tea, pepper, and mustard, were afterwards treated of in the same way. Owing to want of time, Mr. Bell was unable to complete the lecture, so that the adulteration of cocoa, tobacco, and beer was not touched upon. This admirable and instructive lecture was copiously illustrated by the most beautifully executed drawings of the structure of the various substances as exhibited under the microscope. After the lecture many of the Fellows availed themselves of the opportunity afforded them of looking over the extensive collection of microscopic preparations connected with the subject.

Entomological Society, Feb. 2.—Mr. J. W. Dunning, vice-president, in the chair.—Mr. Muller exhibited a blind Myriapod and others found in a limestone cave in the Jurassian Mountains; he believed them to be the first found in the caves of Switzerland.—Mr. Kirby exhibited *Lycæna phœbe* from Australia, which had been described by the Rev. R. P. Murray.—Specimens were exhibited of *Monohammus leuconotus*, a Longicorn beetle which was very destructive to the coffee plantations in Natal. The only remedy that appeared to have been tried was the application of Stockholm tar to the roots of the trees; but handpicking was suggested on the first appearance of the insect in the imago state. This was the practice usually adopted on the continent of Europe with regard to *Melolontha*. Also it was desirable to protect the insectivorous birds, which were frequently shot for the sake of their plumage.—Mr. Butler forwarded some corrections of the synonymy with regard to *Apatura herse* and *A. lycaon* of Scudder and Riley, which were equivalent to *A. clyton* and *A. celtis*, Boisduval; whereas *A. herse* and *A. lycaon*, Fabricius, were sexes of one species = *A. alicia*, Edwards.—A paper was communicated by Mr. Herbert Druce, entitled "Descriptions of fifteen species of Diurnal Lepidoptera, chiefly from South America."

Meteorological Society, February 18.—The papers read were:—"General Remarks on the West Indian Cyclones, particularly those from the 9th to the 21st Sept., 1872," by Mr. F. H. Jahncke, harbour-master of St. Thomas; "New Forms of Alcohol Thermometers," and "An Improved Vacuum Solar Radiation Thermometer," both by Mr. James J. Hicks; and "Note on a Waterspout which burst on the Mountain of Ben Resipol, in Argyleshire, in August, 1873," by Mr. Robert H. Scott, F.R.S. A very interesting discussion followed the reading of each paper. That upon Mr. Jahncke's led to expressions of opinion on the origin, form, tracks, and general characteristics of West Indian Hurricanes, and of the best means of improving and increasing the records of weather phenomena in those parts. The special feature in Mr. Hicks's second paper was the application of an electric current as a test for the perfection of the vacuum, which principle was very beautifully illustrated by experiments.

EDINBURGH

Scottish Meteorological Society, Jan. 29.—Mr. M. Home, of Wedderburn, in the chair.—From the report of the council it appears that two new stations, viz., Broadlands, Peebleshire, and Ochtertyre, Crieff, have been added to the society's stations, and that Kettins and Cairndow have ceased to be stations. Thus the number of stations in connection with the society is the same as at last meeting, viz., 92 in Scotland, 5 in England, 4 on the Continent, 2 in Iceland, 1 in Faro, and 1 in South America. Observations have also been begun to be made for the society at Melstad, in the north of Iceland, and at Fairlie Plains, Paroo River, near the northern watershed of the River Darling, Australia. The council had had offers of many more stations, some in most eligible districts; but the establishment of these would have entailed additional expenditure which the society's funds would not justify. Teachers of several schools had also made known their wish to observe for the society, provided they were furnished with instruments, at the same time proposing to introduce into their schools some instruction in meteorology. The council, however, had been obliged to decline these applications for want of funds. The membership of the society is at present 560. In room of the three members of council who retired, Prof. Alexander Dickson, Dr. J. Robson-Scott, and Mr. George Hope, of Broadlands, were elected.—An application has been made to the council by Mr. Colin McVean on behalf of the Government of Japan for advice regarding the establishment of a system of meteorological observations in Japan. In answer to this application, the council has forwarded a memorandum regarding suitable instruments, their position, hours of observation, the establishment of a central observatory, inspection of stations, publications, and special observations of storms.—Mr. Buchan submitted a second report of the committee appointed to carry out the Marquis of Tweeddale's proposal to investigate the relations of the herring-fisheries to meteorology. The committee had, with the assistance of the Hon. Bouverie F. Primrose, of the Fishery Board, obtained complete returns of the daily catch of herrings and state of the weather from all the fishing districts of Scotland during the past season. Thirty-five weather maps at 9 P.M., specially constructed with reference to this question, and showing the number of boats out fishing in each district each day and the average catch of each boat, were shown to the meeting. Some interesting relations between the catches of the different districts and the prevailing weather were pointed out; and as these were in general accordance with the results stated in the first report, presented in July last, it is highly probable that when the statistics of three or four years' fishings similar to the very satisfactory returns of the past year have been collected, valuable conclusions will be arrived at.—Mr. Thomas Stevenson, in bringing before the meeting a proposed inquiry regarding storms, remarked that the barometric gradients hitherto ascertained having been deduced from readings at stations many miles apart, necessarily could not give more than a rough approximative gradient. What is wanted in order to get a formula for computing the velocity of the wind due to a given gradient is, as he (Mr. Stevenson) suggested in NATURE, vol. ix. p. 103, to have a string of stations at short distances apart. It is now proposed to establish such storm stations, arranged in lines radiating from Edinburgh for a distance of about twenty miles, and it is believed that in addition to the existing stations of the Scottish Society many farmers and others possess good barometers, which could be compared with the society's standard. It is proposed that observations of the instruments and of the weather should be limited to the periods during which storms last, and a special schedule for the observations had been prepared.—Mr. Buchan gave an account of the proceedings of the Meteorological Congress held at Vienna in September last, to which he and Mr. Scott, of the Meteorological Office, London, had been sent as delegates from the British Government.

Geological Society, Feb. 12.—A paper was read by Mr. John Horne, of the Geological Survey of Scotland, on "The Geology of the Isle of Man." The chief points of interest in the paper were the correlation of the red sandstones and Breccias with the Lower Carboniferous series of Scotland, and the proofs adduced that the volcanic rocks were probably on the same horizon as the upper limestone shales of England. Detailed evidence was given to show that the Isle was glaciated by a confluent ice-sheet from the north-west of England, south of Scotland, and the north-east of Ireland. The two submarine hollows lying between the Portpatrick coast and the north-east of Ireland, and between Anglesea and the coast south of Dublin, were attributed to the increased erosive action of the ice-sheets due to the narrowness of the channel at these points.—Mr. Andrew Taylor gave a description of the course of the River Almond, near Edinburgh, and stated that that river followed, at various places, which he specified, lines of "faults."

MANCHESTER

Literary and Philosophical Society, Feb. 3—*Physical and Mathematical Section.*—Alfred Brothers, F.R.A.S., President of the Section, in the chair.—"On the Theory of the Tides," by David Winstanley.

Feb. 10.—R. A. Smith, F.R.S., V.P., in the chair.—"The Northern Range of the Basques," by W. Boyd Dawkins, F.R.S. The northern extension of the Basque race from their present boundary, in ancient times, is demonstrated by the convergent testimony of history, ethnology, and the researches into caves and tombs. In the Iberian peninsula the Basque populations (Vascones) of the west are defined from the Celtic of the east by the Celtiberi inhabiting modern Castille. In Cæsar's time, the Aquitani were surrounded on every side, except the south, by the Celtæ, extending as far north as the Seine, as far to the east as Switzerland and the plains of Lombardy, and southwards, through the valley of the Rhone and the region of the Volscæ, over the Eastern Pyrenees into Spain. The district round the Phocæan colony of Marseilles was inhabited by Ligurian tribes, who held the region between the river Po and the Gulf of Genoa, as far as the western boundary of Etruria, and who probably extended to the west along the coast of Southern Gaul as far as the Pyrenees. The ancient population of Sardinia is stated by Pausanias to be of Libyan extraction, while that of Corsica is described by Seneca as Ligurian and Iberian. The Basques, or Ligurians, are the oldest inhabitants, in their respective districts, known to the historian; while the Celts appear as invaders. We may be tolerably certain that the Basques held France and Spain before the invasion of the Celts, and that the non-Aryan peoples were cut asunder, and certain parts of them left—Ligurians, Sikani, and in part Sardinians and Corsicans—as ethnological islands, marking, so to speak, an ancient Basque non-Aryan continent which had been submerged by the Celtic populations advancing steadily westwards. The Celtic and Belgic invasion of Gaul repeated itself, as might be expected, in Britain. Just as the Celts pushed back the Iberian population of Gaul as far south as Aquitania, and swept round it into Spain, so they crossed over the Channel and overran the greater portion of Britain, until the Silures, identified by Tacitus with the Iberians, were left only in those fastnesses that formed subsequently a bulwark for the Brit-Welsh against the English invaders. The Basque non-Aryan blood is still to be traced in the dark-haired, black-eyed, small, oval-featured peoples in our own country in the region of the Silures, where the hills have afforded shelter to the Basque populations from the invaders. The small swarthy Welshman of Denbighshire is, in every respect, except dress and language, identical with the Basque peasant of the Western Pyrenees, at Bagnéres de Bigorre. The small dark-haired people of Ireland, and especially those to the west of the Shannon, according to Dr. Thurnam and Professor Huxley, are also of Iberian derivation, and, singularly enough, there is a legendary connection between that island and Spain. The human remains from the chambered tombs as well as the river-beds prove that the non-Aryan population spread over the whole of Ireland as well as the whole of Britain. The evidence offered by an appeal to history and ethnology, as to the former northern extent of the Basque peoples, is confirmed by an examination of the human remains in the Neolithic caves and tombs, scattered throughout the area under consideration. The discoveries in the caves of Gibraltar and of the Spanish mainland prove that a small long-headed race, with delicate features and orthognathic profile identical with the Basques who buried their dead in the modern cemetery of Guipuscoa, ranged throughout the Peninsula, using with indifference caves and chambered tumuli for their tombs. And on the same grounds their former range through France, Britain, and Ireland is demonstrated, and as far to the east as Belgium. At the present time the Basque blood asserts itself in the physique of certain isolated populations, and within the historic period is demonstrated to have been more strongly defined, and to have occupied larger areas, and lastly in the prehistoric period to have formed one continuous race from the Pillars of Hercules, as far north as Scotland, and as far to the east as Belgium.

NEW HAVEN, U.S.

Connecticut Academy, Dec. 17, 1873.—Prof. Lyman, president, in the chair.—Prof. Marsh, of Yale College, gave an account of the explorations of his party in the Rocky Mountains and on the Pacific Coast during the past season. The first explorations this year were made in the Pliocene deposits near the Niobrara River. Owing to hostile Indians, the explorations of the party here were attended with much difficulty and danger, but were on the whole quite successful. Many new animals were discovered, and ample material secured for a full investigation of those previously known from that region. A second expedition was made in August from Fort Bridger, Wyoming, and large collections of Eocene fossil vertebrates were obtained, especially of the *Dinocerata*, *Quadrumana*, and *Chiroptera*, which had first been brought to light by the researches of the party in previous years. A third trip was made in September to the tertiary beds of Idaho and Oregon, where some interesting discoveries were made.

PARIS

Academy of Sciences, Feb. 16.—M. Bertrand in the chair.—The following papers were read:—On the acid waters which flow from the volcanoes of the Cordilleras, by M. Boussingault.—On a mechanical equation corresponding to the equation $\int \frac{dQ}{T} = 0$, by M. R. Clausius. This was a paper relating to those of M. A. Ledieu on the same subject which have recently been read.—Report on a memoir, by M. Marey, on the point of action of a wing on the air, M. Tresca, reporter.—Experiments to determine whether all the vascular nerves have their focus of origin and their vaso-motor centre in the rachidian bulb, by M. A. Vulpian.—New topographical chart of Mont Blanc on a scale of $\frac{1}{40,000}$, by M. E. Viollet-Leduc.—M. Ad. Chatin advanced his paper On androgenesis compared with organogenesis another stage.—On the action of soft waters on metallic lead, by MM. Mayençon and Bergeret. Electrolysis was used by the authors to detect the lead, as they considered sulphuretted hydrogen not sufficiently delicate. They found galena slightly soluble in water by long boiling.—On the preservation of wood by means of cupric sulphate, by M. Boucherie.—Facts illustrating the history of yeast, by M. P. Schutzenberger.—On a transformation of Taylor's formula, by M. Jourgon.—On a method of determining vapour densities, by M. Croullebois. This method is a modification of that which depends on observing the tension of the vapour in a barometer tube.—Observations on the efflorescence of the two hydrates of sodic sulphates, by Dr. L. C. de Coppet. This was an answer to a late paper by M. Gernez.—On the "antifermentescible" and antiputrid properties of solutions of chloral hydrate, by MM. Dujardin-Beaumetz and Hirne.—On the method of respiration in certain fish having a labyrinthiform pharynx, by M. Carbonnier.—On the fossils brought from Cape Verde Islands by M. de Cessac, by M. P. Fischer.—On the movements of the chlorophyll in the *Selaginaceæ*, by M. Ed. Prillieux.—On the relations between thermo-electric properties and crystalline form, by M. C. Friedel.—On a method of quickly re-forming vineyards threatened by phylloxera by the introduction of American vines, by M. H. Bouschet.—On anesthesia produced by the injection of chloral, by M. Oré.

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THURSDAY, MARCH 5, 1874

PROFESSOR HUXLEY AT ABERDEEN

THE Address just given by the Lord Rector of Aberdeen University, and published *in extenso* in the March number of the *Contemporary Review*, is second in importance to none of the similar utterances which have been heard of late years. It bears in every line the stamp of a master mind. The many topics touched on, the apparent diversity of which has alarmed the shallow critic of the *Times*, are all grouped round one central idea—the advancement of Science; and there is not only a splendid unity throughout the Address, and no "uncertain sound," which, coming as it does from a Royal Commissioner charged with a special survey of our scientific needs, as well as a Lord Rector, may well fill us with confidence for his advocacy, even if one despairs of much improvement being effected in the lifetime of the present generation. It is indeed to be feared, as Mr. Huxley himself anticipates, that on many points he will be "The Rector who was always beaten;" if so, it is none the less certain that his defeats will become "victories in the hands of his successors."

It is especially fitting that the Address, dealing, as it did by its title, with "Universities: Actual and Ideal," should have been delivered in connection with one of the Scotch Universities, which, in regard to scientific research and teaching, rank higher than the older English Universities, given up in the main to "elementary teaching of youths under twenty," as the ideal University must take rank above them. We cannot too much thank Prof. Huxley for bringing out this point sharply, and quoting Mr. Mark Pattison to intensify it, all the more because the *Times* has taken hold of another sentence of the address, to point out the importance of a "pause" in the Reforms at Oxford and Cambridge, as if things were moving too fast! Surely the older English Universities may at least approach the level of the Scotch Universities, to say nothing of the French and German ones, in the matter of the higher teaching and of research before this "pause" is insisted on?

And, more than this, we conceive it to be possible that the present Government may not treat the Report of the Commission appointed to inquire into the Revenues of the Colleges at Oxford and Cambridge as mere waste paper. It has frequently been roundly asserted that the political distinctions between Liberals and Conservatives by no means represent the line of demarcation between those most and least anxious for University reforms. However this may be, it is well known that one of the most enlightened and far-seeing among University reformers, so far as the highest functions of a University are concerned, is a member of the present Government. Let us hope, therefore, that the magnitude of the pause may have been exaggerated; that the Heads after all may not oversleep themselves, that the last of Endowment may be even as the first, Endowment being, according to Professor Huxley, a foreign element,

> "Which silently dropped into the soil of Universities like the grain of mustard-seed in the parable; and, like that grain, grew into a tree in whose branches a whole aviary of fowls took shelter. . . . . It differed from the preceding, in its original design to serve as a prop to the young plant, not to be a parasite upon it. The charitable and the humane, blessed with wealth, were very early penetrated by the misery of the poor student. And the wise saw that intellectual ability is not so common or so unimportant a gift that it should be allowed to run to waste upon mere handicrafts and chares. The man who was a blessing to his contemporaries, but who so often has been converted into a curse, by the blind adherence of his posterity to the letter, rather than to the spirit, of his wishes—I mean the 'pious founder'—gave money and lands, that the student who was rich in brain and poor in all else might be taken from the plough or from the stithy, and enabled to devote himself to the higher service of mankind; and built colleges and halls in which he might be not only housed and fed, but taught.
>
> "The colleges were very generally placed in strict subordination to the University by their founders; but, in many cases, their endowment, consisting of land, has undergone an 'unearned increment,' which has given these societies a continually increasing weight and importance as against the unendowed, or fixedly endowed, University. In Pharaoh's dream the seven lean kine ate up the seven fat ones. In the reality of historical fact, the fat Colleges have eaten up the lean Universities."

We have already, in NATURE, referred to Prof. Huxley's suggested reforms in respect to the Medical Curriculum, and we may therefore pass lightly over this part of his Address, expressing a hope, however, that his reference to this subject at length may be indicative that it will be considered by the Commission of which he is so distinguished a member.

The Lord Rector points out that while he would drop Zoology and Botany in the Medical Curriculum, he would make them part of the Arts Curriculum; and after remarking that the Faculties of Theology, Law, and Medicine are technical schools, intended to equip men who have received general culture with the special knowledge which is needed for the proper performance of the duties of clergymen, lawyers, and medical practitioners, he adds,—

> "I have no sort of doubt that, in view of the relation of Physical Science to the practical life of the present day, it has the same right as Theology, Law, and Medicine, to a Faculty of its own in which men shall be trained to be professional men of science. It may be doubted whether Universities are the places for technical schools of Engineering, or Applied Chemistry, or Agriculture. But there can surely be little question that instruction in the branches of Science which lie at the foundation of these Arts, of a far more advanced and special character than could, with any propriety, be included in the ordinary Arts Curriculum, ought to be obtainable by means of a duly organised Faculty of Science in every University.
>
> "The establishment of such a Faculty would have the additional advantage of providing, in some measure, for one of the greatest wants of our time and country. I mean the proper support and encouragement of original research."

This at once brings us to what we consider by far the most important part of the Address, the Lord Rector's opinions on the endowment of unremunerative research:—

> "The other day, an emphatic friend of mine committed himself to the opinion that, in England, it is better for a man's worldly prospects to be a drunkard,

than to be smitten with the divine dipsomania of the original investigator. I am inclined to think he was not far wrong. And, be it observed, that the question is not, whether such a man shall be able to make as much out of his abilities as his brother, of like ability, who goes into Law, or Engineering, or Commerce; it is not a question of 'maintaining a due number of saddle horses,' as George Eliot somewhere puts it—it is a question of living or starving.

"If a student of my own subject shows power and originality, I dare not advise him to adopt a scientific career; for, supposing he is able to maintain himself until he has attained distinction, I cannot give him the assurance that any amount of proficiency in the Biological Sciences will be convertible into, even the most modest, bread and cheese. And I believe that the case is as bad, or perhaps worse, with other branches of Science. In this respect Britain, whose immense wealth and prosperity hang upon the thread of Applied Science, is far behind France, and infinitely behind Germany.

"And the worst of it is, that it is very difficult to see one's way to any immediate remedy for this state of affairs which shall be free from a tendency to become worse than the disease.

"Great schemes for the Endowment of Research have been proposed. It has been suggested, that Laboratories for all branches of Physical Science, provided with every apparatus needed by the investigator, shall be established by the State; and shall be accessible, under due conditions and regulations, to all properly qualified persons. I see no objection to the principle of such a proposal. If it be legitimate to spend great sums of money on public Libraries and public Collections of Painting and Sculpture, in aid of the man of letters, or the Artist, or for the mere sake of affording pleasure to the general public, I apprehend that it cannot be illegitimate to do as much for the promotion of scientific investigation. To take the lowest ground, as a mere investment of money, the latter is likely to be much more immediately profitable. To my mind, the difficulty in the way of such schemes is not theoretical, but practical. Given the laboratories, how are the investigators to be maintained? What career is open to those who have been thus encouraged to leave bread-winning pursuits? If they are to be provided for by endowment, we come back to the College Fellowship system, the results of which, for Literature, have not been so brilliant that one would wish to see it extended to Science; unless some much better securities than at present exist can be taken that it will foster real work. You know that among the Bees, it depends on the kind of cell in which the egg is deposited, and the quantity and quality of food which is supplied to the grub, whether it shall turn out a busy little worker or a big idle queen. And, in the human hive, the cells of the endowed larvæ are always tending to enlarge, and their food to improve, until we get queens, beautiful to behold, but which gather no honey and build no comb.

"I do not say that these difficulties may not be overcome, but their gravity is not to be lightly estimated."

It is pointed out that the creation of Faculties of Science will, to a certain extent, remedy the present lamentable condition of things to which we have so often called attention.

"It is possible to place the scientific inquirer in a position in which he shall have ample leisure and opportunity for original work, and yet shall give a fair and tangible equivalent for those privileges. The establishment of a Faculty of Science in every University implies that of a corresponding number of Professorial chairs, the incumbents of which need not be so burdened with teaching as to deprive them of ample leisure for original work. I do not think that it is any impediment to an original investigator to have to devote a moderate portion of his time to lecturing, or superintending practical instruction. On the contrary, I think it may be, and often is, a benefit to be obliged to take a comprehensive survey of your subject; or to bring your results to a point, and give them, as it were, a tangible objective existence. The besetting sins of the investigator are two: the one is the desire to put aside a subject, the general bearings of which he has mastered himself, and pass on to something which has the attraction of novelty; and the other, the desire for too much perfection, which leads him to

"'Add and alter many times
Till all be ripe and rotten;'

to spend the energies which should be reserved for action, in whitening the decks and polishing the guns.

"The necessity for producing results for the instruction of others, seems to me to be a more effectual check on these tendencies than even the love of usefulness or the ambition of fame."

It would indeed be a happy solution of the difficulty if it could be solved in this way, but we confess that on this point we fear that the system advocated by Mr. Huxley will not be all that is needed.

In the first place, take the present appointments to Chairs; are they, as a rule, given to the most distinguished investigators? If not, why not, and why should the present system be altered? In our opinion the present system of appointing teachers is good so long as large ranges of knowledge have to be professed. Take many of our present professors; are they as encumbered by teaching as the German professors are for instance? and yet where are their researches? do they not figure much more often in the "List of Examiners" than in the "Philosophical Transactions"? If these things are so, no benefit will accrue from a mere increase of numbers unless the present pay be largely increased.

There is also another most important point, and here again we quote from the Address:—

"It is commonly supposed that anyone who knows a subject is competent to teach it; and no one seems to doubt that anyone who knows a subject is competent to examine in it. I believe both these opinions to be serious mistakes: the latter, perhaps, the more serious of the two. In the first place, I do not believe that anyone who is not, or has not been, a teacher is really qualified to examine advanced students. And in the second place, examination is an art, and a difficult one, which has to be learned like all other arts."

Are then investigators to be made teachers and examiners in order that they may live, regardless of the fact that they cannot teach, and though they may be ignorant of the "art" of examining?

We believe that powers of teaching and powers of investigation by no means go together, though they are united in some great men like Mr. Huxley; and we believe, further, that on this ground alone the idea of making a man teach in order that he may carry on researches is bad in principle: it is even worse than this, because it is apt to cause the public to underrate research—to think that the end of all research is to teach, while in point of fact the end and aim of the acquisition and teaching of all old knowledge is the acquirement of new knowledge.

It is a source of satisfaction to us that Prof. Huxley agrees with us on the main point, for we are certain that when once the principle is conceded, practical methods of carrying it out, among which undoubtedly that insisted on by the Lord Rector will find place, can easily be found; methods against which no objection can be urged, and from the application of which a tremendous increase in the rate of advancement of knowledge in this country may be anticipated.

POST-TERTIARY GEOLOGY*

The Great Ice-Age and its relation to the Antiquity of Man. By James Geikie. (W. Isbister and Co. 1874.)

II.

WE must next turn to beds which furnish conclusive proof of a return of cold conditions, the well-known shell-bearing clays found here and there along the coast of Scotland. The fossils and the physical condition of these beds both concur in telling the same tale, that an Arctic climate again prevailed in Britain. These deposits are marine, and have not been met with at a greater height above the sea than 360 feet, and they were therefore formed towards the termination of the period during which the land was emerging from the sea. Evidence of a similar change of climate is, however, found in the interior of the country. In the Highland glens and the high valleys of the Southern Uplands morainic deposits, distinguishable from those of the earlier ice period, are of common occurrence, sometimes scattered loosely over the mountain slopes, sometimes arranged in ridges or lines of mounds across the valleys after the fashion of terminal moraines. The climate, therefore, must have become again severe enough to allow of the accumulation of ice; but, since the second set of glaciers is shown by the moraines which they have left behind them to have been confined to the high ground, and each restricted to its own valley, the cold must have been far less intense than during the period of the first glaciation.

The second period of cold, however, passed away, and the record of its gradual disappearance is written for us in this way. In many of the upland valleys concentric lines of mounds, each marking the terminal moraine of a glacier, are arranged one within the other, and as we ascend these piles are found to grow more and more puny, till they at last vanish altogether. From this we see, as clearly as if the operation had gone on before our eyes, how each glacier shrank back step by step into the heart of the mountain glens, and at last yielded to the gradual amelioration of the climate, and melted entirely away. Another train of reasoning leads us to the same conclusion. The rising of the land was not continuous, but broken every now and then by pauses, and during each of these the sea cut a notch or shelf in the rocks and occasionally spread out terraces of shingle and silt, forming what are known as Raised Beaches. These beaches occur at many different levels, from 1,500 feet down to a few yards above the mean-tide level. The higher of these beaches furnish evidence of somewhat Arctic conditions, but as we descend in the series these traces become less pronounced.

We are now approaching the close of the glacial epoch, and the climate, though still colder than now, was approximating to what it is at present.

The author goes on to show, from a consideration of submerged forests, how the elevation of the land went on till Britain was raised above its present level, and probably connected by a land surface with the mainland of Europe; and points out how the continental climate thus produced will account for the dense forests which formerly clothed our island, while a return to insular conditions resulted in a decay of the woods and the growth of peat mosses.

Lastly, our country became again dissevered from the continent, and the submergence which brought about this change went on till the land was sunk somewhat below its present level; while it rose into its present position, low level raised beaches were formed, among which the well-known 25-feet-beach is most conspicuous.

Such then is the succession of physical changes which the Drift-deposits show has taken place in our island.

The author has passed in review also the contemporaneous formations of Scandinavia, Switzerland, and North America, and pointed out how the story they tell agrees in its main features with that deduced from our own glacial formations.

Had he done no more than this he would have produced a work of surpassing interest and value, but the concluding chapters of his book will perhaps attract more attention than any other part of it, for they deal with a question that comes in a measure personally home to us, the antiquity of man and the date of his first appearance in Britain.

The oldest races of men of which traces have yet been discovered are known as the Stone-folk, because they fashioned their implements out of stone and seem to have been unacquainted with the use of metals. These Stone-folk are clearly distinguishable into two classes—the older, known as Palæolithic, merely chipped stones into shape; the later, or Neolithic, had advanced a step farther, and constructed tools highly polished and otherwise more finished than those of their predecessors. We also find associated with the traces of Palæolithic man a group of mammals now wholly or locally extinct, while the mammals accompanying the remains of Neolithic man are many of them still indigenous to the country. In connection with this subject the author has brought prominently into notice a fact which had not received the attention it deserves, that nowhere have any signs been detected of gradual improvement on the part of Palæolithic man, by which he may have passed from abject barbarism to the more advanced skill of his Neolithic successor, but that, on the contrary, the two races are everywhere sharply marked off from one another. In the same way the accompanying groups of mammals are essentially distinct, and we nowhere find traces of the dying out of the one and the gradual coming in of the other. But one inference can be drawn from these facts: between the time when the Palæolithic race inhabited Britain and the coming in of the Neolithic race a long interval must have elapsed, during which man was by some means or other driven out of the country, and went through elsewhere the long series of modifications by which he was himself advanced in civilisation, while at the same time the group of animals associated with him became totally changed. Now we know of no physical change since the second glaciation of the country which could have been the cause of such a migration, for all the evidence both here and elsewhere tends to show, that whatever change of climate has occurred between that event and the present day has been steadily in the same direction—that of improvement. But the great submergence, and severe period which followed it, would exactly bring about the required result, if it can be only shown that the age of Palæolithic man preceded these occurrences.

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* Continued from p. 321.

There is no antecedent improbability in such a supposition; the mild periods that recurred during the formation of the Till may well have been warm enough to allow of northern mammals, and subsequently, as the climate improved, of Palæolithic man and southern forms migrating into our area, to be again driven out each time a return of cold brought the ice-sheet down over the lowlands, and finally expelled, never again to return, by the great submergence. But more than this, our author has shown how anomalies, hitherto inexplicable, receive an easy solution on this hypothesis; how, for instance, it accounts for the mingling of northern and southern forms of mammals in the palæolithic beds; and how it gives a reason for the fact that palæolithic river-gravels are confined to those parts of Britain which were not covered by the ice-sheet, while the palæolithic deposits found in caves are not so restricted.

The hypothesis therefore stands on a firm basis, and the conclusion is irresistible that Palæolithic man was of interglacial—may be of preglacial—date. Thus much had been dimly felt rather than demonstrated by previous thinkers; but Mr. J. Geikie has shed a flood of light on the subject by pointing out that man was driven out of our country by the great submergence; that Britain was not again peopled till the elevation that followed connected it with the continent; and that the colonists belonged to the Neolithic race. In this way he has satisfactorily accounted for the great gap that exists between the two divisions of the Stone-folk.

The reasonable limits of an article are well-nigh reached, but we have by no means exhausted the contents of this comprehensive volume. The chapter on lakes must not be passed by altogether, for besides being a lucid exposition of Prof. Ramsay's theory of the formation of rock-basins, it is illustrated by an admirable map and section of Loch Lomond, and by a beautiful chart of part of the western coast of Scotland, which shows that these hollows are not confined to the land, but are also dotted over the shallow bed of the adjoining sea in exactly the places where a glacialist would expect to find them. The chapter on the English Drift would itself furnish materials for a review, as would also the note distinguishing the formations which are considered to have yielded traces of ice action. On the latter head we may point out that the presence of glaciers or icebergs is not in itself proof of a glacial epoch. Where we find, as in the Permian beds, evidence of the presence of ice at localities so far apart as Ireland, the west of England, and the centre of Germany, it looks like an indication of wide-spread severity of climate; but such a case as the Brecciated Beds of the Ord is better explained by a local development of glaciers, specially as the fauna of the associated strata forbids the existence of a general low temperature. It is worthy of note that these periods, which give the most satisfactory indications of glacial conditions, come close upon others, when a genial climate prevailed far up into northern latitudes; the Permian, for instance, followed hard upon the Carboniferous, and the Miocene epoch, if the glacial character of portions of it be fairly established, would yield a still more striking instance. But these juxtapositions of strongly-contrasted phases of climate, so far from being matter for surprise, are a necessary result of Mr. Croll's theory, according to which each hemisphere would, during a period of high eccentricity, experience alternately the severity of a glacial epoch and eras of almost perpetual spring.

Space will allow us to point out one only of the numerous results which will probably follow from the conclusions of this work. They must lead to a revision of our nomenclature of the Tertiary strata. The conditions of the Pliocene epoch were merely the commencement of a series of changes which received their full development during the Glacial era; and the latter is linked on by an equally unbroken succession of events with modern days. If therefore we are to have a Post-tertiary, Quaternary, or Recent Period, it should on physical grounds include Pliocene times; while the continental character of the Miocene epoch in Europe, and the important events that brought it to an end, mark it out as the natural termination of the Tertiary era.

In conclusion we have only to express a hope that the imperfect sketch we have given of the Great Ice Age may lead many readers to arrive at a fuller appreciation of its merits by turning to the work itself.

A. H. GREEN

SCHWEINFURTH'S "HEART OF AFRICA"

The Heart of Africa; or, Three Years' Travels and Adventures in the Unexplored Regions of the Centre of Africa. By Dr. Georg Schweinfurth. Translated by Ellen E. Frewer. 2 vols. (London: Sampson Low and Co., 1874.)

THE "Heart of Africa" is a valuable contribution to African literature, and we lay down the last volume with regret. This regret is enhanced by the grievous disappointment all geographers must feel that a man so capable and so reliable as Dr. Schweinfurth should have limited his scientific acquirements to botany and natural history without having qualified himself as a traveller by the use of astronomical instruments.

When we first glance at the elaborate map of the author's travels, embracing an extraordinary series of curves, zigzags, and the like, until we reach his most southern limit, we are delighted with this apparently valuable addition to geography, and we feel a first impulse to congratulate Germany as an ally in Central African Exploration, but to our complete dismay after these ardent expectations we find ourselves actually without one astronomical observation.

As geographers, we really have a right to complain. If Dr. Schweinfurth had been an uneducated adventurer, or even a mere sportsman attracted to wild countries by a love of wandering, we should have regretted a barren geographical result after an arduous journey of three years. Dr. Schweinfurth is, on the contrary, a man of scientific education and a botanist—in addition to being an accomplished draughtsman. He is a man of cultivated tastes, and he evidently combines the qualities requisite for a traveller in wild countries. Why should he not have fitted himself prior to his voyage by a few months' study for the only practical and reliable work of a geographer or scientific traveller? In the absence of astronomical observations we can only regard his map as the author's *idea* of his journey. We have no compass bearings or any reference to such observations having been taken. We must therefore accept his map as simply a conscientious endeavour to introduce us to his wanderings; at the same time, geographically speaking, we can only allow that he has been wandering about in the "Heart of Africa." It is with regret, therefore, that we cannot accept him in the first rank of geographers. A future traveller over the same ground may contest every position; thus, instead of our author's journey having added to our geographical knowledge, it may simply add to those geographical strifes which are the inevitable results of un-scientific journeys.

Having, as a matter of duty, expressed this opinion upon a work otherwise most valuable, it is a pleasure to be able to grasp one geographical fact that is well established, and is independent of astronomical observations. This is the watershed towards the West which forms the boundary of the Nile Basin. The large flow of water discovered by Dr. Schweinfurth is passing towards the Atlantic. This at once disproves the theories laid down by Livingstone, but never accepted by geographers, that he rivers to the west of the Tanganika Lake flowed northward to the Nile. As Schweinfurth passed out of the Nile Basin in about 28° E. long., so also Livingstone arrived in a western watershed south of the equator in about the same meridian.

The botanical information collected by Dr. Schweinfurth is invaluable, and can only be estimated by a professional botanist. We envy the traveller in many of his floral rambles, which are described with the energy and vividness of an enthusiast. Nothing new has been added to the known list of African fauna. We conclude, from the description of the habits of the so-called "rock rabbit," that our author means the "hyrax," which, although resembling a rabbit in appearance, is not a rodent.

Dr. Schweinfurth having been properly supported by an introduction from the Berlin Academy was saved many difficulties to which other travellers have been subjected; he was well received by Djiaffer Pacha, the Governor-General of Soudan, at Khartoum, who handed him over to the care of one Ghattas, a Coptic slave trader and ivory merchant. Ghattas entrusted him to the guidance of his own people, who appear to have behaved extremely well. Dr. Schweinfurth had every opportunity of examining the mysteries of the slave trade, and he is perfectly right in his description of the immense importance of the Darfur and Kordofan route, by which vast multitudes are conveyed who can thus elude the cruisers on the White Nile. At the same time the author is in error and has been purposely deceived by his informants (themselves slave traders) when (p. 429, vol. ii.), speaking of the upper district of the White Nile, inclusive of the Albert and Victoria Lakes, as one of the territories that form the sources of the slave trade in north-eastern Africa, he says, "The expedition of Sir Samuel Baker has stopped this source. The annual produce in the most favourable years did not exceed 1,000." There were no less than ten slave stations situated in the territory under Sir Samuel Baker's command. In each of these stations were at least 1,000 slaves.

The last act of Sir Samuel Baker, on his homeward route, was to overtake three vessels from the Bohr, lat. 5°20 N., with 700 slaves on board, which were openly on their route to pass the Government station of Fashoda! thus proving what Dr. Schweinfurth himself states respecting the connivance of the Egyptian officials, p. 442, vol. ii.—"In Kordofan, where there is a resident Egyptian Governor, the trade is truly enormous, and there is now as well the slave-trade from Darfur." In a cursory review of the slave-trade Dr. Schweinfurth makes a remark that few Englishmen would sanction, p. 433, vol. ii.—"Two great nations have speeded on the work, England in theory, North America in practice." If the payment of twenty millions sterling for emancipation was not the most practical, and not only theoretical, work, we really do not understand what practice means.

It would have been interesting had Dr. Schweinfurth given us more details of the ivory trade carried on by the people who acted as his chaperons in Africa. These were avowedly slave traders, and we should be gratified to learn that they formed some exception to the rule, and actually traded with merchandise instead of bartering slaves and stolen cattle for ivory.

The reward of ignorant ages to the returned traveller was general incredulity. Even in the present day there are ignorant persons who question the existence of cannibalism. Dr. Schweinfurth has arrived fresh from the cannibals of Monbuttoo with human skulls and bones almost warm from the saucepans of the savages. He can even describe the sauces which these gourmands use in their dainty dishes. Mushrooms and capsicums for a "sauce piquante aux champignons" are the literal civilised adjuncts for a dish off a stewed baby, only two days old, whose mother had deserted it! The baby was dying while the preparations for cooking it were already commenced. This is the real truth and no traveller's joke, as the babies and fond mothers would quickly discover should they visit the tribe of Monbuttoo. It may be asked, "How did Dr. Schweinfurth escape?" but it must be remembered that the Monbuttoo do not eat men of science, who are generally very lean. A fat missionary, with a family fresh from Exeter Hall, may meet with immediate attention, with the warm but brief Monbuttoo invitation, "walk in."

It would be useless for us to closely criticise this book. Few books are perfect. There may be a little excess of detail of the dull routine of African daily life that if omitted would have reduced two bulky volumes to a more convenient size. But on the other hand, some people like bulky volumes and enjoy as many pounds avoirdupois as they can obtain for their money; just as some people, especially the rural population, enjoy long sermons.

We cordially recommend all interested in exploration to read the book, at the same time reminding them that they may safely rely upon the high character and status of the author; for although Dr. Schweinfurth fails as a scientific geographer, he in no way fails as a scientific explorer devoted to the particular object of his studies—botany. In this branch of science he is better qualified than any former African traveller.

Such men as Dr. Schweinfurth will always have the regard and esteem of all true friends of Science; he belongs to the same metal that has already formed a wedge which will force open the secrets of inner Africa.

OUR BOOK SHELF

Adulterations of Food, with short Processes for their Detection. By Rowland J. Atcherly, Ph.D., F.C.S. (London: W. Isbister & Co., 56, Ludgate Hill, 1874.)

THE attempt to notice the adulterations of food in 100 pages of large type is a somewhat rash one, and it is not therefore surprising that the author of the treatise is frequently compelled to dismiss his subject in a very cursory manner.

For two of the classes of readers whom he addresses, the dealer and consumer, the work will no doubt be of use, and it is also likely to be useful to the chemist, as affording him a brief conspectus of the most likely adulterants in any particular article. Of what use, however, the last 12 pages of letterpress describing the making and use of volumetric solution are to the "trained chemist," to whom the author addresses them, we are at a loss to conceive.

The information given in the part upon adulterations is generally sound, though the statement on p. 34 that prussic acid is found when nitro-benzol has been used as a flavouring is absurd; so far is this from being the case, that it would be an indication of the use of a genuine but insufficiently purified oil of bitter almonds. The process for detecting alum in bread on p. 15 is also very unsatisfactory, and certainly not adapted for the use of either dealer or consumer. The book concludes with 21 neatly executed cuts of various starches, chicory, cocoa, tea-leaves and adulterating leaves found in tea, &c., as seen under the microscope. In conclusion, we would advise the author in a future edition to considerably expand the part on adulteration and to entirely omit the part intended for the "trained chemist," leaving that person to obtain his information on volumetric solutions from the proper sources.

R. J. F.

An Easy Introduction to Chemistry. Edited by the Rev. Arthur Rigg, M.A., late Principal of the College, Chester. (Rivingtons: London, Oxford, and Cambridge, 1873.)

THE present work, founded, as the editor states, on a "First Book of Chemistry," by Dr. Worthington Hooker, published in America, is intended for the use of children. Mr. Rigg calls attention to the inquiries of "young persons" as generally suggested by their observations of things touched and handled, and states that his aim has been "To supply information in a form which it is hoped may be intelligible and interesting to all parties concerned in thus learning to read the ever open book of nature."

The intention is a worthy one, and we have no doubt that the work will serve its purpose in instructing some of its readers, though we doubt if it will prove very intelligible for "persons" so young as those to whom the style of its commencement would seem to prescribe its use. We do not say this with any desire to find fault, for it would indeed be difficult to place the information in a simpler form than has been done, but because of the great difficulty of convincing young minds of the alterability of matter. Either talking or reading alone is quite incompetent to do this. Without experimental illustration they are utterly meaningless except to well-advanced intellects, and even there cannot do much, as anyone can tell who has had the honour of meeting the chemist whose knowledge extends not beyond books. In fact, chemistry is not to be taught without the laboratory and its experiments, and Mr. Rigg has shown his sense of their importance by the insertion of 46 beautifully-executed woodcuts of experiments and a frontispiece of a laboratory with its apparatus and fittings.

Excepting in a school, however, the "young persons" of the preface are not likely to meet with the actual experiments of which illustrations are supplied, and those that are of sufficient age to go to such a school might surely have a rather more advanced book placed in their hands. The question, however, which a reviewer ought to ask himself is, Is the book such a one as would fairly carry out the author's intention? and to this we must, in this case, answer "Yes." Granting the possibility of teaching chemistry to young children, Mr. Rigg's book would certainly serve its purpose well. With regard to his facts, Mr. Rigg is, as a rule, sound; but we must demur to his statement on p. 134, that "If (silica) is to these (grasses and grain) and other plants very much what bones are to animals;" and again, on p. 167, "Every stalk of grain or grass is chiefly wood. In both cases fine particles of flint are scattered in the wood to make it firm enough to stand even in a gale of wind." The experiments of Sachs and others have long since disproved this theory. Such blemishes as these are, however, of but little moment when the main principles of the science are the object of teaching, and on these Mr. Rigg is perfectly orthodox. We must, in conclusion, compliment the publishers on the very elegant get-up of the book.

Die Rohstoffe des Pflanzenreiches: Versuch einer technischen Rohstofflehre des Pflanzenreiches. Von Dr. Julius Wiesner. (Leipzig: Engelmann, 1873. London: Williams and Norgate.)

THIS is one of those elaborate German works which seem as if they were intended completely to exhaust the subject of which they treat. Every substance of economical or technical importance which is obtained from the vegetable kingdom is treated of in detail from the point of view of its practical utility rather than its physiological history; its chemical, mechanical, and microscopical properties, the mode of its preparation or manufacture, and its utility in the arts or commerce, are described. The book is, in fact, a repertorium of technical botany.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

On a Proposed Statistical Scale

AT a lecture last Friday evening, at the Royal Institution, I spoke on a subject which happens to lie at the meeting-point of many special sciences, and therefore, as I am desirous of having it well discussed, and from many points of view, it seems to me best to state it afresh in your columns for that purpose. It refers to the *definition* of the estimated *degree of development* of any quality whatever, without reference to external standards of measurement. The scale I propose depends on two processes; the one is securely based on the law of statistical constancy, the other is doubtfully based on the law of frequency of error. (1) At present we are accustomed to deal with averages and the like, which can only be obtained by measuring *every* individual by a *detached standard scale*, and going through an arithmetical process afterwards. Now I want to deal with cases for which no external standard exists, and I propose to proceed in quite another way, on the principle that *intercomparison* suffices to define. We have only to range our group in a long series, beginning with the biggest and ending with the smallest; and then we know by the law of statistical constancy that the individual who occupies the half-way point, or any other fractional position of the entire length, will be of the same size as the individual who occupies a similar position in any other statistical group of similar objects. We state his size with statistical precision by saying that his place is so and so in a series. We appeal to a standard which lies dormant in every group, and which a statistician can evoke, for temporary purposes of comparison, whenever he will. (2) What places in the series shall we select for our graduations? Equal fractions of its length will never do—I mean such as one-tenth, two-tenths, &c. —because of the great inequality of the variation in different parts of the series, being insensible between those whose position is near its middle and great between those at either end. I propose to use a scale founded on the law of Frequency of Error, which gives a scale of equal parts wherever that law applies, and I use the "probable error" for the unit of the scale. Thus, in a row of a hundred individuals the graduations of + 2°, + 1°, 0°, − 1°, − 2°, respectively would be at the following places, in percentages of the length of the series:—2, 9, 25, 50, 75, 91, 98. We know that the law of Frequency of Error applies very closely to the linear measurements of the human form. Now suppose that I want to get the average height and "probable error" of a crowd of savages. Measuring them individually is out of the question; but it is not difficult to range them—roughly for the most part, but more carefully near the middle and one of the quarter points of the series. Then I pick out two men, and two only—the one as near the middle as may be, and the other near the quarter point, and I measure them at leisure. The height of the first man is the average of the whole series, and the difference between him and the other man gives the probable error. The question I put is, whether any more convenient subdivision of a series can be suggested for *universal use* than that above mentioned. Its merits are, that it applies very fitly to linear measurements of all natural groups; also to errors of observation, which are akin to many of the moral qualities, for the measurement of which the scale is especially needed. It would not apply to weight, but is less out of relation to it than most persons might think, because weights do *not* vary as the cubes of the heights. Tall men are often thin, and short ones are fat, and the curious fact seems thoroughly verified that the general relation between height and weight is *strictly* as the *squares*. (See Gould's "Sanitary Memoirs of the War of the Rebellion," Cambridge, U.S., 1869, p. 408—410.) If we arrange a series and graduate it according to equal differences of the squares of the heights of the men, we are not so far astray as if we had dealt with the cubes. But I cannot imagine any quality, unless possibly music and memory, to vary so rapidly towards the large end of the series as the latter division would show. To sum up: subdivision in *equal* parts is of no use practically, and is therefore out of the question; the law of error will do very accurately for many large groups of cases; the law of error modified by being brought into relation to bulk will rarely, if ever, be right for other qualities. It therefore seems to me reasonable to adopt the law of error series, as the best compromise, and to accept it as "the common statistical scale." If, for example, I estimate a soldier's energy at + 2° (S.S.), I state what everybody who cared to inquire into the subject would construe in exactly the same sense as I used the phrase, and he would also be inclined to believe, until better informed, that the difference between such a man's energy and that of a man of + 0° (S.S.) was twice as great as between him and a man of + 1° (S.S.).

Lastly, how can we best find individuals who represent the 0°, ± 1°, &c., of any and every quality, that they may be studied and their abilities illustrated and described, so as to serve as permanent standards of reference? These would gradually give us means of finding the equivalent of the S.S. graduation in the natural scale—as we might learn to say, + 4° (S.S.) of energy = + 3°5 in the natural scale. Those who have to deal with bodies of men, whether as examiners, instructors, masters, overseers, or officers, could best tell. How about the ordinary subjects of competitive examination? Is there any optical observation made under (sensibly) identical circumstances and with (sensibly) identical instruments, of which the probable error of each observer is known? If one could only get two or three hundred nautical observers together, and make them take sextant angles of the same objects, and learn the probable errors of each, we should have data to give us once for all the values of the S.S. as regards ability to observe, in terms of absolute values. Can no drawing-master give accurate descriptions of the delicacy of touch of his pupils, corresponding to the graduations of the S.S. scale? How about mechanical manipulation among operatives? How about music and memory? Each separate quality requires and deserves a monograph, which, once thoroughly well done, would become a most valuable standard of comparison and check upon the S S. scale, which it must be remembered is securely based on no ground except that of statistical constancy, but which, when it *proves* to be a scale of equal parts, is doubly acceptable.

I will not go on writing now, being rather desirous of raising discussion and learning more, than of saying all my say.

42, Rutland Gate, S.W. FRANCIS GALTON

Simultaneous Meteorological Observations

WITH reference to the scheme of international simultaneous observations proposed by the War Department of the United States and adopted by the Meteorological Congress at Vienna in September last, a provisional arrangement was entered into at Vienna, between General Myer and myself, at his desire, by which the Scottish Meteorological Society was to assist the American Government in carrying out the proposed scheme by an exchange of meteorological observations between the two bodies. At a meeting of the Council of this Society on February 9, a letter was read from General Myer, dated January 27, 1874, formally requesting the co-operation of this Society in carrying out the international scheme, which letter being identical with the one on the same subject published in NATURE (vol. ix. p. 300), it is unnecessary to subjoin.

A considerable number of observers have been already obtained in connection with the scheme, and copies of the American *Monthly Weather Review* and *Daily Meteorological Record* have, along with the special schedules for the observations, been sent to them, as an acknowledgment on the part of the American Government for their assistance in the work. The Council are ready to receive the assistance of others of their own observers, and of any other observers who may be willing to co-operate in this cosmopolitan scheme, from which cosmopolitan benefits may be confidently looked for.

ALEXANDER BUCHAN

Scottish Meteorological Society, Edinburgh, March 2

The Limits of the Gulf-stream

MUCH discussion has recently taken place respecting the limits of the Gulf-stream, and the Admiralty Chart of the North Atlantic, published last year, is supposed to embody all that is known of its boundaries. My observations, however, which have extended over a series of years, differ so widely from it that I am induced to send you an abstract chart of them.

In December 1872 I found the stream wedged in to a distance of fifteen miles off Cape Hatteras, and following the coast-line at that distance to Roanoke Sound. On arriving in Norfolk I found that the reports of several ships corroborated my observations.

The remarkable bend east of George's Shoals is confirmed by H.M.S. *Gannet*, and also by the Nantucket fishermen and pilots.

Maury, in his "Physical Geography of the Sea," makes the stream, in summer, wash the southern shores of Newfoundland, but in no month of the year have I found it so far north as the red line in the accompanying chart. I am of opinion that if it once passed over the bank every codfish would be destroyed. The highest temperature recorded by me in September on this line is 56°.

At the points of sudden change I have seen the ripples at the distance of a mile previous to entering them. Those which are recorded may be relied on to a mile, as I have discarded those made from dead reckoning. In every case the deep blue colour of the sea, the presence of sun-fish, Portuguese men-of-war, and numerous *débris*, confirmed the observations made with the thermometer, and I may add, what is of more importance to seamen, the strong easterly set.

The southern boundary of the stream is taken from the observations of five years. As summer advances it becomes more difficult, when east of Bermuda, to detect the line of demarcation, for the rays of the sun heat the water almost to Gulf-stream temperature right down to the limit of the trade-wind. From the data which I have been able to collect, as well as from personal observation, the limits of icebergs in the Admiralty Chart appear to be equally erroneous. To me it appears impossible that bergs could drift square across the heated waters of the Gulf-stream to lat. 39° N. almost in the teeth of the prevailing summer winds, and a strong north-easterly set of two miles per hour. The Admiralty Chart gives the current a higher velocity.

The most southern iceberg ever seen by a Cunard steamer (and there cannot be a higher authority) was in lat. 43° 10′ N., long. 49° 40′ W., and the most eastern, which has come under my observation, by the *Grace Gibson*, on June 11, 1868, which ship passed four between lat. 43° 15′ N. and 43° 20′ N. and long. 41° 20′

W. to 42° 10′ W. It certainly must appear singular to geographers that the limits of the best-known stream in the world should be so ill-defined; but the temperature of the sea at the places marked in the chart cannot suddenly change 12° from any other cause than the irruption of the Gulf-stream or the ordinary waters of the ocean. Had it occurred in a single season only, the correctness of the observations might have been impugned; but extending, as they do, over several years, their accuracy cannot be challenged.

It is the opinion of many that the Gulf-stream is extending its boundaries northward, and ameliorating the climate of the British islands. Such an assumption is not an impossibility, although there are no changes of volume or velocity at its outlet into the Atlantic. There are, however, grounds for believing that the Labrador current does not run with its former force, as icebergs are seldom seen south of the parallel of 43° 30′ north latitude. Observation can alone confirm this theory, but whether correct or not it in nowise affects the accuracy of my data.

Wm. W. Kiddle

U.S. White Star Mail Steamship *Oceanic*, Feb. 2

[We have received a chart from Mr. Kiddle; but it is too large for insertion in Nature.]

A Lecture Experiment

Mr. Tait's letter in Nature of February 26 calls to mind an effective lecture illustration I have used in my classes to illustrate a fog or cloud produced by cooling air containing moisture. Instead of using an air-pump as described in "Heat, a mode of Motion," take a flask of one or two litres capacity, rinse it out with distilled water, and attach to the neck a cork and glass tube of about twenty or thirty centimetres in length. Place the glass tube in the mouth and exhaust, when a dense cloud will be formed; then on blowing into the flask the cloud disappears. The cloud may be produced and dissolved as often as wished, and if a beam from the oxy-hydrogen light be sent through the flask, the experiment becomes very effective.

Midland Institute, Birmingham C. J. Woodward

The "Treasury of Botany"

It might be inferred from your notice of the new edition of the "Treasury of Botany" (Nature, vol. ix. p. 300) that the stereotyped pages of the original text of that work—of which you are pleased to speak in terms of commendation—had been reprinted without alteration. Will you allow me space to state that this is by no means the case (as indeed is stated in the preface), but that a large number of corrections have been made, as may be detected by a keen eye in consequence of the slight difference which is observable in the type where the alteration has extended over two or three lines or more. Hence it is not to the Supplement alone that the reader must look for such of the "additions to botanical knowledge made during the last eight years" as it has been found practicable to include in the revised edition.

Thos. Moore

[We are glad of the opportunity afforded by the foregoing letter of repeating our opinion, already expressed, that in the department of botanical nomenclature and classification, the new edition of the "Treasury of Botany" is an altogether admirable and indispensable work. It is in this department only, or almost exclusively, that the corrections alluded to by Mr. Moore—and to which we perhaps ought to have called special attention—have been made, at least as far as we have been able to detect. We regret that we cannot withdraw from our statement that the same care has not been taken with the histological and physiological section. We might quote a number of instances in support of this assertion—a very ungracious task in speaking of a work so excellent in other respects—but will only refer to a single one. Notwithstanding that a very good and useful epitome of the more important properties of "Cellulose" is given in the Supplement, the statement is allowed to stand in the article in the body of the work, that "its composition, according to the latest analysis, is $C_{24}H_{20}O_{10}$" a formula which does not, and never did, even under the old notation, represent anything near its composition. —A. W. B.]

The Moons of Uranus

In your "Notes," this week, it is stated that since Mr. Lassell's observations at Malta, no one has seen the four moons of Uranus, until the re-discovery of the two small ones lately with the new Washington telescope.

In 1869-70, the planet was observed with the Melbourne reflector; the observations were specially directed to the disc, but at the same time the positions of the four satellites were noted on successive nights and thus identified.

I speak from memory, but have no doubt that the observations are to be found in the Melbourne records. The statement "have actually been measured by Prof. Newcomb," probably refers to position, angle, and distance.

March 1 L. S.

MEN OF SCIENCE, THEIR NATURE AND THEIR NURTURE*

The lecturer spoke of the qualities by which the English men of science of the present day were characterised; he showed the possibility of defining and measuring the amount of any of those qualities, and concluded by summarising the opinions of the scientific men on the merits and demerits of their own education, and gave his interpretation of what, according to their own showing, they would have preferred. His data were obtained from a large collection of autobiographical notes, most obligingly communicated to him, in response to his requests, from the larger part of the leading members of the scientific world. He had addressed 180, who, being Fellows of the Royal Society, had, in addition, gained medals or filled posts of recognised scientific position; 115 answers had already been received, of which 80 or 90 were full and minute replies to his long and varied series of questions. He dealt with only a small part of his deductions from this valuable material, referring to a forthcoming work for the rest.

Regarding the chief qualities in the order of their prevalence among the scientific men, they were—(1) Energy both of body and mind; (2) Good health; (3) Great independence of character; (4) Tenacity of purpose; (5) Practical business habits; and (6) What was usually the salt of the whole, strong innate tastes for science generally or some branch of it. He illustrated his remarks by reading many anonymous extracts from the returns, and explained in what way a notable deficiency in any of the above-mentioned qualities would tend to disqualify a man from succeeding in science.

As to the measurement of qualities, it was argued that the law of constancy in vital statistics might be taken for granted, being evidenced by the experience of insurance offices against fire, death, shipwreck, and other contingencies, always with the proviso that the facts are gathered with discretion, on well-known general principles. Hence we may say with assurance, that although two common nuts may differ, yet the contents of different packets, each containing 1,000 nuts, will be scarcely distinguishable, for the same number of nuts of different sizes will be found in each. Let the contents of the several packets be each arranged in a long row, in order of size, beginning with the biggest nut and ending with the smallest, and place the rows rank behind rank; then by the law of statistical constancy the nuts in the same *files* will in all cases be closely alike (except the outside ones, where more irregularity prevails). Again, if we incorporate two rows into one of double length, still preserving the arrangement as to regular gradation in size, the centre nuts of the two original series will still be found at or near the centre of the compound series, the nuts in quarter positions will still be in quarter positions, and so on. Hence, whatever be the length of the series the *relative* position in it of the nut will be a *strict criterion* of its size. This is of course equally true of all groups of qualities or characters whatever, in which the law of statistical constancy prevails, the series, in each case, being arranged according to gradations of the quality in question. Each individual is measured against his neighbour, and it is quite unnecessary to have recourse to any external standard. As regards a scale of equal parts, the lecturer made use of a converse application of the law of "frequency of error," which he illustrated by many experiments, and which showed that in a row (say as before) of nuts, if we took those which occupied the three quarterly divisions (1st quarter, centre, 3rd quarter) as the three elementary graduations of size, a range of successive graduations would be obtained by the following series, in which the places of the nuts are supposed to be reckoned from the end of the row where the large nuts are situated, and to be given in per-thousandths of the entire length of the row. It might be called the "Common Statistical Scale" (S. S.). The place of + 4° would be at 4 thousandths from large end; + 3°, at 21 thousandths; + 2° at 89; + 1, at 250; 0° at 500; − 1° at 750; − 2° at 911; − 3° at 979; and − 4° at 996, or 4 thousandths from the small end of the row. Thus if we say that the size of a nut is + 2° S. S., we absolutely define it. Anybody can procure such a nut independently by getting a quart of nuts and arranging them. Also we know that the difference between a nut of + 4° S. S. and + 1° S.S. is 3°, and therefore three times as great as between one of + 2° S. S. and the latter. It cannot be affirmed that this is a precise scale of equal parts for all qualities, but it is found to hold surprisingly well in a great variety of vital statistics; perhaps, too, the mere thickness of tissues may be a chief element in the physical basis of life. This scale appears, at all events, more likely to be nearly approximative to one of equal parts, for qualities generally, than any other that can be specified, and it certainly affords definite standards subject to the law of statistical constancy. The habit should therefore be encouraged in biographies, of giving copious illustrations which tend to rank a man among his contemporaries, in respect to every quality that is discussed, in order to give data for appraising those qualities in terms of the Statistical Scale. By the general use of a system of measurement like the above, social and political science would be greatly raised in precision.

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* Lecture on Friday evening, Feb. 27, at the Royal Institution, by Francis Galton, F.R.S.

Regarding education, the lecturer disavowed speaking of what might be suitable for boys generally, but he summarised the replies of the scientific men with reference to their own special experience, and notwithstanding the diversity of branches of science, he found unanimity in their replies. They commonly expressed a hatred of grammar and classics, the old-fashioned system of education being utterly distasteful to them. The following seems the programme they themselves would have most liked:—1. Mathematics, rigorously taught up to their capacity, and copiously illustrated and applied, so as to throw as much interest into its pursuit as possible. 2. Logic. 3. Some branch of science (observation, theory, and experiment), some boys taking one branch and some another, to insure variety of interests under the same roof. 4. Accurate drawing of objects connected with that branch of science. 5. Mechanical handiwork. All these to be rigorously taught. The following not to be taught rigorously: reading good books (not trashy ones) in literature, history, and art. A moderate knowledge of the more useful languages taught in the easiest way, probably by going abroad in vacations. It is abundantly evident that the leading men of science have not been made by much or regular teaching. They craved for variety. Those who had it, praised it; and those who had it not, concurred in regretting it. There were none who had the old-fashioned high-and-dry education who were satisfied with it. Those who came from the greater schools usually did nothing there, and have abused the system heartily.

INFLUENCE OF GEOLOGICAL CHANGES ON THE EARTH'S ROTATION

At the annual meeting of the Geological Society of Glasgow, on Feb. 12, the president, Sir William Thomson, F.R.S., gave an address on the above subject, of which the following is an abstract:—

He first briefly considered the rotation of rigid bodies in general, defining a principal axis of rotation as one for which the centrifugal forces balance while the body rotates around it. He then took the case of the earth; and, having pointed out the position of its present axis, showed that if from any cause it were made to revolve round any other, that would be an "instantaneous axis," changing every instant, and travelling through the solid, from west to east, in a period of 296 days round the principal axis It would shift continually in the figure, owing to the varying centrifugal force of two opposite portions of the body. This would produce, by centrifugal force, a tide of peculiar distribution over the ocean, having 296 days for period. An inclination of the axis of instantaneous rotation to principal axis of 1″, or 100 ft. at the earth's surface, would produce rise and fall of water in 45° latitude, where the effect is greatest, amounting to ·17 of a foot above and below mean level.

He noticed, in passing, the application of these dynamical principles to the attraction which the sun and moon exercise on the protuberant parts of the earth, tending to bring the plane of the earth's equator into coincidence with the ecliptic. This causes an incessant change, to a certain limited extent, in the position of the axis of rotation, thereby occasioning what is known as the "precession of the equinoxes." Having illustrated these remarks by some interesting experiments, Sir William Thomson proceeded to consider more particularly the circumstances according to which the axis of the earth might become changed through geological influences, and the consequences of any such change. The possibility of such a change had been adduced to account for the great differences in climate which can be shown to have obtained at different periods in the same portion of the earth's surface. In the British Isles, for example, and in many other countries, there is clear evidence that at a comparatively recent period a very cold climate—much colder than at present—prevailed; while in the same places the remains of plants and animals belonging to several preceding eras indicate a high temperature and a comparatively tropical climate. The question arose, can changes in the earth's axis account for these changes of climate? In the present condition of the earth, any change in the axis of rotation could not be permanent, because the instantaneous axis would travel round the principal axis of the solid in a period of 296 days, as already stated. Maxwell had pointed out that this shifting of the instantaneous axis in the solid would constitute in its period a periodic variation everywhere of "latitude," ranging above and below the mean value, to an extent equal to the angular deviation of the instantaneous axis of rotation from the principal axis; and, by comparing observations of the altitude of the Pole-star during three years at Greenwich, had concluded that there may possibly be as much as $\frac{1}{2}''$ of such deviation, but not more.

In very early geologic ages, if we suppose the earth to have been plastic, the yielding of the surface might have made the new axis a principal axis. But certain it is that the earth at present is so rigid that no such change is possible. The precession of the equinoxes shows that the earth at present moves as a rigid body; and during the whole period of geologic history, or while it has been inhabited by plants and animals, it has been practically rigid. Changes of climate, then, have not been produced by changes of the axis of the earth. The learned professor then inquired what influences great subsidences or great elevations in different parts of the earth might have on the axis of rotation. No doubt the removal of a large quantity of solid matter from one part of the globe to another would sensibly alter the principal axis, as well as the axis of rotation, which so nearly coincides with it; but it could be shown that it would produce in the latter only about 1-300th part of the change produced in the former. We know too little of the changes in the interior of the earth accompanying such changes on its surface to be able to state results with certainty. But he estimated that an elevation, for example, of 600 feet on a tract of the earth's surface 1,000 miles square and 10 miles in thickness, would only alter the position of the principal axis by *one-third of a second*, or 34 feet. He called attention to the effect of tidal friction and subterranean viscosity in reducing any such deviation, and pointed out that it must be exceedingly slow; using for evidence the observationally proved slowness of the diminution of the earth's rotational velocity, and of the inclination of its equator to the ecliptic. It therefore seemed probable that geological changes had not produced any perceptible change in the principal axis or in the axis of rotation within the period of geological history.

OBSERVATIONS OF MAXIMUM AND MINIMUM SEA-TEMPERATURES BY CONTINUOUS IMMERSION

WHEN the Scotch Meteorological Society was instituted, now nearly twenty years ago, observations on sea-temperature were set on foot at the suggestion of the late Prof. Fleming, and have since been continued. These observations were made by the immersion of thermometers with small cisterns attached, and were taken at the surface and at a depth of 6 feet. Besides these, special observations were made for me on the temperature of the flood and ebb tide at depths extending to 50 feet in the Pentland Frith,* and hourly observations continued at intervals during four years ending in 1863 by Capt. Thomas, R.N., at depths extending to 60 feet.† Such occasional observations seemed to me to be insufficient to show properly the changes in temperature to which the sea is subject, and in August 1872 I suggested to my friend, Prof. Wyville Thomson, the propriety of ascertaining, on his exploring voyage, maximum and minimum temperatures by means of thermometers constantly immersed in the sea. For this purpose a thin malleable iron plate of an oval shape, as shown in Fig. 1, is fixed to the outside skin of the ship so as to form a small cell into which the sea-water finds ready ingress through numerous perforations. This cell, which need not project more than two inches, so as not to cause any appreciable obstruction to the speed of the vessel, should extend so far under the smooth water level as to prevent its lower end from rising above the trough of the sea, or an upright pipe might be placed within the vessel. In sailing ships there might be a cell on each side so as to secure constant immersion while the ship "is on a wind." In this cell a frame carrying a maximum and minimum thermometer slides in checks so as to be capable of being raised above water to the level of the cabin or the deck, where there should be a porthole to admit of the instruments being read and the indices being re-adjusted.

An arrangement similar in principle to that described was made in the *Challenger* exploring vessel before she left on her voyage.

In this way, during the whole of an over-sea voyage, regular observations of maxima and minima may be obtained as often as may be desired. This arrangement is peculiarly suitable to floating lights, and the Scotch Meteorological Society have been in correspondence with the Mersey Board in order to establish observations at the North-West Lightship.

The Marquis of Tweeddale in 1872 proposed that the Scotch Meteorological Society should enter upon the investigation of the migrations of fishes, and particularly those of the herring, in connection with sea-temperatures and weather generally, and his Lordship informed me that in his opinion it was likely that the herrings followed belts of water of a higher temperature than that of the sea generally.

In carrying out his Lordship's suggestion the Society has been favoured through the courtesy of the Fishery Board with returns of the daily catch of herrings and of the weather from the different fishing districts of Scotland for the last two years; and already two elaborate reports on the subject have been drawn up by Mr. Buchan, the Secretary, and published, which give good ground to hope that some positive results of considerable importance will be obtained. With reference to this investigation, I suggested, for piers and harbours, the adoption of a cast-iron pipe for containing the thermometer as shown in Fig. 2, and application was accordingly made to the Trustees of Peterhead harbour, where observations by continuous immersion have been made by Mr. William Boyd, F.R.S.E., since May 1873. It is to be regretted that these observations have in the meantime been stopped, owing to a ship having come in contact with the pipe.

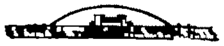

FIG. 1.

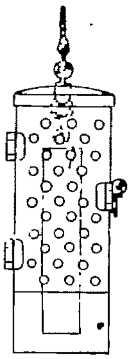

FIG. 3.

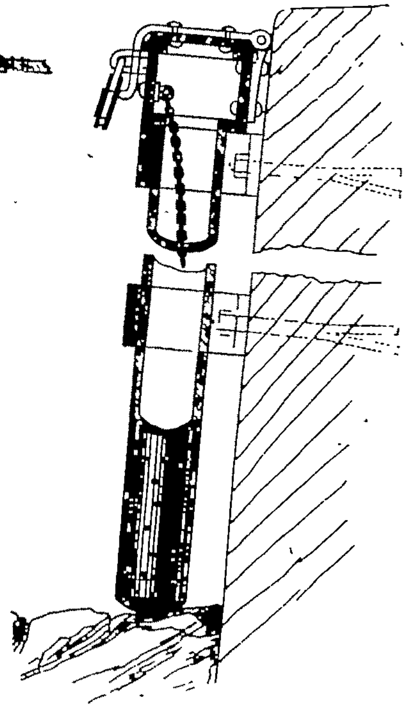

FIG. 2.

In addition to observations near the surface at floating lights, it would be extremely desirable to have thermometers immersed at greater depths, and for this purpose a copper vessel weighted below should be used, as represented in Fig. 3, with perforations in the upper part and a cistern about 4 in. deep in the lower part. The Scotch Meteorological Society, at its meeting on February 9 last, authorized an application to the different lighthouse authorities for sanctioning these deep-water observations as well as those of the surface and of the air.

THOMAS STEVENSON.

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* "Edin. Phil. Jour." Nov 25, 1857.

† "Jour. Scot. Met. Soc." vol 1, p. 156.

OZONE*

I.

TOWARDS the end of the last century, Van Marum, while experimenting with his powerful electrical machine, observed that oxygen gas through which electrical sparks had been passed acquired a peculiar odour and the property of attacking mercury. This subject attracted no further attention for upwards of half a century after the publication of Van Marum's observations.

The discovery of ozone was announced by Schonbein in a memoir which he presented in 1840 to the Academy of Munich. In this important communication he states that in the electrolysis of water, an odorous substance accompanies the oxygen evolved at the positive pole, that this substance may be preserved for a long time in well-closed vessels, and that its production is influenced by the nature of the metal which serves as the pole, by the chemical properties of the electrolytic fluid, and by the temperature of that fluid, as well as of the electrode. The same body he found to be produced by holding a strip of platinum or gold near the knob of the prime conductor of an electrical machine in good order. With great sagacity he recognised the identity of the peculiar odour which accompanies a flash of lightning with that of the new substance. In this memoir Schonbein supposes the odorous body, for which, in a note at the end, he proposes the name of ozone, to be a new electro-negative element belonging to the same class as chlorine and bromine; but in a paper published a little later he hints that ozone may be one of the constituents of nitrogen.

FIG. 2.

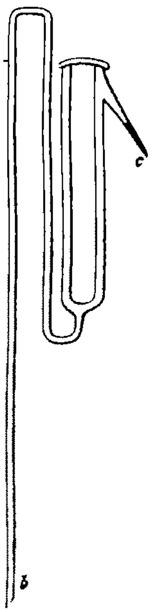
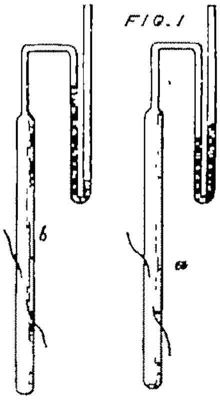


FIG. 1



Schönbein soon afterwards discovered that ozone is formed when phosphorus oxidises slowly in moist air or oxygen.

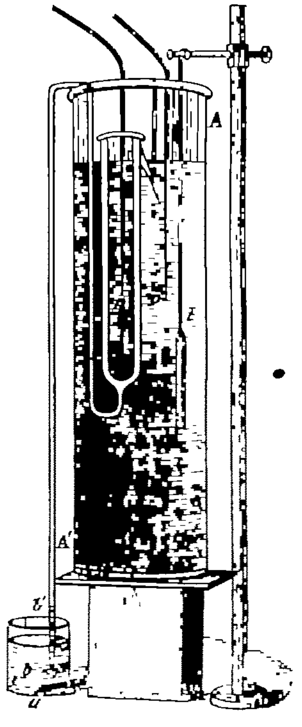
In the following year, he returned to the consideration of the subject, and partly from his own observations, partly from experiments communicated to him by De la Rive and Marignac, he abandoned his former view of the nature of ozone, and concluded that it is an oxide of hydrogen different from the peroxide of hydrogen of Thénard.

Many of the properties of ozone described by Schönbein were soon afterwards verified by Marignac, who found, as Schonbein had already stated, that it is only in the presence of moisture that air or oxygen when passed over phosphorus produces ozone, and that no ozone can be formed from air, even if moist, which has been deprived of its oxygen. He also confirmed the observations of Schonbein that the peculiar properties of ozone disappear when it is heated to a temperature between 300° C. and 400° C., and that it is not absorbed by water or sulphuric acid.

In a subsequent investigation (1845) which Marignac conducted with De la Rive, the important fact was established that ozone is formed by the passage of electrical sparks through pure and dry oxygen gas. Frémy and Becquerel also showed that pure oxygen contained in a tube inverted over a solution of iodide of potassium is entirely absorbed by that liquid, if electrical sparks are passed for a sufficiently long time through the gas.

The last hypothesis of Schonbein, according to which ozone is an oxide of hydrogen, was manifestly inconsistent with the production of that body by the passage of electrical sparks through pure and dry oxygen. On the other hand, it received support from some experimental inquiries which appeared about this time, and particularly from an elaborate investigation which was conducted by Baumert in the laboratory of the University of Heidelberg, and published in *Poggendorff's Annalen* for 1853. Baumert maintained that water is always formed when dry ozone, prepared by electrolysis, is destroyed or decomposed by heat, and further endeavoured to establish its composition by determining the increase of weight of a solution of iodide of potassium when it is decomposed by ozone. He inferred, as the result of his researches, that two distinct bodies had been confounded under the name of ozone; (1) allotropic oxygen, formed by the passage of the electrical spark through oxygen; and (2) a teroxide of hydrogen, produced in the electrolysis of water. The experiments and conclusions of Baumert attracted a great deal of attention at the time they were published, and received very general assent.

FIG. 3



Having repeated, soon after it was announced, the experiment of Baumert, in which ozone prepared by electrolysis was destroyed by heat, and having failed to obtain the slightest trace of water in numerous trials, I deemed it important to undertake a careful investigation of the subject, the results of which were communicated in 1853 to the Royal Society of London. By employing an acidulated solution of iodide of potassium, I found that its increase of weight, when decomposed by ozone, exactly agreed with the weight of the ozone calculated as allotropic oxygen from the iodine set free. The numbers deduced from five careful experiments were 0·1179 grammes for the increase in weight of the solution, and 0·1178 grammes for the calculated weight of the oxygen. As regards the supposed formation of water in the destruction of ozone by heat, it may be sufficient to mention the results of two experiments performed with great care, in one of which 6·8 litres of electrolytic oxygen containing

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* An Address delivered before the Royal Society of Edinburgh on December 22, 1873, by Dr. Andrews, LL.D., F.R.S., Honorary Fellow of the Royal Society of Edinburgh.

27 milligrammes of ozone, and in the other 9·6 litres of the same gas containing 38 milligrammes of ozone, were exposed to the action of heat, so as to destroy all ozone reactions, when not a trace of water was obtained; the increase in weight of the desiccating apparatus being in the first case only one-third, and in the second one-half, of a milligramme. If Baumert's experiments had been correct, 24 milligrammes of water should have been formed in these experiments. The general conclusions at which I arrived were: "that no *gaseous* compound, having the composition of a peroxide of hydrogen, is formed during the electrolysis of water, and that *ozone from whatever source derived is one and the same body, having identical properties and the same constitution, and is not a compound body, but oxygen in an altered or allotropic condition.*" ("Phil. Transactions" for 1856, p. 13.)

The next step in the investigation of this singular body was the discovery that oxygen gas in changing into ozone diminishes in volume, or becomes condensed, recovering its original volume when the ozone is changed back into oxygen by the action of heat or otherwise. This relation between ordinary oxygen and ozone was first announced in 1860 by Prof. Tait and myself in a communication to the Royal Society of London. Oxygen gas in a dry and pure state was introduced into a tube sealed at one end and terminating at the other in a fine tube bent as shown in Fig. 1, and containing a short column of sulphuric acid. Two platinum wires were hermetically sealed into the sides of the wide tube, the distance of the ends within the tube being about 20 millimetres.

When an electrical discharge without visible sparks was passed between the extremities of the platinum wires, the sulphuric acid rose in the adjacent leg of the U-tube, and from the change of level the amount of the condensation, or diminution of volume, which the oxygen had undergone was easily calculated. The apparatus was then hermetically sealed and the reservoir heated to 270° C., so as to destroy the ozone. After allowing the reservoir to cool, the sealed end of the U-tube was opened, when the original volume of the gas was found to be restored. Strong electrical sparks were found to give scarcely one-fourth of the contraction which occurred with the silent discharge, and if sparks were passed through the gas when fully contracted by the silent discharge, the contraction was reduced to that which the spark would have produced in the original gas. In the same paper it was shown that no further diminution of volume occurred when the contracted gas was agitated with a solution of iodide of potassium so as to absorb the ozone. A similar result was obtained on agitating the contracted gas with iodine. The ozone reactions in all these cases disappeared, but without any change in the volume of the gas. With mercury and silver, not only was there no contraction, but expansion actually occurred, which was explained on the assumption that the oxide at first formed exercised a catalytic action on part of the ozone and restored it to the state of ordinary oxygen. Similar results were obtained with electrolytic ozone. Three years later these experiments on the condensation of oxygen in changing into ozone, and on the action of ozone upon a solution of iodide of potassium were repeated and confirmed by Von Babo and by Von Babo and Claus.

We did not attempt to give any absolute explanation of these singular facts; but discussed them under different aspects. We showed that on the allotropic view of the constitution of ozone its density must be enormously great; unless it was assumed that "when ozone comes into contact with such substances as iodine, or a solution of iodide of potassium, one portion of it, retaining the gaseous form, is changed back into common oxygen, while the remainder enters into combination, and that these are so related to one another that the expansion due to the former is exactly equal to the contraction arising from the latter." On this assumption, which however we did not consider probable, we remarked that "our experiments may be reconciled with the allotropic view, and an ordinary density, but still one greater than that of oxygen." A similar explanation of our experiments but connected with a peculiar view of the molecular constitution of oxygen was proposed in 1861 by Dr. Odling. "If we consider," he remarks, "ozone to be a compound of oxygen with oxygen and the contraction to be consequent upon their combination, then if one portion of this combined or concentrated oxygen were absorbed by the reagent, the other portion would be set free, and by its liberation might expand to the volume of the whole; thus, if we suppose three volumes of oxygen to be condensed by their mutual combination into two volumes, then on absorbing one-third of this combined oxygen by mercury, the remaining two-thirds would be set free and consequently expand to their normal bulk, or two volumes—

$$\overset{-}{O}\,\overset{+}{O}\,\overset{-}{O} + \overset{+}{Hg^2} = \overset{+}{Hg^2}\overset{-}{O} + \overset{+}{O}\,\overset{-}{O}.\text{"}$$

Soret, experimenting in 1866 upon the mixture of oxygen and ozone obtained by electrolysis, made the important discovery, that if this mixture is brought into contact with oil of turpentine, or oil of cinnamon, a diminution of volume takes place, equal in amount to twice the augmentation of volume which the same mixture would sustain if the ozone were converted by heat into ordinary oxygen. In other words the volume of ozone, measured by its absorption by the essential oil, is twice as great as the difference between the volume of the same ozone and oxygen. Hence Soret concluded that the density of ozone is one and a half times that of oxygen gas.

The latest investigations on this subject are due to Meissner and Brodie. The former has fully confirmed my early experiments, according to which the increase in weight of an acid solution of iodide of potassium, when electrolytic ozone is passed through it, corresponds exactly to the weight of oxygen absorbed, as calculated from the liberated iodine. Meissner has also found, as I had long before stated, that when a neutral solution of iodide of potassium is employed, the results are variable and untrustworthy.

Brodie has examined the action of ozone on a variety of liquids, and has confirmed the results of Prof. Tait and myself that no diminution of volume occurs when ozone is removed from a mixture of ozone and oxygen by a solution of iodide of potassium. With other liquids he has obtained volumetric results which he considers to be definite and which differ from any previously observed. I am inclined to think that they are rather complex cases, involving the volumetrical changes already known in variable proportions. His experimental results, moreover, when examined in detail, do not appear to be sufficiently concordant to justify the sharp conclusions he has deduced from them.

Brodie has obtained for ozone prepared by the electrical discharge the same density (one and a half times that of oxygen) which Soret had previously obtained for ozone prepared by electrolysis. He considers a suggestion of Prof. Tait and myself, that oxygen may possibly be decomposed by the electrical discharge, not to be supported by the facts he has observed.

I will now give a brief statement of the methods of preparing ozone and of its leading properties.

Ozone may be obtained by the action of the electrical spark, or the glow or silent discharge on pure oxygen. With the silent discharge, as has been before stated, at least four times as large an amount of ozone is obtained as with the spark. As regards the actual amount of oxygen which, under the most favourable conditions can be converted into ozone, the highest recorded result was obtained in an experiment by Prof. Tait and myself, in which a contraction of one-twelfth of the original volume of the oxygen, or 8·3 per cent., occurred; but we were unable in other trials to produce again so great a diminution of volume. The greatest contraction attained in the experiments of Von Babo and Claus amounted to 5·74, and in those of Brodie to 6·52 per cent. The doubt which existed as to the accuracy of our solitary experiment I have lately been able to remove, and by a slight modification in the form of the apparatus I have succeeded in obtaining greater contractions than any hitherto recorded. In one of the first trials the diminution of volume amounted to more than 10 per cent., and there can be little doubt that with care even greater contractions than this may be attained.

As the method referred to enables the contraction of oxygen in changing into ozone to be exhibited as a class experiment, I will describe it in some detail. The excellent induction tube of Siemens, in which the electrical discharge from an induction coil acts upon air or oxygen, as it flows between two thin tubes of glass, whose surfaces are at a distance of a few millimetres from one another, has hitherto been employed to obtain a continuous stream of ozone in a more or less concentrated state. But this apparatus can easily be modified so as to show the contraction which takes place when oxygen is converted into ozone. Fig. 2 exhibits the modification I have given for this purpose to the ordinary form of Siemens' tube. At *c* it terminates in a capillary tube, the end of which is hermetically sealed, after a stream of pure and dry oxygen gas has been passed through the apparatus for a sufficient time to displace the air. In exact experiments the other end (*b*) is at the same time sealed and afterwards opened under sulphuric acid. For class purposes it will be found sufficient to immerse it quickly under the acid, contained in the beaker (*a*), as shown in Fig. 3, where the induction-tube is seen immersed to within 12 millimetres of its upper surface in water contained in an *insulated* cylindrical vessel (A A′). The inner cavity of the induction-tube is also filled with water to about the same level. By means of wires covered with caoutchouc, except at the lower ends (*p p′*), the discharge from an induction-coil, capable of giving 10 millimetre sparks in air, can be passed through the apparatus. The water in A A′ is maintained as steadily as possible at the temperature of the apartment, and any slight changes in the course of the experiment are noted by means of a delicate thermometer (*t*). The variations of the barometer are also carefully observed. In very exact experiments the surfaces of the induction-tube should be covered with tinfoil, and the cylindrical vessel filled with ice. Before commencing the observation, it will be found convenient, if the temperature has not already effected the adjustment, to expel a little oxygen from the induction-tube, so that the level of the acid may stand somewhere about *b′*. On passing the electrical discharge, the acid will at first be depressed a few millimetres, from the repulsive action of the particles of the electrified gas, but will afterwards steadily rise, and for some time with such rapidity that the ascent of the acid column can be easily followed by the eye. When the current is interrupted, a sudden rise of the acid column will occur equal to the depression which took place on first making connection with the induction coil, after which the new level of the acid may be read.

Another method of obtaining ozone is by the electrolysis of water and of certain acid and saline solutions. The most convenient liquid for this purpose is a mixture of one part of sulphuric acid with six or eight parts of water, and the lower the temperature at which the electrolyte is maintained during the process the greater is the amount of ozone. The simplest and most efficacious arrangement for obtaining ozone by this method is one I have used for many years and exhibited in my lectures. It consists of a bell-jar (Fig. 4, *a*), or glass cylindrical vessel, open below, and contracted to a neck above, which is suspended in a round cell (*b b′*) of porous earthenware, leaving a clear space of two inches between its lower edge and the bottom of the porous cell. The whole is placed in a glass jar (*c c′*) of somewhat larger dimensions than the cell; a bundle of platinum wires (*p*) suspended below the bell-jar serves as the positive pole, and a broad ribbon of platinum (*n n′*) placed between the outer glass jar and the porous cell as the negative pole of a voltaic arrangement of three or four couples. A delivery tube hermetically united to the neck of the bell-jar conveys the mixture of oxygen and ozone disengaged at the positive pole to a sulphuric acid drying tube (*d*). From the desiccating tube the gas passes through the connecting tube (*e*) and thence to other tubes, for the purpose of illustrating the properties of ozone. Thus, in the figure, it is represented as traversing a tube of hard glass (*f f′*) covered with fine wire gauze, and terminating near the surface of mercury contained in the flask (*h*). So long as the gas is heated strongly as it passes through the tubes (*f f′*) by the spirit lamps (*g g′*), not the slightest change is produced upon the mercury; but when the lamps are removed, and the tube allowed to cool, the mercury is rapidly attacked. I ought, perhaps, to mention that all the junctions are made with dry and tightly fitting corks, care being taken that the ends of the connecting tubes project a little beyond the corks. With these precautions the loss of ozone, from its action on the corks, is altogether insignificant.

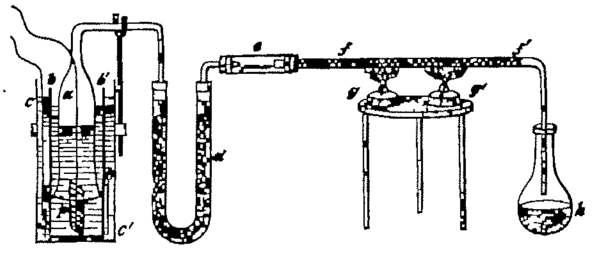

Fig. .

Ozone can also be obtained by the slow oxidation of phosphorus, and of certain ethers and essential oils in presence of moisture.

(*To be continued.*)

NOTES

ONE of the last and one of the best acts of the late Government was to grant a pension of 150*l.* a year on the Civil List to Prof. Sharpey. Dr. Sharpey has done as much as any living teacher for the advancement of physiological knowledge, while his personal worth has secured for him universal respect and esteem.

AT the last monthly meeting of the Russian Imperial Geographical Society M. Venioukoff, the secretary, before proceeding with the business of the evening, said the Society owed a duty which must first be fulfilled, and that was to render homage to the memory of Dr. Livingstone, the importance of whose discoveries and the perseverance of whose labours had placed him in the rank of the most remarkable travellers of all times and of all nations. His biography belonged to the annals of geographical science. M. Venioukoff then read a memoir of Livingstone, which concluded as follows:—"Let England, which may be proud of having given birth to Livingstone, and of having supported him in his labours, learn that among us the merit of her great men can be appreciated." The whole assembly, which was very large, then rose in order to pay a last tribute of respect to the memory of Dr. Livingstone.

THE University of St. Andrew's has conferred upon Mr. J. Gwyn Jeffreys, F.R.S., the honorary degree of LL.D.

WE would draw special attention to the programme, which has just been issued, of a new course of twelve lectures on Zoology, to be delivered during the ensuing spring, in the Zoological Gardens, Regent's Park; the Council of the Society having determined to appropriate the interest of a small bequest which they hold for scientific purposes—the Davis Fund—to the subject. Mr. P. L. Sclater, F.R.S., the Secretary to the Society, will deliver the Introductory Address on April 14; and he will follow it by four lectures *On the Geographical Distribution of Mammals*. After these Mr. A. H. Garrod, the Prosector to the Society, will give five lectures *On the General Classification of the Vertebrata;* and Dr. Carpenter, F.R.S., will conclude the course by giving two *On the Aquarium and its Inhabitants*. The lectures will be delivered on the Tuesdays and Fridays in April and May, at 5 o'clock in the afternoon; they will be free to Fellows of the Society and their friends, and to other visitors to the Gardens. The subjects will be treated in a manner which will make them of general interest, and it is to be hoped that ladies will avail themselves of the opportunity thus afforded, of obtaining information on this too much neglected branch of the great science of Biology.

MR. PHILIP BARNES, who died on Feb. 24, at the age of 82, was one of the oldest Fellows of the Linnean Society. He was a native of Norwich, and a cousin of the Sowerbys. Thirty-four years ago he founded the Royal Botanic Gardens in the Regent's Park, and was the oldest Fellow and father of the Society. A portrait of Mr. Barnes was in the last International Exhibition, and a bust in that of the year before. He was father of Robert Barnes, M.D., and of the late Philip Edward Barnes, the former known in the scientific world by his professional discoveries and writings, and the latter the author of a work on the Belgian Constitution.

THE death, from heart-disease, of Prof. J. F. Holton is announced as having taken place in Everett, Massachusetts, U.S., on the 25th of January. Prof. Holton was well known as a botanist, having devoted many years to the study of the science. He visited South America with special reference to prosecuting his researches in this direction, and studying the relation between the physical geography and the vegetation of the Andes. His somewhat extended sojourn in that country enabled him to collect materials for a work, which was published after his return by Harper and Brothers, and is frequently quoted by botanists.

MR. WILLIAM DUNVILLE has presented a valuable endowment in trust for ever to the Queen's College, at Belfast. The endowment consists of two studentships, one for the encouragement of the mathematical and physical and the other for that of natural sciences. They are intended by the donor to enable distinguished students who attained graduation to pursue their collegiate studies further. The studentships are tenable for two years, and are of the value of 45*l.* for the first, and 100*l.* for the second year.

WE hear from Cambridge, Massachusetts, that the chair of Zoology held by Professor Agassiz during his lifetime is most probably to be discontinued, and that the teaching he was accustomed to give will, for some time at least, be carried on by Prof. McCrady and Prof. Shaler. Mr. Alexander Agassiz is to be Curator of the Museum, which post being very onerous, prevents him from accepting any professorial work. The new Zoological School at Penikese is also to be under his charge, and we hope that that promising institution will be kept up with vigour notwithstanding the great loss it has sustained in the death of its illustrious founder.

WE learn from the *Lancet* that a memorial to Agassiz is in contemplation. At Boston a meeting for the purpose was addressed by Profs. Rogers and Wendell Holmes, after which it was resolved to make the Museum of Zoology at Cambridge—the work of Agassiz's best years—a memorial monument. For this it was proposed to raise the sum of 300,000 dols. to complete its endowment. 65,000 dols. were subscribed before the proceedings closed.

THE first part of a new Russian work by M. Prijevalsky, entitled "Mongolia and the country of the Tanguts," may be expected before the end of the year. It will contain an account of the author's travels in Central Asia, together with a description of the Zoological and Botanical results he has arrived at. In all, 64 species of mammalia, and 292 species of birds were obtained, including among the most remarkable of the former, the Wild Yak, the Orongo Antelope and *Ovis poli*; of the latter *Gyps nivicola* and a new species of *Pterorhinus*. The botanical collection includes, according to the botanist Maximovitch, a great many new and rare specimens. In the mountains of Kansu about 500 different plants were obtained, including the seeds of the medicinal rhubarb.

THE Cambridge Syndicate appointed to organise courses of lectures or classes with the necessary examinations in a limited number of centres of population have received applications from several places to supply teachers during the ensuing winter. Among the subjects suggested for choice are Political Economy, Mental and Moral Science, History, English Literature, Physiology, Physical Geography, Geology, Astronomy, Mechanics, various branches of Physical Science, and other subjects of a kindred character to these. The remuneration offered varies from 125*l.* to 200*l.* for the term of three months, there being two such terms to be provided for between October and May. The pupils are chiefly from among young men and women of the middle and working classes. The Syndicate request any gentleman willing to take part in such work to send his name and the statement of subjects he would be willing to give instruction in to Mr. Stuart, M.A., Trinity College, the secretary.

THE Council of the Senate of Cambridge University recommend that a Demonstrator of Experimental Physics be appointed at an annual stipend of 150*l.* The duties of such person shall consist of assisting the Professor in giving class instruction and making experiments. He is to be appointed by the Professor, with the consent of the Vice-Chancellor. A discussion of this report takes place to-day.

M. J. PLATEAU has recently published, in two volumes, a work entitled "Statique expérimentale et théorique des liquides soumis aux seules forces moléculaires" (London: Trubner). M. Plateau has effected the realisation, on a large scale, of a part of the figures of equilibrium, indefinite in number, which would affect liquids if gravity did not act upon them; he has thus furnished the experimental verification of a series of results obtained by geometers in respect to surfaces whose mean curvature is constant, such surfaces as those of figures of equilibrium. The work referred to contains an account of the author's researches on the forms and phenomena presented by liquids in the condition named, as well as the consequences which result therefrom. The following are two examples of these consequences:—1. The froth which is formed on champagne and other liquids is evidently an assemblage of laminæ, which enclose in their interstices small portions of gas. One might naturally expect that in this assemblage all would be ruled by chance, but it is nothing of the kind; the small laminæ never unite but three and three, and make with each other, at the small liquid edge which unites them, equal angles of 120°. Moreover, the liquid edges throughout unite four and four, and thus form between them, at the point where they meet, equal angles, angles whose cosine is $-\frac{1}{3}$. 2. The beautiful observations of Savart have taught us that a vein of liquid pierced by a circular orifice is gradually converted, during the passage of the liquid composing it, into a series of isolated masses. The illustrious French physicist, to account for this phenomenon, has tried to prove that the very act of flowing gives rise, in the orifice, to pulsations which produce in the vein successive protuberances, and this hypothesis has been adopted by most of the students who have inquired into the matter. M. Plateau shows that this ingenious notion is quite insufficient to account for the facts, that the conversion into isolated masses is a result of the molecular forces which are in action at the surface of the vein, and that from this naturally result all the particulars established by Savart.

SCIENCE seems likely to be treated royally in Sweden this year. For the expenses of the Congress of Archæology and Prehistoric Anthropology, which will be held at Stockholm from Aug. 7th to 16th, the Government has asked from the Diet a grant of 20,000 fr.; a magnificent palace has been set apart for the holding of the congress; two grand fêtes will be given by the king and by the city; and visitors will be carried by the railways at half-fares. The programme includes papers and discussions on the stone age, bronze age, and iron age, and on prehistoric archæology; and excursions will be made to places of archæological interest and remains of prehistoric man in the neighbourhood. The "Congrès d'archéologie slave" will also be held at Kiew, from Aug. 14 to Sept. 3. Altogether, students of prehistoric man will have a good time of it in North Europe this summer.

THE appointments to the Bureau des longitudes at Paris for 1875 are—M. Puiseux as president, M. Faye as vice-president, and M. Yvon-Villarceau as treasurer and secretary.

THE French Academy is publishing a large 4to volume of 300 pages, containing all the reports and maps relating to the next Transit of Venus. A copy has been presented to each member, and the book is to be had at M. Firmin Didot's, the publisher to the French Institute.

SOME carpenters are at present engaged in building in the Jardin de Luxembourg at Paris a photographic studio, for the use of the photographers who are to be sent out with the Transit expedition. The observations are soon to begin, and will be under the direction of M. Fizeau, member of the French Institute; but that gentleman will not leave Paris to follow the operations.

THE young King of Siam having come of age on October 10 last, great feasts were given to his subjects at Bangkok, the chief town of his dominion. Amongst other attractions was the ascent of a small mounted balloon, which had been constructed in Paris and had arrived by steam a few days previously.

Liberal offers were made to procure an aëronaut, but were of no avail, nobody amongst the Siamese presuming to ascend. Consequently his Majesty ordered a slave, selected from amongst the less heavy of his household, to be sent up in the car. In order to encourage the poor aeronaut, so frightened for his life, he was promised to be rewarded with his enfranchisement. The ascent took place and elicited much enthusiasm from the bystanders; but, unhappily, nothing was heard from the poor fellow or of the craft.

THE Universal Exhibition to be held in the Champs Élysées Palace in 1875 is merely a private enterprise; the French Government having no intention to interfere except in giving its authorisation. No charge will be made on the national Exchequer, but it is rumoured and hoped that the Municipal Council of Paris will grant a considerable sum of money.

MR. G. J. SYMONS writes to yesterday's *Times* suggesting various methods, all good, and we think practicable, of distributing daily, or even at certain intervals during each day, the accurate time throughout London. This is an advantage possessed for long by many provincial towns; though London in this, as in many other respects, is far more "provincial" than many a second-rate provincial town. We are glad to see, however, from Mr. W. Abbott's letter in the same paper, that the want complained of by Mr. Symons will soon, to some extent, be remedied; as one of the objects of the British Telegraph Manufactory (Limited), which has just taken over all the inventions of Sir Charles Wheatstone, is to establish a large electrical driving clock in a central position of the metropolis.

THE Commissioners of the Fairmount Park, Philadelphia, U.S., are making great efforts in the way of bringing together, in the form of a zoological garden, a complete collection of animals of North America, with a view of their exhibition at the approaching Centennial Exhibition. The Commissioners are also expecting considerable consignments from other parts of the world, as South Africa, South America, &c., and the whole enterprise bids fair to assume a very great magnitude.

AT a meeting of the California Academy of Sciences in November last, photographs of strange but beautiful hieroglyphics, cut in wood, and found on Easter Island, were received from Mr. Thomas Croft, of Papeeti, Tahiti. From vague traditions among the natives, they were supposed to represent the written language of some prehistoric nation. The stone idols found on the island exhibit a refined form of art, and other relics found there go to prove that the present population has gradually degenerated from a previous one. In the letter accompanying the hieroglyphics, Mr. Croft stated from the best information he could obtain, that none except the priests and a chosen few could decipher these strange characters. A letter was read from this gentleman at the last meeting, in which he stated that he had found a native of the island who could read them, and who was going to teach Mr. Croft the language, so that he will shortly be able to translate them. Mr. Croft thinks that he has discovered the relics of a great Malayan empire, which extended its power over that part of the ocean at some former period of the island's history.

"THE Treasury of Languages, a Rudimentary Dictionary of Universal Philology" (London: Hall and Co.), is an attempt at making an exhaustive alphabetical list, with brief explanations, of all the known languages and dialects of the world. It contains, besides, explanations of terms used in the science of language. The volume contains 300 pages, with an average of fifteen names on each page; this will convey some idea of the variety of tongues on the face of the earth. The author, who is nameless, but who, we are told, is a "literary amateur," moreover intimates that he has received additional material sufficient to make a second volume. What a bewildering field is before the student of languages, to whom the present work is calculated to be extremely useful.

WE have received a second and richly illustrated edition of Mr. Hartwig's "Polar World" (Longmans). The record of Arctic discovery has been succinctly brought up to the present time, and the work is well calculated to convey to the general reader a vivid, and on the whole correct, idea of man and nature in the Arctic and Antarctic regions of the globe.

PART I. of Vol. V. of the "Natural History Transactions of Northumberland and Durham" (Williams and Norgate) has come to hand. It contains the usual Annual Address of the President, Mr. H. B. Brady, F.L.S., who recounts the excursions of 1872, and touches on one or two important questions of the day, with clearness, vigour, and brightness. The following are the titles of the papers in this part:—"Note on the recent occurrence in Northumberland and Durham, of the Camberwell Beauty Butterfly," by T. J. Bold, who also contributes papers on "The Museum Collection of British Insects," and "The Occurrence of Lepidoptera in Northumberland and Durham in 1872;" "Note on Bones dredged from the bed of the river Wear in 1872," by Dr. D. Embleton; "Meteorological Report for 1872," by the Rev. R. F. Wheeler and the Rev. Dr. R. E. Hooppell; "First Instalment of a Catalogue of the more remarkable Trees of Northumberland and Durham," by Mr. G. C. Atkinson, who has devised a "hypsometer," a simple but useful instrument for ascertaining the height of trees; "Note on Cinerary Urns found at Humbledon Hill, near Sunderland."

PART III. of Vol. III. of "Proceedings and Transactions of the Novia Scotian Institute of Natural Science," has been sent us. Besides a summary of the Proceedings of the Society, it contains twelve papers of varying value on scientific subjects, eight of these being by three of the members; this Society, like many others at home apparently, having many names on its roll but few working members. Three papers are by the Rev. Dr. Honeyman, F.G.S., Director of the Provincial Museum:—"On the Geology of Nova Scotia and Cape Breton," "On the Metamorphism of Rocks in Nova Scotia and Cape Breton," and "The History of a Boulder." Of the other papers we may mention two by Dr. J. B. Gilpin on "The Eagles of Nova Scotia," and "The Stone Age of Nova Scotia;" "The Great American Desert," by Mr. H. S. Poole; and "The Vegetation of the Bermudas," by Mr. J. M. Jones, F.L.S. Appended is a brief note on the visit of the *Challenger* to Halifax.

THE "Report of the Birmingham School Natural History Society for the year 1873," is on the whole satisfactory. All the sections seem to be in good working order, their meetings fairly attended, and some profitable field-work is being done. The papers, abstracts of which are published in the report, are creditable to the young gentlemen who wrote them.

WE have received a large sheet containing Statistical Tables relating to the Colony of Victoria, compiled from official records in the Registrar-General's Office, Melbourne, by Mr. W. H. Archer, Registrar-General. The tables contain a vast amount of information, well and compactly arranged, concerning the population, industry, education, &c., of the colony. The sheet contains also the usual meteorological statistics for the twelve months of the year, and extending over a period varying from six to fourteen years.

THE additions to the Gardens of the Zoological Society during the past week include a Common Rhea (*Rhea Americana*) from S. America, presented by Mr. A. Maxwell; a Black-tailed Godwit (*Limosa melanura*), British, presented by Mr. H. Stacy Marks; a Red-faced Deer (*Cervus anops*) and two Falcated Teal (*Querquedula falcata*) from China, purchased; a Chinese Water Deer (*Hydropotes inermis*), a Reeves' Muntjac (*Cervulus reevesi*), and a Japanese Teal (*Querquedula formosa*) from China; a Collared Peccary (*Dicotyles tajuça*) from S. America, deposited.

SCIENTIFIC SERIALS

American Journal of Sciences and Arts, January 1874.—This number commences with an account, by Mr. H. Gillman, of some Indian mounds and skulls in Michigan. The numerous tibiæ unearthed showed the compression or flattening which characterises platycnemic men; and the race, from Detroit River to St. Clair and Lake Huron, seems to have been marked with platycnemism to an extreme hitherto unobserved in any other part of America, or perhaps any other country in the world. The writer thinks the type of bone will be found predominant in the entire region of the great lakes.—Mr. Hilgard follows with a note on silt analyses of Mississippi soils and sub-soils (the author having used his "churn elutriator"); and Mr. Longbridge discusses the distribution of soil ingredients among the sediments obtained in silt analysis, and the influence of strength of acid and time of digestion in the extraction of soils.—Mr. Lesquereux communicates the remarkable discovery that traces of land vegetation exist in the Lower Silurian of America; branches or small stems of a species referable to *Sigillaria* having been found by the Rev. H. Herzer in clay beds of the Cincinnati group. The only records hitherto had of vegetable remains from the Silurian of North America are some fragments of stems and rhizomes of *Psilophyton* observed by Dawson in the Gaspé group of Canada; the only link of connection of the Devonian flora with that of the Silurian period. In Europe, too, the first remains of land plants have been found in the Lower Devonian; and as yet only a single specimen of *Sigillaria*. The same writer, in another note, argues against the view, recently advanced, that the lignite beds of the Rocky Mountains have been formed by the heaping up of drifted materials. We also find notes on the geology of Western Texas (Jenney), on the results of recent dredging expeditions on the coast of New England (Verrill), on fossil woods of British Columbia (Dawson), on a combination of silver chloride with mercuric iodide (Lea), &c.—An appendix contains a paper (with two plates) by Prof. Marsh, treating of the structure and affinities of the Brontotheridæ.

Poggendorff's Annalen der Physik und Chemie, No. 11, 1873.—This number contains the concluding part of M. Kundt's paper on the vibrations of square air plates.—Dr. Schüngel describes some experiments made with reference to change in the pitch of sounds through a movement of translation of the sounding body. He arranged an apparatus in which a tuning fork, mounted, with its case, on a little waggon, was rapidly drawn along (by a cord passing round a drum) towards the observer, who stood beside another fork making a slightly greater number of vibrations than the moved one. In this way a different number of beats was obtained; less than that produced when both forks were at rest. The pressure of a key, giving rise to this motion (through electro-magnets, &c.), caused a telegraphic strip of paper to be at the same time impressed, showing a continuous line; and a second pendulum, closing a current at each swing, produced a series of points on the same strip. By this means could be measured the time in which a certain number of successive beats was heard, and the rate of motion of the travelling fork. The author points out how the method may be employed for determining the velocity of sound, and commends it to the attention of physicists for further development.—M. Zollner replies, at some length, to the considerations urged by M. Reye against his explanation of the sun-spots and protuberances; and M. Behrens communicates a note on porcelain and allied products.—A mercury air-pump, of improved construction, is described by M. Mitscherlich; and a variation-barometer by M. Kohlrausch; the latter instrument being formed with the vacuous metallic ring of a Bourdon aneroid.—M. Herwig makes a calculation of the number and weight of ether-molecules contained in electrical conductors.—Among the extracted matter, we note several important papers; one by Dr. Heinrich Streintz (Vienna Acad.), on the changes in elasticity and length of a wire traversed by a galvanic current; one by M. Plateau (Belgian Acad.) on the measurement of physical sensations, and the law which connects the intensity of these with that of the exciting cause; and one by M. Helmholtz (Berlin Acad.) on galvanic polarisation in gasless liquids.

Astronomische Nachrichten, No. 1,974.—In this number Prof. Spoerer writes a very interesting account of his sun-spot and prominence observations, from which he concludes that faculæ occupy the same places where protuberances arise or where the points of the "flaming chromosphere" are situate; and further, that protuberances are in most cases connected with spots, and are very conspicuous before and at the commencement of a group of spots. In many cases, he says, it is possible to calculate when a spot will appear, from the observation of a flaming protuberance, and that the spots are produced by the substances thrown up becoming cooled and producing a cloud of products of condensation.—Dr. Hugo Goricke contributes a number of observations of position of the minor planets Asia, Flora, Thetis, and Hera.

Astronomische Nachrichten, No. 1,975.—In this number Prof. Schmidt gives an account, in full, of his observations on Sun-spots, the number of groups being given for each day in 1873, the maximum number on any one day being 9, and the minimum 1. There is no day on which the sun was free from spots. Prof. Schmidt says that clouds have prevented observations on 12 days; we, in this country, should be content with missing 120 days. Prof. Schmidt also gives the maxima and minima of a number of variable stars, and we regret that want of space prevents our reproducing his results, which are worthy of perusal by those interested in the matter.

Der Naturforscher, Jan. 1874. In this number we may first note an account of some observations by M. Hann, at Hong Kong and in Ceylon, as to the decrease of temperature with the height. It appears that the yearly average of decrease is much the same in the tropics as in central Europe. During the regular monsoons, the decrease is much more gradual on the windward side of a mountain than on the lee. The quick decrease in time of rain is due, in part, to increase in quantity of rain with the height, and greater cooling in consequence.—From experiments on alcoholic fermentation, by M. Brefeld, it is concluded that alcohol yeast always requires free oxygen for its growth; it cannot grow on oxygen from a compound like sugar; further, that living, but non-growing, yeast-cells (free oxygen being excluded) may yet excite fermentation in sugar solution. As showing the affinity of the yeast-cell for free oxygen, the author states that it may grow in CO_2 containing less than $\frac{1}{100}$ of its volume of such gas, and will fully absorb it.—We also find a note of some experiments on butyric fermentation, by M. Paschutin; and in the botanical department there are several interesting notes.—On the reaction of plant protoplasm to mechanical injuries, by M. Hanstein; the cause of periodical motions in leaves, by M. Batalin, stated to be, chiefly, unequal growth, preponderating at one side or the other, through varying conditions of light, temperature, and turgescence; on the morphological differentiation of the lower plants, by M. Pringsheim, and others.—In a suggestive mineralogical paper, M. Hirschwald theorises on the cohesion-relations in drops and in crystals.—Physics is represented by several extracted notes—on evaporation, on phenomena of polarisation produced through dispersion of light, on relations between capillary and electric phenomena, on intermittance of the electric current, &c., most of which have already been noticed elsewhere in our columns; and in physiology there is an account of a valuable investigation by M. Forster, as to the significance of ash-constituents in food.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Feb. 12.—"On the influence of Ethyl Alcohol on the Bodily Temperature, the Pulse, and the Respirations of a healthy man." By E. A. Parkes, M.D., F.R.S., Professor of Hygiene, Army Medical School.

The author made a large number of experiments on a strong healthy soldier, T.R., aged twenty-five, height 5 ft. $8\frac{1}{4}$ in., weight (naked) 67·46 kilogrammes, or 148 lbs.

The course of the experiments was as follows:—His breakfast was taken at 6.30, was finished every day by 7 A.M.; he took for breakfast 8 ounces of bread, $\frac{1}{4}$ ounce of butter, and 17 fluid ounces of tea with sugar, and with 3 ounces of milk. Immediately after breakfast he went to bed again, and did not get out of the recumbent position for any purpose until 2 o'clock. He then dined on 12 ounces of beefsteak, 4 ounces of bread, and 8 ounces of water.

After dinner he took exercise and smoked, had tea (same food as at breakfast) at 6, and a glass of water at 9 P.M., when he went to bed. He took daily precisely the same diet and quantity of water.

Thermometers (tested for accuracy and exactly corresponding) were placed in the axilla and rectum at 6 o'clock, and, except at breakfast, they were removed only for the purpose of being read at first every 30, and then every 15 minutes, and were at once replaced, until 2 o'clock; after which time the temperatures were only taken every two hours.

After several days' preliminary examination (during which time he took no alcohol) the experiments were commenced and carried on for six days without alcohol; then during five days undiluted brandy containing 50 per cent. of absolute alcohol was given once daily, viz. at 11 A.M., four hours after breakfast.

On the first day one fluid ounce of brandy (= ½ ounce of alcohol) was given; on the second day two ounces, on the third day four ounces, on the fourth day six ounces (= 3 ounces of alcohol), and on the fifth day also six ounces.

The following were the conclusions arrived at:—

1. The change in the temperature of the axilla and rectum produced by brandy was very slight. It was never increased, but was probably slightly lowered; but the result is not quite certain; and if any lowering occurred, it did not exceed 0°·35 Fahr., and may not have been more than 0°·07 Fahr.

2. The pulse, which was lessened in number by long rest in a recumbent position, was increased in frequency by a single dose of brandy for three hours, but subsequently fell in number, so that the daily work done by the heart was the same on the water and the brandy days. What occurred was accelerated work for a certain time, and compensation for this by lessened work afterwards. That brandy increases the force as well as the number of the pulse, was shown by sphygmographic tracings in the papers already communicated to the Royal Society; and in order not to disturb the state of rest, no sphygmographic observations were taken in this case.

3. The respirations appeared to be slightly lessened by brandy; but the evidence is not very strong.

The author made another series of experiments to determine the effect of alcohol after sixteen hours' fasting.

The following conclusions may be drawn from the observations formerly recorded ("Proceedings of the Royal Society," Nos. 120, 123, and 136), and from those now laid before the Royal Society:—

1. When alcohol in dietetic doses (=2 fluid ounces, or 57 cub. centims., of absolute alcohol) was given to a healthy man fasting and at rest, a decided though slight lowering of bodily temperature (as judged of by the heat of the rectum) was caused. The amount of lowering was under half a degree of Fahrenheit; and sometimes even this amount was not perceptible, being probably counteracted by the opposing influence of the heat-producing changes in the body, which cause slight variations of temperature independent of food and movement. The greatest effect was produced about from one to two hours after the alcohol was taken, and the effect was evidently passing off in three hours.

2. When alcohol in dietetic doses was given to a healthy man at rest, and in whom the process of digestion was completed, and whose temperature, raised by the food, was again commencing to fall, a lessening of temperature was also proved, but its amount was not so great; it could not have been more than 0°·35 Fahr., and may have been only 0·07 Fahr.

3. When alcohol was given with food with either usual or increased exercise, no effect on temperature was perceptible, even though the alcohol was given in large quantities, viz. from 4 to 8 fluid ounces of absolute alcohol (114 to 227 cub. centims.) in twenty-four hours. It is to be presumed that the amount of heat generated from the food and movement concealed the effect of the alcohol, which would require a more delicate method for detection.

4. In no case did alcohol raise the temperature.

5. The effect of alcohol on the pulse was uniform in the four men experimented upon. The contractions of the heart were more frequent during complete rest, from five to ten beats per minute for some time; and when exercise was taken the increase was greater. The mean pulse of the twenty-four hours was, however, not increased unless the amount of alcohol was large and repeated. In other words, the heart's beats were less frequent than natural when the effect of the alcohol had passed off. The pulse became both fuller and softer to the touch; and this relaxation of the radial artery was shown also by the sphygmograph. That the smaller vessels were relaxed was shown by the redness of the surface and by the evident ease with which the blood traversed the capillaries, as shown by the sphygmographic tracings.

6. The respirations were not increased in number by alcohol; they were indeed lessened, and were deeper in some of the experiments; but the effect was not very marked.

Feb. 26.—"The Winds of Northern India, in relation to the Temperature and Vapour-constituent of the Atmosphere," by Henry F. Blanford, F.G.S., Meteorological Reporter to the Government of Bengal.

Geological Society, Feb. 20.—His Grace the Duke of Argyll, K.T., F.R.S., president, in the chair. In handing the Wollaston Gold Medal to the foreign secretary, Mr. W. W. Smyth, for transmission to Prof. Heer, of Zurich, the president referred to the fact that last year the council had awarded the balance of the proceeds of the Murchison Geological Fund to Prof. Heer, and remarked that it gave him much pleasure that the Wollaston Medal, the highest honour which the Society had it in its power to confer, should be so worthily bestowed. He alluded briefly to the labours of Prof. Heer in the difficult departments of Fossil Botany and Entomology, and to the admirable works in which he had given to the world the results of his indefatigable researches.

Mr. W. W. Smyth, in reply, said:—"My Lord President, it is with a great pleasure that I undertake the transmission to Prof. Heer of this new testimony of the importance attached by this Society to his long-continued labours. I have received from our valued foreign member a letter stating that my announcement of the award had found him extended on the bed of sickness, and begging me to assure the Society that, but for this misfortune, nothing would have given him greater pleasure than to have been present at this meeting, and to have thanked the Society personally for the high honour which has now been awarded to him."

The President then presented the balance of the proceeds of the Wollaston Donation Fund to the foreign secretary for transmission to Dr. H. Nyst, of Brussels, remarking that this distinction had been well earned by Dr. Nyst by his admirable researches upon the Molluscan and other fossil remains of his native country.—Mr. W. W. Smyth briefly thanked the president on behalf of Dr. Nyst.

The president next presented the Murchison Medal to Dr. J. J. Bigsby, F.R.S., and remarked in so doing that there was a peculiar fitness in this award, which would have met the approval of the distinguished geologist in accordance with whose last wishes this medal was given. It was awarded to Dr. Bigsby in recognition of his long and valuable labours in that department of geology and palæontology with which the name of Murchison is more particularly connected.—Dr. Bigsby replied, thanking the Society for the honour conferred upon him, and the president for the terms in which he had spoken of his labours.

The president then handed half the balance of the proceeds of the Murchison Geological Fund to R. Etheridge, F.R.S., for transmission to Ralph Tate, F.G.S., expressing a hope that it would be regarded by him as a testimony of the value set by the Society upon his palæontological researches, especially on the Fauna of the Lias, and that it would enable him to enlarge the sphere of his investigations.—Mr. Etheridge, in reply, read the following letter of acknowledgment from Mr. Tate:—

"My Lord President and Gentlemen, To say that I am unworthy of the honour that you have awarded me by the bestowal of the 'Balance of the Proceeds of the Murchison Fund,' would be to call into question your judgment, and would render nugatory its value to me. The encouragement that such an award conveys is ample recompense for labour bestowed in palæontological research, and is a real incentive to more diligent work. It is in this spirit that I accept the award, and tender my warmest thanks to you for the distinction it confers. It is now twelve years since I was led to select for special study the geological history of the Lias, which appeared to me not to have received that attention at home that it had upon the Continent, and which it claimed by offering the earliest phase of Mesozoic life, and presenting a number of physical problems that seemed upon the threshold of the inquiry to reward even the casual observer with a rich harvest. I have published from time to time fragments relating to the stratigraphy and palæontology of this period, but I hope soon, in conjunction with my friend Mr. J. F. Blake, F.G.S., to submit, in a work entitled 'The Yorkshire Lias,' a comprehensive review of the chief characteristics of the period, embracing the remarkable variation of mineral conditions, and the particular distribution of organic life, as indicative of peculiarities of depth of ocean, the direction and proximity of land, &c. Despite all these efforts, the ambition to acquire the position of an expositor of the life of this interesting group of strata urges me to the completion of a *Prodromus* or *Thesaurus Liassicus*, the materials for which have been accumulated during several years; but from the great labour demanded to bring into harmony the nomenclature of the fossils, without which the compilation can have no real value, some time must elapse before the results can be submitted to you.—Faithfully yours, Ralph Tate."

The President then presented to Mr. Alfred Bell the other half of the balance of the proceeds of the Murchison Geological Fund, and stated that this was awarded to him in recognition of his valuable researches upon the fossils of the newer Tertiary beds of this country, and to assist him in the completion of his work upon the Crag deposits of the eastern counties. Mr. Bell, in reply, said that he was most grateful for this token of the Society's appreciation of the value of his labours, and stated that up to the present time he had been enabled to distinguish about 3,000 fossil species from the newer Tertiaries of Britain, and that he hoped yet to add very largely to their number.

The President then proceeded to read his Anniversary Address, in which he stated that the pressure of his official duties during the period of his presidency had prevented his keeping himself thoroughly acquainted with the recent progress of geological research, and he therefore proposed in his present address to advert rather to those questions in geology which seemed to him still to require an answer. He referred to the relations between geology and cosmogony, to the effects and causes of volcanic and earthquake action, and finally to the great questions which are still unsettled as to the origin of life and the sequence of organic beings on the face of the earth. The address was prefaced by some obituary notices of Fellows and Foreign Members and Correspondents deceased during the past year, including Mr. J. Wickham Flower, Mr. J. Garth Marshall, Prof. Agassiz, and M. de Verneuil.

The Ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President—John Evans, F.R.S. Vice-Presidents—Robert Etheridge, F.R.S.; R. A. C. Godwin-Austen, F.R.S.; Sir Charles Lyell, Bart., F.R.S.; Joseph Prestwich, F.R.S. Secretaries—David Forbes, F.R.S.; Rev. T. Wiltshire, M.A. Foreign Secretary—Warington W. Smyth, F.R.S. Treasurer—J. Gwyn Jeffreys, F.R.S. Council—The Duke of Argyll, K.T., F.R.S.; H. Bauerman; Prof. G. Busk, F.R.S.; J. F. Campbell; Frederic Drew; Sir P. de M. G. Egerton, Bart., F.R.S.; R. Etheridge, F.R.S.; John Evans, F.R.S.; David Forbes, F.R.S.; Capt. Douglas Galton, F.R.S.; R. A. C. Godwin-Austen, F.R.S.; J. Gwyn Jeffreys, F.R.S.; Sir Charles Lyell, Bart., F.R.S.; C. J. A. Meyer; J. Carrick Moore, F.R.S.; Joseph Prestwich, F.R.S.; Prof. A. C. Ramsay, F.R.S.; Samuel Sharp, F.S.A.; Warington W. Smyth, F.R.S.; Prof. J. Tennant, F.C.S.; W. Whitaker, B.A.; Rev. T. Wiltshire, F.L.S.; Henry Woodward, F.R.S.

Anthropological Institute, Feb. 24.—Sir Duncan Gibb, Bart., M.D., in the chair.—Mr. Biddle Lloyd, C.E., F.G.S., read a paper on the Beothucs, a tribe of Red Indians, supposed to be extinct, which formerly inhabited Newfoundland. The author, after reviewing the various accounts related of the aborigines of the island from the time of Sebastian Cabot downwards, gave the results of the information he picked up from various sources during an exploratory cruise he made last summer round the coast of Newfoundland, respecting the strange tribe of Indians which inhabited the island up to a period which terminated about forty years ago, when, by reason of the cruelties practised on them by the English fishermen, and the warfare carried on against them by the Mic-mac Indians, they were reduced in number, and finally the few of them that were left, it is supposed, crossed over the straits of Belleisle, or at all events disappeared. Several singular circumstances in connection with the Beothucs, as they styled themselves, were noticed: namely, the curious shape of their birch-bark canoes, the fact that the dog was not domesticated by them, and their manner of hunting the Caribou by means of long lines of fencing put up to keep the herds of deer along certain tracks.—Mr. Lloyd also read notes on Indian remains found on the coast of Labrador. The Indian remains found on the coast of Labrador consisted of rudely-constructed buildings, of stone slabs, which were discovered on the sea-shore at the western entrance of the straits of Belleisle. They were described to the author as Indian graves, but there was no evidence to show that such was the use to which they had been applied. On the contrary, it seemed probable they were stone wigwams built by some Indian families for a summer residence. The author was fortunate enough to discover at L'Anse du Diable, which is a cave situated about 20 miles east of the locality where the so-called Indian graves were found, a few arrow-heads of quartzite and hyaline quartz on a sandy "barren" which stretched inland from the head of the cave. From circumstances connected with the cave, the author concluded that the locality had been chosen by some unknown tribe of Indians for the manufacture of their arrow-heads during an occupancy of some considerable time on the spot.—A paper was read by Dr. Sinclair Holden on a peculiar Neolithic implement from Antrim.

Royal Horticultural Society, Feb. 10.—Annual General Meeting.—Viscount Bury, president, in the chair.—The Report of the Council having been taken as read, it was moved by Mr. Haughton as an amendment to the motion for its adoption, and seconded by the Rev. C. P. Peach, that the meeting be adjourned to enable the opinion of the Court of Chancery to be taken as to the legal position of the Society. (The commissioners of the Annual International Exhibitions dispute the validity of the election of the Council chosen last year.) The amendment being put to the vote was lost by a majority of six, the numbers (including ladies' proxies) being, for, 225; against, 231. The report was then put and carried. The following vacancies were filled for the ensuing year:—President—Viscount Bury, K.C.M.G. Treasurer—Mr. Bonamy Dobree. Secretary—Mr. W. A. Lindsay. Members of Council (extraordinary vacancies)—Lieut.-Gen. Hon. Sir A. H. Gordon, K.C.B.; Mr. Joseph Robert Tritton; Mr. Burnley Hume; Mr. Henry Webb.

General Meeting, Feb. 18.—Henry Little in the chair.—The Rev. M. J. Berkeley commented on a plant shown by Mr. Bull, under the name of *Rapatea pandanoides*. It is a species of *Saxo-fridericia*. He also gave some account of Dr. Cunningham's microscopic examinations of air in Calcutta.

Scientific Committee.—A. Smee, F.R.S., in the chair.—A large number of subjects were brought before the Committee. Among the more important were—Mr. Grote: The Tea-bug of Assam, supposed by Prof. Westwood, from the figures, to belong to the Cimicideous family, Capsidæ, and nearly allied to a species which injures chrysanthemum-buds. Mr. A. Muller thought it much more likely to be some aphis, though it might be immature.—A communication was sent through Dr. Hooker on a new disease of the coffee plantations in India (Tellicherry). The leaves turn yellow, and the back is found to be covered with a rust-coloured dust. Further information was requested.—Prof. Thiselton Dyer exhibited specimens of the Balaniform gabey gall of the oak with specimens of the Cynips which had been bred from them. These had been identified by the Rev. T. A. Marshall as *C. radicis*, Fab. He also read a note from Mr. Fenn, of Woodstock, as to the practical impossibility of making keeping wine from out-door ripened grapes without the addition of sugar—a point of interest in connection with the supposed deterioration of the English climate; also a note on the condition of an armour-plated ship which was being rapidly destroyed by dry-rot, and a photograph of the tree of the orange or Pearmain apple, with a drawing of the branch which had produced the russet sport exhibited to the Committee last November.

Entomological Society, Feb. 16.—Sir Sidney Smith Saunders, C.M.G., president, in the chair.—Mr. Weir exhibited a sample of wheat from Australia infested with a weevil, *Sitophilus oryzæ*; the cargo was so much damaged that about two tons were utterly useless. The weevil was accompanied by *Læmophlœus ferrugineus*. Some wheat from Japan was also infested with *Sitophilus granaria* and *Rhizopertha pusilla*.—Mr. Higgins exhibited a number of *Cetoniadæ* from the Philippine Islands, which had been described by Dr. Mohnike.—Mr. F. Smith read extracts from a letter from Mr. J. T. Moggridge of Mentone, on a small beetle, *Coluocera attæ*, Kraatz, found in the granaries of *Aphænogaster* (*Atta*) *structor*; and stating that *Platyarthrus* was also very common in the nests. He was much struck by the frequent occurrence of the nests of trap-door spiders in the very soil of the ants' nests; the spiders' tubes often running quite close to, and in the midst of, the galleries of the ants. As ants form a large portion of the food of trap-door spiders, this helped him to understand how it was that the spiders got a living without leaving their nests.—Some conversation took place on the ravages of the Colorado potato beetle (*Doryphora decemlineata*) in North America; a writer in the *Times* recommending the encouragement of small birds as the best security against the pest; but it was much doubted whether the small birds would care to meddle with the insect, as it was stated that when crushed it caused blisters on the skin, and that if a wound was touched severe inflammation and painful ulcers followed.

Institution of Civil Engineers, Feb. 10.—Mr. T. E. Harrison, president, in the chair.—The paper read was on the construction of Harbour and Marine Works with artificial blocks of large size, by Mr. Bindon Blood Stoney, M.A. The author described a new method of submarine construction, with blocks of masonry or concrete far exceeding in bulk anything hitherto attempted. The blocks were built in the open air on a quay or wharf, and after from two to three months' consolidation, they were lifted by a powerful pair of shear legs, erected on an iron barge or pontoon. When afloat, the blocks were conveyed to their destination in the foundations of a quay wall, breakwater, or similar structure, where each block occupied several feet in length of the permanent work, and reached from the bottom to a little above low-water level. The superstructure was afterwards built on the top of the blocks in the usual manner by tidal work. By this method the expenses of cofferdams, pumping, staging and similar temporary works were avoided, and economy and rapidity of execution were gained, as well as massiveness of construction, so essential for works exposed to the violence of the sea.

EDINBURGH

Royal Society, Monday, March 2.—Sir Robert Christison, honorary vice-president, in the chair.—The following communications were read:—"On the Parallel Roads of Glen Roy, with a Notice of finding Fossil *Diatomaceæ* in the Deposits," by the Rev. Thomas Brown.—"On the Perception of Musical Sounds," by Dr. M'Kendrick.—"On the Establishment of the Elementary Principles of Quaternions on an Analytical Basis," by Mr. G. Plarr. Communicated by Prof. Tait.—"Preliminary Note on Spectra under exceedingly small Pressures," by Prof. Tait and Mr. J. Dewar.—"Laboratory Notes," by Prof. Tait (1) On Atmospheric Electricity; (2) On the Thermoelectric Position of Sodium.

DUBLIN

Royal Irish Academy, Feb. 9.—Rev. J. H. Jellett, president, in the chair.—W. H. Baily, F.L.S., read a preliminary report on the plant-fossils of the Kiltorkan district. In this preliminary report Mr. Baily stated that the most frequent plant met with is *Palæopteris hibernica*, first noticed by the late Prof. E. Forbes, under the provisional name of Cyclopteris, afterwards referred to Adiantites by A. Brogniart, and now placed by Schimper in his genus Palæopteris, differing as it does from the former genus in the arrangement of its leaflets and from the latter in its mode of fructification. Some of the fronds met with were nearly five feet long and three wide. Two new species were described as *Sphenopteris hookeri* and *S. humphresianum*, both of which were comparatively rare.—A fine example of the stem of *Sagenaria baileyana* of Schimper was met with, the total length of which was 20 ft. 4 in., and at its lowest portion it was 6 in. in diameter; it branched at about 15 ft. from the base; and the upper portion of these branches corresponds with *Cyclostigma minuta* of Haughton. Cone-like bodies, somewhat resembling Lepidostrobus of the coal were met with. They are composed of elongated scales, terminating in long linear processes showing large and very distinct sporules.—These presumably belong to the Sagenaria but have never yet been found attached.—The report was referred to Council for publication.—Mr. G. Kinahan, of the Geological Survey, believed the report was a most valuable one, and that the researches of Mr. Baily had proved that many of Mr. Carruther's species were but portions of the same plant.—Prof. M'Nab read a report on some researches into the physiology of plants. These experiments were first a series to determine the amount of water transpired by leaves, and secondly the ascent of water in the stem. The plants selected for both series of experiments were the cherry-laurel, the common privet, and the common elm. It would be impossible to condense the large series of experiments made by the author. One series, to determine the amount of water transpired by leaves, made on August 7, 1873, showed that, with very nearly the same exposure and under the same conditions, the cherry-laurel lost, of water, 51·81 per cent. of the weight of the branch employed; the privet, 26·78; the elm, 65·61. Very many experiments were made to determine the actual rate at which fluid ascends in the stem. In Sach's experiment on this subject he fixed the rate to be 9 in. per hour. In Dr. M'Nab's first experiments he obtained a rate of 24 in. per hour. The present series of experiments were made on the same species of plants mentioned above. In the privet the rate was 6 in. per hour; in the elm the rate was 15·6 in. per hour. But in both plants the leaves and stem soon became placid, and the experiments were not completely satisfactory. In the cherry-laurel the rate in one experiment was 24 in. per hour; in a second, 13·2 in. per hour; and in a third, 18·6 in. per hour. The author also recorded a large series of experiments: 1. As to the rapidity of the ascent of fluid in stems when in (*a*) sun, (*b*) diffused daylight, and (*c*) darkness. 2. Rapidity of ascent in branches cut off in the dark. 3. Rapidity of ascent in branches with the cortical tissue removed. 4. Rapidity of ascent in stems deprived of their leaves. 5. Rapidity of absorption of lithium when applied at apex of the branch; and 6. Rapidity of ascent when fluid was taken up under pressure of mercury, intended to represent the root pressure of the plant.—This report was also referred to Council for publication.

VIENNA

Geological Institute, December 3, 1873.—One of the most obscure questions in the geology of the Austrian empire has been the geological position of the Vienna and Carpathian sandstones which form a broad continuous zone on the northern flank of the Austrian Alps, and by far a broader one still on the northern and eastern flank of the Carpathians in Moravia, Silesia, Hungary, Galizia and Transylvania. Only in Silesia, by the investigations of the late Hohenegger, and in Northern Hungary, by those of M. C. Paul, a more satisfactory knowledge has been obtained on this subject. They agree that in both regions the Carpathian sandstones may be divided into several easily distinguishable groups which belong partly to the older tertiary, and partly to the cretaceous formations. Two very valuable memoirs on this subject were read; the first from M. F. Herbich, on the Carpathians in Eastern Transylvania, between the Gyimes and the Tomós Pass. The lowest member of the Carpathian sandstones is formed here by white or yellowish sandstones, which, higher up, pass into coarse conglomerates and belong to the middle neocomian formation; they are covered by a large series of dark-grey sandstones which contain characteristic fossils, and belong to the upper neocomian. The next member, developed near Zaizon, is a grey limestone with Caprotina Lonsdali, identical with the well-known Caprotina limestone of the Alps, and belonging also to the upper neocomian. Above the neocomian strata follow again different sandstones, which M. Herbich thinks to be identical with the Godula sandstones of Hohenegger, and therefore to belong to the Gault. The second memoir, by M. Ch. Paul, treats of the Carpathian sandstones in the eastern Bukowina. They are divided in five different stages, viz., (1) Lower Teschen slates; (2) Neocomian Aptychus limestones; (3) Ropianka beds; which were formerly thought by the author to belong the Eocene series, whilst now he considers them as probably Cretaceans. They are of very great importance, because they contain in Galicia and Bukowina, as well as in Hungary, exclusively the sources of petroleum. 4. Menilith schists, with nests of fossil fishes, which are generally thought to belong to the middle oligocene formation. 5. Magara sandstone, probably upper oligocene.—Dr. Neumayer on the character and the distribution of some Neocomian cephalopods. The author, referring to a former memoir, ("Jahrb. d. k. k. geol. Reichsanstalt," 1871, p. 521), states that the European jurassic deposits form three different provinces, the Mediterranean, the middle European, and the Russian province. By very interesting observations on the faunas of these provinces, as well as on that of the neocomian period, he establishes some facts relating to the physical geography of the mesozoic period. First he states, that at the end of the jurassic time, the middle parts of Europe were laid dry, and whilst therefore the marine life in the middle European province ceased, it continued, and was differently developed in the Mediterranean (deposits of Stramberg, of Berrias, &c.) and in the Russian province. Afterwards, about the time of the Valenginien, the middle part of Europe was again submerged and now peopled partly from the northern and partly from the southern seas; that the immigration went really partly from the north, is proved by the very curious and close affinities between some of the middle European neocomian cephalopods and those of the upper jurassic strata of Russia.

Academy of Natural Sciences, Dec. 11, 1873.—M. Hoernes described the geological features of the island of Samothrace, from observations made in the spring of 1873.—Prof. Knoll presented a paper on the reflex effects produced on respiration, by introducing some volatile substances into the air passages under the larynx. When chloroform is inhaled through a tracheal canula (the mucous membrane of the nose being guarded against its action), there is acceleration and shallowing (*Verflachung*) of the respiratory movements, with low position of the diaphragm, and, sometimes, entire stoppage in the position of inspiration. Ether, benzine, and oil of mustard have a similar, though less, effect. Section of the vagi at the neck shows that these changes depend on reflex action of the vagi. The vapour of a strong solution of ammonia produces great change in the respiration, often lasting several minutes, and varying between a retarding and deepening effect, with long stoppage in position of expiration, and retardation and shallowing in position of inspiration. This also is due to reflex action through the vagi. Inhalation of pure carbonic acid through the tracheal canula produces, both when the vagi are cut and uncut, first, a moderate acceleration, then a considerable retardation of the respiratory movements. No phenomenon occurs which can be explained by a direct stimulation of the vagi by the Co_2.—Dr. Fitzinger communicated a paper on the species of the family of deers (*Cervus*) according to their natural relations. He enumerates twenty different species, four of which he has himself introduced, viz., *Strongyloceros*, *Elaphoceros*, *Dorycerus*, and *Nanelaphus*. To Wagner's species *Macrotis* and *Furcifer*, he gives the names *Otelaphus* and *Creagroceros*, the two former names having had a previous application in zoology. Dr. Schenck presented a note on the eggs of *Raja quadrimaculata* within the oviduct; describing the structure of the shell, and the development of the embryo.—Drs. Nowak and Kratschmer made a communication on phosphoric acid as a re-agent with alkaloids. They find that it gives, with several alkaloids, peculiar colour-reactions, in some of which characteristic reactions of smell are developed. In both respects it presents some advantages over the similarly-acting sulphuric acid It is specially preferable to this in determination of atropia, for reasons which the authors give.

PHILADELPHIA

Academy of Natural Sciences, Oct. 7, 1873.—Dr. Ruschenberger, president, in the chair.—"Law of Seed Germination in Swamp Plants." Mr. Thomas Meehan said that it was an error to suppose that Nature placed trees in places the best suited to their growth. Almost all of our swamp trees grew much better when they could get into dryer places, if in ordinary good land. He referred among others to *Magnolia glauca*, *Acer rubrum*, *Celtis occidentalis*, *Ilex opaca*, *Cupressus chamæcyparis*, *Cephalanthus occidentalis*, *Salix babylonica*, especially as, within his own repeated observations, growing better out of swamps than in them. Why it was that they grew in swamps was no enigma to those in the habit of raising forest trees from seed. It was found that seeds of these trees would only germinate in damp places, and, of course, in a state of nature the tree had to remain in the place where the seed germinated. He thought the principle taught that plants required water to grow well was true only in so far as a humid condition of the soil was concerned. Plants as a general thing, though they were of the class known especially as water plants, preferred to grow out of the water, except in those which grew almost entirely beneath the surface. As was well known, the *Taxodium distichum* in the southern swamps sent up "knees" from various points as the roots extended, often as large as old-fashioned bee-hives, and several feet above the surface.

Oct. 21.—Dr. Ruschenberger, president, in the chair.—"Stibiaferrite, a new Mineral from Santa Clara County, California," by E. Goldsmith.

Oct. 28.—Dr. Ruschenberger, president, in the chair.—The following paper was presented for publication:—"Descriptions of Mexican Ichneumonidæ," by C. T. Cresson.

Nov. 18.—Mr. Vaux, vice-president, in the chair.—The following papers were presented for publication:—"On the Homologies and origin of the Types of Molar Teeth in Mammalia Educabilia," by E. D. Cope; "Contribution to the Ichthyology of Alaska," by E. D. Cope. Prof. Cope remarked that he had observed in the Rocky Mountain region circles of stones arranged by human hands, in countries not now inhabited by the Indians. One of these is in South-western Wyoming near South Bitter Creek, inside the horseshoe of the Mammoth Buttes. The locality is a very barren one, and could hardly be regarded as a camping-ground. The circle consists of three uninterrupted concentric rings close together, the hole having a diameter of about 15 ft. The stones are of moderate size, composed of a dark silex, and evidently derived from the drift material brought down from the Uinta Mountains, which is found on the summits of the bad-land mesas. Five or six miles from this place was found a flint factory with numerous implements and cores. Two other circles were observed, in Colorado, about a hundred miles east of Long's Peak, and about five miles from a spring in a well-grassed country. The locality is unsuitable for a camp, in consequence of the remoteness of wood and water. The country is not inhabited by Indians, the nearest, a temporary camp, for travelling Cheyennes, Sioux, &c., being forty miles distant.

Nov. 25.—Dr. Ruschenberger, president, in the chair.—The following paper was presented for publication:—"Description of Seven New Species of Unionidæ of the United States," by Isaac Lea. The committees to which were referred the following papers:—"On the Homologies and Origin of the Types of Molar Teeth in Mammalia Educabilia," by Edward D. Cope; and "Contributions to the Ichthyology of Alaska," by Edward D. Cope," reported in favour of their publication in the *Journal*.—Disposition of the *Flexor perforans*, *Flexor longus hallucis*, and *Flexor accessorius* in *Paradoxurus musanga* Gray, by Dr. H. C. Chapman.

PARIS

Academy of Sciences, Feb. 23.—M. Bertrand in the chair.—The following communications were made:—On the undulatory movement of a train of wagons due to a shock, by M. H. Resal.—On the acid waters which rise in the Cordilleras, by M. Boussingault.—Determination of vapour densities, by H. Sainte-Claire Deville. The author criticised the apparatus for the determination of vapour densities, recently devised by M. Croullebois.—M. Dumas communicated a note on a process invented by Dulong for taking vapour densities.—Observations concerning the last communication by M. Clausius, on the equation

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by M. A. Ledieu.—M. Milne-Edwards gave news of l'Abbé A. David, now travelling in Western China, and presented, on the part of this naturalist, a note containing descriptions of several new birds.—Memoir on the swim-bladder from the point of view of station (*station*) and locomotion, by M. A. Moreau. The author described some experiments made upon a perch (*Perca fluviatilis*).—Organogenesis compared with Androgenesis in its relations to natural affinities (*Class Œnothereæ*), by M. A. Chatin.—On a new mode of ramification observed in plants of the family of the Umbelliferæ, by M. D. Clos.—Observations relative to a recent memoir by M. Helmholtz upon "Aerial Navigation," by M. W. de Fonvielle. On the lines which are doubly tangential, to the "*surface lines*" of the centres of curvatures of a surface of the second order, by M. Laguerre.—On the permanent magnetism of steel, by M. E. Bouty.—Note on the distribution and determination of thallium, by Mr. T. L. Phipson.—On the presence of metallic silver in galena, by the same author.—Anatomical researches on rickets of the vertebral column," by M. Ch. Robin.—Geological sketch of the Isle of Tros, by M. H. Gorceix.—On a new apparatus for registering the direction of clouds, by M. H. de Parville.—On three new human skeletons discovered in the caves of Menton, and on the disappearance of chipped flints and their replacement by sandstone and limestone instruments, by M. E. Rivière.—On pine-culture in Central France, by M. de Béhague.

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THURSDAY, MARCH 12, 1874

THE LINNEAN SOCIETY

ON Thursday last the Fellows of the Linnean Society met together in a general meeting, which had been specially convened to consider the disputes which have almost paralysed its work for the past two months. One painful episode which arose out of these disputes has already been alluded to in these columns. This alone gave importance to matters which otherwise it would have been difficult to discuss with patience. But so serious a crisis as the resignation of a president so distinguished as Mr. Bentham brought together a larger meeting of the Fellows than had probably ever assembled together before in the history of the Society, and produced the very decided feeling that at least the prospect of a settlement must be reached before the meeting dispersed.

The result was, on the whole, a satisfactory one. After a debate which lasted for about two hours, and in which a considerable number of Fellows took part, a motion proposed by Major-General Strachey was finally carried with only three dissentients, to the effect that the Council possessed the confidence of the Fellows, and that the question of the disputed bye-laws should be referred to some authoritative legal adjudicator, whose decision should be regarded as final.

Those who have had no opportunity of taking any part in the proceedings will naturally wonder what can have been the nature of the portentous questions which have so violently disturbed so grave and staid a body as the Linnean Society. So far as we can arrive at a clear comprehension of the facts, they may be stated as follows :—

At the commencement of the present year the charter and bye-laws were out of print, and the Council having determined to reprint them, before doing so made and submitted to the Fellows a number of amendments in them which appeared to be advisable. It is necessary to explain that, by the constitution of the Society the Council alone has the power to legislate, and the general body of Fellows is only able to reject or ratify what the Council has done. At the meeting on January 15, when the amendments in due course came before the Fellows, the President was requested to put them to the vote *seriatim*, and not *en masse*. This was *primâ facie* a reasonable request, and might, perhaps, have been acceded to without any great inconvenience. The President, however, ruled against it, and his ruling may be defended on two grounds. In the first place the custom of the Society on other occasions appears to have been in accordance with it, and as a general principle it seems obvious that it would be inconvenient for the Fellows to modify in detail a scheme which the Council had presented to them as a whole. In the second place, although the charter is a most difficult instrument for a layman to interpret, it is held by those who ought to be able to construe it, to require that the Council's propositions should be accepted or rejected in their entirety and without modification. The amendments were accordingly put to the meeting *en masse*, and were carried by the necessary majority of two-thirds. The minority declared themselves much aggrieved by the course that had been taken. It is not easy, however, to appreciate their objection; for it is clear that to put all the amendments *en masse* cannot facilitate their acceptance, but that, on the contrary, it brings to bear collectively upon the whole scheme all the objections which might be raised separately to different parts of it.

At the meeting in which the amendments were carried, only one of them was actually objected to. The effect of this amendment was to enable the Council to pay a Fellow to assist in editing the publications. The sum proposed was not large, and it seems very desirable that the work should be paid for, and not voluntary. It is quite obvious that in the former case the secretaries would have no scruple in criticising, if necessary, what was done, which might easily seem an ungracious proceeding in the case of unpaid labour.

Subsequently, however, to the meeting, the minority discovered that another amendment, removing the appointment of the Librarian from the general suffrages of the Fellows to the Council, was repugnant to the provisions of the charter. A competent legal authority has declared that this is not the case; nevertheless, certain of the Fellows hold a contrary opinion, and regard the change as a derogation from their privileges.

We have already referred to what took place on February 5. Mr. Carruthers, who took the lead in the opposition, proposed to discuss the legality of the amendments, and attempted to raise this question upon the confirmation of the minutes of the meeting at which they had been carried. He and his supporters being in a majority in a very thinly-attended meeting refused to acquiesce in the ruling of the President against the regularity of this proceeding; the meeting broke up in confusion, and Mr. Bentham resigned the chair which he has occupied so long to the great advantage of the Society. The difficulties of the Society began like a slight and neglected illness which terminates fatally: before the general body of Fellows had time to even realise the nature of the dispute it had culminated in an event which it will never be possible to look back upon except with the strongest regret. It was, however, a matter for satisfaction that the Fellows assembled last Thursday were anxious to efface this from Mr. Bentham's recollection; and Mr. Carruthers, whose action was the immediate cause which led to the President's resignation, spoke with befitting dignity of the regard he felt for Mr. Bentham's services to the Society and to Science generally, and of his own extreme regret that the course he had considered himself compelled to take had led to such an untoward result.

As to the points apparently in dispute it is difficult to estimate seriously the position of the dissidents from the Council's action. It is objected that the person employed as sub-editor ought not to be a Fellow, or ought on accepting the position *ipso facto* to cease to be one. But where, it may be asked, can the Society expect to find, except in its own ranks, anyone competent for the work? and why should there be any more scruple about employing a Fellow for such a purpose than there is in employing Fellows as printers and engravers?

As to the election of a Librarian, what arrangement could be more objectionable than for the Society at large to elect to an office of this kind? How could testimonials

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be properly weighed by the members generally, or the fitness of candidates in any way tested? And when it is argued that the clerk and housekeeper, as well as the Librarian, ought to be appointed by the Society also, and not by the Council, the acme of absurdity in the matter seems to have been reached.

It is quite evident, from what has been said, that the less a learned Society indulges in legislation the better. What must be called the "opposition" were anxious, at the meeting on Thursday last, for a further revision of the whole laws of the Society; fortunately, however, the common sense of the Fellows was against them. Sir John Lubbock pointed out, at the conclusion of the debate, that none of the speakers had made out even a *primâ facie* case for further change. It may be hoped, therefore, that when the technical question of the legality of the amendments has been disposed of, the Society will enjoy undisturbed peace and quietness.

One practical suggestion seems to educe itself from what has been said. The only way to settle matters of dispute of this kind is to have an authoritative arbitrator. If we ever get a minister to take charge of our scientific institutions, a legal assessor might be conveniently attached to his staff, to act in lieu of a Visitor to the learned Societies which now possess a quasi-official status from being housed at the public expense. If the points which the dissentients raised in the present case could have been authoritatively and impartially settled off-hand, there would have been no need for an important scientific Society to have wasted a considerable portion of its session over matters in themselves of the slenderest possible consequence, and absolutely without importance in a scientific sense.

THE MOON

The Moon Considered as a Planet, a World, and a Satellite. By James Nasmyth, C.E., and James Carpenter, F.R.A.S. With 24 illustrative plates of lunar objects, phenomena, and scenery, and numerous woodcuts. (London: Murray, 1874.)

THE illustrations to this book are so admirable, so far beyond those one generally gets of any celestial phenomenon, that one is tempted to refer to them first of all. No more truthful or striking representations of natural objects than those here presented have ever been laid before his readers by any student of Science; and I may add that, rarely if ever, have equal pains been taken to insure such truthfulness. Mr. Nasmyth, not content with the drawings he has been accumulating for many years, has first translated them into *models*, which, when placed with a strong light shining obliquely upon them, should reproduce the ever-changing lunar effects of light and shadow. Having obtained models which bore this test, he has photographed them with the light falling, now on one side, and now on the other, to represent the sunrise and sunset appearances on our satellite, as observed in the telescope. The result is perfect; far more perfect than any enlargement of photographs could possibly have been, because, by every such enlargement, a softness is brought about, whereas, the more powerful the telescope employed and the more perfect the atmospheric conditions, the more does the unevenness and sharpness of every lunar detail come out.

But, though I have given the first place to a general reference to the illustrations, I by no means intend thereby to imply that the text is of secondary importance. In fact, the more carefully the text is read, the more obvious does it become that Mr. Nasmyth has used his drawings as a means to an end, and that he and Mr. Carpenter between them have produced a work which is not only a very beautiful and a very readable one, but one of some importance. From this point of view it is to be regretted that the book had not been published a month or two later, as then the authors might further have illustrated their subject by a reference to Mr. Mallet's most important paper on volcanic energy, which has just appeared in the "Philosophical Transactions"—a paper which supports the authors' views in many important particulars, and though it clashes with others, if we are not mistaken, a discussion of the question from the two points of view presented will ultimately enable us to carry our conclusions further than they have gone hitherto.

Again, it is not a little curious that another communication presented to the Royal Society not long ago, and not yet published in this country, may also throw new light upon one at least of the interesting points presented to the student of lunar physics. I refer to the working hypothesis on which I have attempted to explain the absence of metalloids from the sun's reversing layer in its bearing upon the moon's atmosphere.

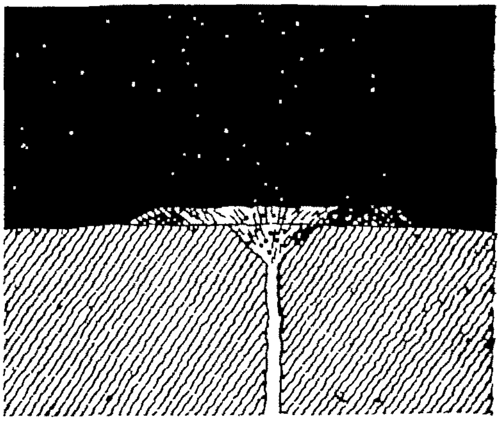

FIG 1.—First stage of a lunar Volcano

Before, however, more detailed reference to these points, it is as well to state briefly, for those less conversant with lunar matters, the principal points in which Selenology differs from Geology, or rather the principal effects which have been produced on the moon in past time which differ from the effects which have been produced on our planet in past time.

First among these is undoubtedly the evidence of volcanic action on a scale far surpassing anything that we have an idea of here. Witness craters 74 miles in diameter, and if the walled plains are accepted as craters, then diameters of craters reaching 300 miles, the volcanic energy not being scattered here and there, but making up the entire surface over large areas.

Next, after the tremendous evidence of vulcanicity afforded by the craters and walled plains, come the bright streaks which have ever been a puzzle to observers. These are seen under various illuminations to radiate from several craters for hundreds of miles. Here I will quote from the book, p. 133:—

"There are several prominent examples of these bright streak systems upon the visible hemisphere of the moon; the focal craters of the most conspicuous are Tycho, Copernicus, Kepler, Aristarchus, Menelaus, and Proclus. Generally, these focal craters have ramparts and interiors distinguished by the same peculiar bright or highly reflective material which shows itself with such remarkable brilliance, especially at full moon; under other conditions of illumination they are not so strikingly visible. At or nearly full moon, the streaks are seen to traverse over plains, mountains, craters, and all asperities; holding their way totally disregardful of every object that happens to lie in their course. The most remarkable bright streak system is that diverging from the great crater Tycho. The streaks that can be easily individualised in this group number more than one hundred, while the courses of some of them may be traced through upwards of six hundred miles from their centre of divergence. Those around Copernicus, although less remarkable in regard to their extent than those diverging from Tycho, are nevertheless in many respects well deserving of careful examination; they are so numerous as utterly to defy attempts to count them, while their intricate reticulation renders any endeavour to delineate their arrangement equally hopeless."

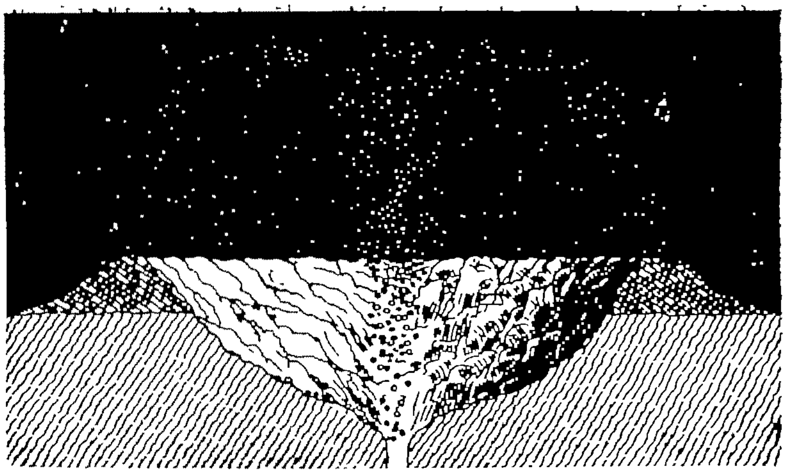

FIG. 2.—Second stage. The crater increases its diameter

Quite different from these radiating streaks are very definite "cracks," some of which are easily seen with moderate telescopic powers. There are, however, a very large number recorded, some of which are excessively delicate objects.

Last among exceptional phenomena to be recorded is the circumstance that our satellite has no atmosphere to speak of: no clouds, no fogs have ever been seen on the moon as on Mars, while no effects have been produced at occultations of stars and planets by the moon such as should be produced did an atmosphere in any way comparable to our own exist on our satellite.

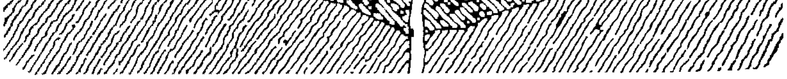

FIG. 3.—Third stage. The central cone is formed by a reduced action

Although the most important part of the text consists of an attempted explanation of all but the atmospheric phenomena, much of it deals, and in a very admirable way, with lunar topography, a perusal of which will be desirable before the discussion is taken up, in the case, at all events, of those unfamiliar with the moon's surface. The consideration of the various questions involved is preceded by chapters on the cosmical origin of our system and the generation of heat, the result of which was that at one time the moon was an incandescent sphere with a cooling crust, and even then if I understand the authors, there was no lunar atmosphere (p. 17), for they give an answer, or think they do, in this sense to the question

"How can a volcanic theory of the lunar phenomena be upheld consistently with the condition that it possesses no atmosphere to support Fire?"

In the chapter on the cooling of the crust (Chap. III.), special attention is directed to observations tending to show that cast-iron and even slag and Vesuvian lava expand on cooling. This will be new to physicists:—

"The broad general principle of the phenomenon here referred to is this: That fusible substances are (with few exceptions) specifically heavier while in their molten condition than in the solidified state, or in other words, that molten matter occupies less space, weight for weight, than the same matter after it has passed from the melted to the solid condition. It follows as an obvious corollary that such substances contract in bulk in fusing or melting, and expand in becoming solid.* It is this expansion upon solidification that now concerns us.

"Water, as is well known, increases in density as it cools, till it reaches the temperature of 39° F., after which, upon a further decrease of temperature, its density begins to decrease, or in other words, its bulk expands, and hence the well-known fact of ice floating in water, and the inconvenient fact of water-pipes bursting in a frost. This action in water is of the utmost importance in the grand economy of nature, and it has been accepted as a marvellous exception to the general law of substances increasing in density (or shrinking) as they decrease in temperature. Water is, however, by no means the exceptional substance that it has been so generally considered. It is a fact perfectly familiar to iron-founders, that when a mass of solid cast-iron is dropped into a pot of molten iron of identical quality, the solid is found to float persistently upon the molten metals—so persistently that when it is intentionally thrust to the bottom of the pot, it rises again the moment the submerging agency is withdrawn" (p. 20).

There will be many for whom this part of the work will possess great interest, but I take it few will accept the startling conclusions drawn from the asserted expansion.

"This expansion of volume which accompanies the solidification of molten matter furnishes a key to the solution of the enigma of volcanic action; and that such theories as depend upon the agency of gases, vapours, or water are at all events untenable with regard to the moon, where no gases, vapour, or water appear to exist" (p. 27).

I will return to this point presently, but meantime let us follow the contracting globe. Messrs. Nasmyth and Carpenter quite accept tangential pressure as being the only true cause of elevation, and its effect is very well put:

"When the molten sub-stratum had burst its confines, ejected its superfluous matter, and produced the resulting volcanic features, it would, after final solidification, resume the normal process of contraction upon cooling, and so retreat or shrink away from the external shell. Let us now consider what would be the result of this. Evidently the external shell or crust would become relatively too large to remain at all points in close contact with the subjacent matter. The consequence of too large a solid shell having to accommodate itself to a shrunken body underneath, is that the skin, so to term the outer stratum of solid matter, becomes shrivelled up into alternate ridges and depressions or wrinkles" (p. 28).

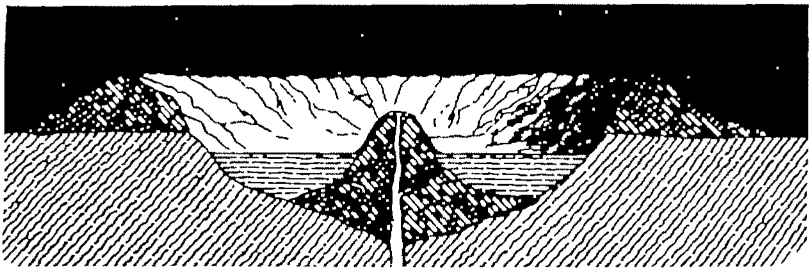
FIG. 4.—Fourth stage. The old crater is nearly filled with lava, leaving the cone visible in the centre

The preceding extracts will give an idea of the authors' view of the general phenomena which accompanied the cooling of the crust. We have no atmosphere, the crust as it cools expands and cracks, and through these cracks the interior liquid is ejected, and finally tangential pressure does the rest.

Now leaving the question of the atmosphere for a time, let us compare this with Mr. Mallet's reasoning. As a planet cools, the crust thickens, and the nucleus contracts; the crust must follow the nucleus, hence tangential pressure and its concomitants, elevation where possible, where not possible then tremendous interior motions, which motions are converted into heat, which heat + water produces volcanic activity. Further, the smaller a globe is, the more rapidly will it cool, and the more marked will the phenomena which accompany cooling be. Hence Mr. Mallet's hypothesis is competent to explain all the extreme development of volcanic activity on the moon by exactly similar causes which we know to have gone on here.

Now as I have said, Mr. Mallet wants water for his volcanoes, both here and on the moon, but Messrs. Nasmyth and Carpenter will not even allow that an atmosphere, still less water, has ever existed there. Now here I unhesitatingly range myself on the side of Mr. Mallet. I believe in an absolute uniformity throughout all Nature in such matters. I do not mean uniformity of matter, so far as chemical materials go, but of manner.

Now what is an atmosphere? or to put the question more specifically, what is our atmosphere? Is it not a residue? We have free oxygen in the atmosphere at the present time; had we not very much more before the various metals which now exist in combination with that metalloid existed in their pure state? Now how has combination been brought about? By exposing the metals to the atmosphere and its contained oxygen. Now suppose the machinery, the function of which in past time has been to bring these metals to the surface, had been a thousand times more powerful, would there be as much oxygen in the air now as there is? A child can answer this question, and it is one of several which might be asked all tending to show that it is as unnecessary as it is unphilosophical to suppose that there never was a lunar atmosphere, because there is only a tenuous one at the best now. I shall not follow this subject out further now because it is part of a larger one, which deals with the atmospheres and densities of all the bodies of our system, and to discuss it at length would lead me too far from the present subject; suffice it to say that the enormous atmospheres of Saturn and Jupiter and the absence of a lunar atmosphere result from one single cause, that cause being if I mistake not the chemical constitution of the exterior parts of the solar nebula as each planet was thrown off, and the subsequent action on each globe.

To come to details. Whether the walled plains are really due to volcanic action or not, our authors offer no opinion beyond referring to the hypothesis of Prof. Dana, as being the most rational. Dana suggested that, as at Kilauea, the lava was simply boiling, and gradually extending its boundaries from a centre, so that at last, if the heat continued, a quasi-crater might be formed of any extent. That the smaller craters are true craters Messrs. Nasmyth and Carpenter take great pains to show, and their evidence is of the closest and most satisfactory kind. The woodcuts which we produce will show how the central cones, which *are scarcely ever absent in the craters we are now discussing*, have originated on the theory advanced; at the same time it is shown how, on this idea, even when the central cone is absent or the crater is filled to the brim, as in the case of Wargentin, one of the most curious of lunar objects, the effects observed may be explained.

Mr. Nasmyth long ago illustrated the bright streaks on the moon by the experiment which he here details, and a very striking one it is. He took a glass globe, filled it with water, and having hermetically sealed it, plunged it into warm water, "the enclosed water expanding at a greater rate than the glass, exerts a disruptive force on the interior surface of the latter, the consequence being that at the point of least resistance the globe is rent by a large number of cracks diverging in every direction from the focus of disruption."

From the photograph, it is clear that the result is strikingly similar to the rays which have Tycho for a focus, and on the strength of this similarity the authors claim this as another effect of expansion due to cooling. I so, however, the experiment with the glass globe is not in point. But however the cracks were produced, they imagine that thus having them travelling along for hundreds of miles irrespective of surface inequality, there was an up-flow of molten matter which spread out on both sides and turned the narrow crack into a broad streak.

I trust I have said enough about this book to induce all interested in physical problems to peruse it for themselves; it is altogether an admirable production, and if space permitted each picture even would merit a special paragraph.

J. NORMAN LOCKYER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Natural Selection and Dysteleology

IN his reply to a criticism which appeared in NATURE, Prof. Struthers alluded to a question of considerable interest to evolutionists, viz., whether or not the presence of useless organs "proves too much for the argument."[*] The difficulty is one often met with, and has been well stated by Prof. Huxley, thus:—"Prof. Haeckel has invented a new and convenient name, 'Dysteleology,' for the study of the 'purposelessnesses' which are observable in living organisms—such as the multitudinous cases of rudimentary and apparently useless structures. I confess, however, that it has often appeared to me that the facts of dysteleology cut two ways. If we assume, as evolutionists in general do, that useless organs atrophy, such cases as the existence of lateral rudiments of toes, in the foot of a horse, place us in a dilemma. For, either these rudiments are of no use to the animal—in which case, considering that the horse has existed in its present form since the pliocene epoch, they surely ought to have disappeared—or they are of some use to the animal, in which case they are of no use as arguments against teleology," &c.[†]

Now it appears to me (as I think it must to all upon adequate consideration), that the dilemma thus presented is only apparent; its first-mentioned horn having no existence. In other words, we can never in any single case be sure that any length of time would have been sufficient to enable natural selection totally to obliterate a useless organ.

Mr. Darwin, in the "Origin" and in the "Variation," has mentioned three causes which operate towards the removal of useless structures. These causes are Selection, Disuse, and Economy of Growth. Recently he has suggested a fourth cause, which may be epitomised as the dwarfing influence of impoverished conditions, progressively reducing the average size of the useless structure, by means of free intercrossing.[‡] I shall endeavour to show that these causes are not sufficient to obtain the complete extinction of rudimentary parts in all cases.

Selection may be considered as applying only to those rare instances in which changed conditions of life may be supposed to have rendered a previously useful organ injurious; for so far as selection operates in obliterating a merely useless organ, it will be more convenient, for the sake of brevity, to identify it with the Economy of Growth.[§] Since, however, it is obvious that an injurious organ must pass through the merely useless stage before it becomes wholly suppressed, we may dismiss this cause without further comment.

Disuse and Economy of Growth are so much entangled in their operation, that it is hopeless to find examples illustrating their separate action. In so far, therefore, as we choose to disentangle them, we must discuss the question in the abstract.

Disuse may be left out of the question, so far as its influence is due to the principle of inheritance at corresponding periods of life; for, as Mr. Darwin tersely observes, "as most organs could be of no use at an early embryonic period, they would not be affected by disuse; consequently they would be preserved at this stage of growth, and would remain as rudiments."[‖] It may,

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[*] NATURE, vol ix p. 83.

[†] "Critiques and Addresses," pp. 307–8.

[‡] NATURE, vol. viii pp. 432 and 505

[§] I think, also, that if the Economy of Growth is really a principle independent of the "more general principle," viz., the *direct* influence of "natural selection in continually trying to economise every part of the organisation" (Compare "Origin," 1873, pp 117–18), it may yet, without any great stretch of inference, be considered as due to the *indirect* influence of natural selection. For, the survival of the fittest demands that each individual shall utilise its stock of nutriment to the best advantage, and, this demand being continued through many generations, it does not seem in itself improbable, that it should thus at last secure to all the members of the surviving species an inherited tendency so to economise their nutritive power when fresh occasion requires—an advantageous innate *temperament* distinct from the external *eliminative* agency of Natural Selection. Only in some such way can we account for the facts of acclimatisation, in those cases where the adaptive changes take place immediately after the transportation of the organism also for an analogous class of facts, such as that of the shells in the same species of Mollusca, differing in their thickness upon the weather and the lee sides of the Plymouth breakwater.

[‖] "Variation," vol ii p 317 I may mention, also in passing, that it seems to me not unopen to question whether disuse is the *principal* cause even of reduction There is no doubt that disuse causes more or less of atrophy in the individual, and from this fact it is argued, that the principle of inheritance at corresponding periods of life must entail the continued reduction of a disused part in the species. Now the only effect of the principle relied on is that of prolonging, as it were, the life of the disused part over many generations—thus affording it an indefinite time to succumb to the conditions which reduced it in the life of the individual. But it is necessary for the validity of the inference that it would so succumb, to show that these conditions are the efficient causes of this reducing process in the one case, as they prove themselves to be in the other Suppose, for instance, that failure of nutrition is the principal cause of atrophy under disuse, does it follow when such failure has done, to all appearance, its utmost during the life of the individual (as we see in disease), that it could do any more were that life indefinitely prolonged? Of course in the case of short-lived animals, where the dwarfing influences may not have time to exhaust themselves in a single generation, the principle of inheritance at corresponding ages may be drawn however, be objected that the doctrine which Mr. Darwin elsewhere inculcates,[*] and deems sufficient to account for the total suppression of rudimentary organs, viz., inheritance at earlier periods of life, is fatal to dysteleology as a prop to evolution—at least in the case of long-lived species. And so it would be, were not this principle of so shadowy an application that, while it is perfectly legitimate to point to it as a possible cause of total suppression in some cases, it would be simply absurd to argue that such must be its effect in all.

We next come to the Economy of Growth. Suppose an organ to become suddenly useless, this principle would at first cause its rapid reduction. In proportion, however, as its presence ceases to be injurious, the arresting process becomes slower and slower, until a point is reached at which it is presumably *nil*. That such a point of rest must somewhere be attained seems evident, if we consider that the smaller the diminishing organ becomes the less is it subject to the influence of the Economy of Growth. In other words, when the organ undergoing reduction becomes so minute relatively to the size of the animal (or, more correctly, to the available store of nutrition), that the supply of nourishment it requires is no longer perceived by the organism at large, it then remains permanently of that size. "The economy of growth will not account for the complete or almost complete obliteration of, for instance, a minute papilla of cellular tissue representing a pistil, or of a microscopically minute nodule of bone representing a tooth;"[†] and, the whole principle being one of relation, it is a question, for instance, whether the rudimentary digits of a horse consume a greater relative amount of nutrition than does the "minute papilla." Besides, without entering into details, I think there is very good reason to believe that the Economy of Growth is unable to reduce an organ which was originally large, to the same absolute size as it can an organ which was originally small. From all this it follows, that if the struggle for existence were in any case so keen as to afford Selection (*i.e.*, Economy) the opportunity of totally obliterating every rudimentary organ, it seems probable that the species itself would require to become extinct.

Turning now to the last of the causes propounded by Mr. Darwin, there can be no doubt that it is (theoretically) sufficient to procure total obliteration. Forasmuch, however, as we can never know in any given case whether or not the requisite conditions have been supplied,—*i.e.* impoverished nutriment for an enormous length of time,—this newly added cause affords no further justification for the old statement, that the theory of Natural Selection fails to account for all the facts of Dysteleology.

The perusal of the last-mentioned thoughtful conception has suggested to me the probable existence of another cause, having a more general application; but as it can never induce complete suppression, I shall reserve it for the subject of another communication.

Mr. Mivart supposes that organs may become suddenly aborted[‡]; but, apart from the weighty objections to this view,[§] there is no case on record, so far as I am aware, of an organ thus becoming *totally* suppressed in any domestic species. A sport of this kind always leaves a rudiment, and it is upon the analogy of such sports alone that Mr. Mivart's argument is founded.

Having now enumerated all the causes ever proposed by evolutionists to account for the reduction of useless parts, it is evident that we should antecedently expect to find innumerable examples of such parts in the condition of rudiments.[‖] Indeed the only difficulty is to account for that final disappearance of organs which must, by any theory of evolution, be postulated to have taken place. The solution is afforded by the exhaustive contemplations of Mr. Darwin, for, whether or not we believe in pangenesis, we cannot but deem it in the highest degree probable that the influence of inheritance is not of unlimited duration. If so, we have at once an adequate cause for the eventual destruction, even in the embryo, of rudimentary parts; but, as it is a cause which would only act after an immense lapse of time, it would have no influence until the original specific type had undergone a considerable modification. Thus, the facts of dysteleology, far from "cutting two ways," afford the strongest confirmation of the natural selection theory; for, as time is thus shown the chief agent in the final destruction of rudiments, and as species are always undergoing change, on the one hand we have an explanation of the fact, that the greater the divergence of the specific type from its original the fewer rudiments do we find of organs characteristic of the latter, while on the other hand, the less such divergence the greater the number of such rudiments—a fact of which the necessary consequence is, that "with species in a state of nature, rudimentary organs are so extremely common that scarcely one can be named which is wholly free from a blemish of this nature."

GEORGE J. ROMANES

The Action of the Heart

HAVING replied to Mr. Garrod's criticism of my "Locomotion of Animals" (NATURE, vol. ix. p. 281), I now wish to show that the explanation given by him of the diastole of the heart is not in accordance with fact.

In a recent number of NATURE (vol. ix. p. 282) I asked Mr. Garrod to explain "how the left ventricle of the heart opens after a vigorous contraction in which all the blood contained in the ventricular cavity is ejected and the ventricle converted into a solid muscular mass, if not by a spontaneous *elongation* of all its fibres." He replies:—"At first sight it might seem that the active dilatation of the heart during the diastole did depend on an inherent power in the muscular fibres of the ventricles to elongate, but the peculiarities of the coronary circulation are quite sufficient to explain the phenomenon without the introduction of so unnecessary a theory as that of Dr. Pettigrew. For in the heart when removed from the body, as in the living body during diastole, the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur" (NATURE, vol. ix. p. 391).

The explanation given by Mr. Garrod of the manner in which the ventricles of the heart open up during the diastole is eminently unsatisfactory. In fact it is no explanation at all. He informs us that the active dilatation of the ventricles is due "*to peculiarities in the coronary circulation*" . . . "for in the heart when removed from the body the injection of fluid into the coronary vessels causes the whole heart to open up from the congestion of the ventricular walls, and so produce the active dilatation which is well known to occur."

The coronary vessels, as everyone is aware, simply supply the blood which nourishes the substance of the heart. There is no peculiarity whatever in the circulation of the blood through them. The blood flows through the coronary vessels in a more or less steady stream as through all the other vessels of the body. In other words the substance of the heart is full of blood during the closure or systole of the ventricles, as well as during the opening or diastole of the ventricles. The presence of the blood, therefore, within the coronary vessels can exert no influence in opening up or actively dilating the ventricles. This is proved by direct experiment. If the heart be cut out of the living body and the coronary vessels divided, the ventricles go on opening and closing with the utmost regularity for protracted periods. Here, however, the power which, according to Mr. Garrod, opens the ventricles is inoperative. The same thing happens when the heart is cut out of the body and the vessels laid open. If, however, the ventricles open and close when the coronary vessels are freely divided, and the blood which is said to distend or open up the ventricles is allowed to escape from the cut surfaces, it is quite clear that the blood pressure within the ventricular walls can exert no influence whatever in producing the diastole. If blood is not admitted into the coronary vessels, or if admitted it is allowed freely to escape, it cannot of course act as a distending medium. Allowing, however, for the sake of argument, that the flow of blood through the vessels of the ventricles occasioned the opening or dilatation of the ventricles, it is evident, for the same reason, that the presence of the blood within the ventricular walls, from the fact that the blood is nearly constant in quantity and virtually incompressible, would prevent the closing or contraction of the ventricles. Mr. Garrod, I opine, is

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upon until the point of such exhaustion is attained; but is it not open to question whether this point can never be reached at all? It must be remembered, too, that in the case of wild species the effects of disease are always associated with other reducing causes, so that here we may easily over-rate the share it has in the work; but in the case of domesticated species the effects of disease are much more isolated (in consequence of Economy of Growth, etc., being, to a great extent, in abeyance), and here we find that atrophy of disused parts, although at first very rapid, eventually does not proceed to nearly so great an extent as it does in the case of wild species. The question thus raised, however, is of no practical importance, since whether or not disuse is the chief *causa* of atrophy in species, there is no doubt that atrophy *accompanies* disuse.

[*] "Variation," vol. ii. p. 80.

[†] "Variation," vol. ii. p. 397.

[‡] "Genesis of Species," 1st ed. p. 103.

[§] See "Origin," pp. 201—204.

[‖] It is unnecessary to consider the *collective* action of these causes, for a moment's reflection will now make it evident that none such exists below the point at which the Economy of Growth ceases to be felt.

here on the horns of a dilemma. He evidently puts the cart before the horse. It is the movements of the heart which determine the movements of the blood, and not the converse.

The cardiac movements are due to a change of shape in the sarcous elements or ultimate particles of the muscular fibres of the heart, and in the adult organ can only be effected by a vital and alternate elongation and shortening of all the fibres composing the heart; the elongation occurring during the diastole and the shortening during the systole. Similar remarks are to be made of the voluntary muscles which, as stated in my work on "Animal Locomotion," are endowed with centrifugal and centripetal movements.

That the opening and closing of the ventricles of the heart are in no way connected with the passage of blood through the substance of the organ, is proved indirectly by the movements of the heart of the embryo. Here the heart opens and closes with time-regulated beat, while yet a mass of cells, and before it contains blood either in its cavities *or in its substance.* But that the presence of blood is not necessary to such movements is placed beyond doubt, for rhythmic movements occur in the vacuoles of certain plants, as *e.g.* the *Volvox globator*, *Gonium pectorale*, and *Chlamydomonas*, where no blood is present.

Lastly, if a frog be slightly curarised and its spinal cord destroyed, it is found, on exposing the heart, that the sinus venosus, vena cava inferior, the auricles and ventricles *are quite destitute of blood*, and yet the organ beats normally and with the utmost regularity. Mr. Garrod has consequently not yet succeeded in answering my query as to how the diastole of the left ventricle is produced He has failed to show that it is not effected by the *active elongation or centrifugal movements of all its fibres.*

J. BELL PETTIGREW

Lakes with two Outfalls

HAVING observed the discussion lately carried on in your pages as to the existence of lakes with two outfalls, I think the following description of such a lake by Prof. Bell, of the Geological Survey of Canada, may be interesting to some of your readers. It occurs on the summit of the high Laurentian country between Lake Superior and Hudson's Bay:—

"In crossing the country from Lake Nipigon to the Albany River, we first followed the Ombabika River to its source, which is in Shoal Lake, three and a half miles long and one mile wide, lying at a distance of twenty-five miles north-east of the mouth of the river. This lake lies due north and south, and discharges both ways, the stream flowing northward towards the Albany, called the Powitik River, being nearly as large as the southern outlet. No portage occurs on the Ombabika for about nine miles before reaching Shoal Lake, nor for nearly five miles beyond its northern outlet; so that we passed the height of land with the greatest possible ease, having had about seventeen miles of uninterrupted canoe navigation, from the time we made the last portage, in going up the southern side, till we came to the first on going down on the northern. Shoal Lake has an elevation of scarcely 300 ft. over Lake Nipigon, or about 1,200 ft. above the sea."—"Report of Progress Geological Survey of Canada for 1871-72," p. 107.

GEORGE M. DAWSON

Montreal, Feb. 19

The Ink of the Cuttle-fish

WITH reference to the interesting account in NATURE, vol. ix. p. 332, of a gigantic Cephalopod captured in American waters, and of a still larger one, which attacked the boat belonging to some fishermen near Newfoundland, by twining its arms round the vessel, and which, having had two of those arms cut off by the fishermen, moved off, "ejecting a large quantity of inky fluid to cover its retreat," I desire to draw attention to an observation respecting this fluid, which I made on the occasion of a visit to the Crystal Palace Aquarium. My friend Mr. Lloyd was good enough to dislodge a cuttle from its place of concealment, and the usual inky discharge followed, as the creature shot across the tank. Mr. Lloyd states in his interesting "Handbook to the Marine Aquarium," "that the ink (which is viscid) does not generally become diffused through the water as writing ink would be, but is suspended in the water in a kind of compact cloud till it gradually settles down, and is dispersed in flakes." Now I quite think, with Mr. Lloyd, that this being the case, it is difficult to perceive how, according to the generally received opinion, its retreat is covered by the ejected cloud. It seems to me more likely that this discharge is to divert the attention of a pursuer—a dog-fish for instance—which would for the moment be startled by the sudden appearance of masses of dark colour in the water, and in the confusion the cuttle makes his escape.

Now that public aquaria are becoming so general in our great towns, it is much to be hoped that this and many other interesting problems in marine zoology may be solved.

Birmingham, Feb. 28 W. R. HUGHES

Transmission of Light in a Squall

ON the Admiralty Pier, Dover, during a "squally" gale, I remarked an occasional jerking or unsteadiness in one of the adjacent lights, say two miles off, to one of the coast-guard's men with whom I was talking at the time.

To him this was a well-known observation in squally weather. At times, he said, two lights could distinctly be seen for a second or so; frequently the shape of the light was changed, by elongation, vertically and horizontally.

The above phenomenon, if not generally known, might be worth noticing and verifying in your excellent paper.

I suppose an explanation is to be found in the different densities of the atmosphere through which a ray of light must pass in rough unsteady weather; the second image being simply the persistence of the one seen immediately before the change in position of the ray by refraction.

JAMES C. INGLIS

DR. LIVINGSTONE AND THE CAMERON EXPEDITION

IN NATURE for Feb. 26, we expressed the desire which we felt, in common with our readers, for information respecting the orders that have been sent to Zanzibar as to the disposal of Dr. Livingstone's body. We now have great pleasure in being able to announce that Lord Derby acted with the promptitude and energy which might be expected from a statesman who has always shown a warm sympathy for the cause of geography. With the concurrence of the family, his Lordship has sent a telegram ordering the body of the illustrious traveller to be sent to England, and we believe that it is to be accompanied by one or two of Livingstone's faithful negro followers.

The melancholy death of Dr. Dillon and the return of Lieut. Murphy, leaves Lieut. Cameron alone, to proceed to Ujiji, to recover the box of papers left there by Livingstone, and to prosecute further geographical exploration. Heavy unforeseen expenses obliged Lieut. Cameron, who has proved himself to be a resolute and observant explorer, to purchase stores at exorbitant rates at Unyanyembe. The necessity for providing for the march of Murphy and Dillon to the coast, with Livingstone's body and most of his followers, is his complete justification for incurring this unauthorised expenditure, and there can be no doubt that the Geographical Society will treat its gallant emissary in a generous and liberal spirit. Cameron has suffered cruelly from fever and ophthalmia, and he is now resolutely pressing onwards in the performance of his work—the Society's work—in the face of greater difficulties than were encountered by any previous expedition. He carries with him our warmest wishes for his success, and the sympathy of every true geographer in England.

ON THE NEW RHINOCEROS AT THE ZOOLOGICAL GARDENS

A GLANCE at our list of additions to the Zoological Gardens during the last week will inform the reader that the Zoological Society has been successful in adding to its already unrivalled collection of specimens of the genus Rhinoceros still another species, which is exhibited for the first time in the Society's collection, and most probably in this country.

It is well known amongst naturalists that in Asia there are to be found two species of Rhinoceros, with a single horn developed on the top of the nose. The larger of these is the gigantic Indian Rhinoceros (*R. unicornis*), many specimens of which have been brought to this country, and a very fine male example of which is living in the Regent's Park Gardens. In it the skin, which is immensely thick, is thrown into massive folds or shields, making the animal appear as if clad in armour-plating. Each shield is thickly studded with nearly circular slightly-raised tubercles, which look very much like the heads of innumerable bolts intended to strengthen and retain the shield in position. The folds that surround the neck, where it joins the head, are very ample, producing the appearance of the now so fashionable ruff, somewhat modified. According to the observations of the late Mr. Edward Blyth, the Indian Rhinoceros is found only at the foot of the Himalayan hills, and in the province of Assam, along the valley of the Brahmapootra.

The second species of one-horned Rhinoceros is generally called the Javan Rhinoceros (*R. sondaicus*). It is found in Java, and in the country stretching from Malacca up through Burmah to Assam. It is considerably smaller than the Indian species; the shields are not so strongly marked, and are not arranged in an exactly similar manner, the gluteal shield not being completely divided into two by a transverse fold situated half-way down it; and the middle neck fold, instead of running backwards on each side before it reaches the spine, crosses the middle line, and so divides off a saddle-shaped shield, which is median, and as deep from before backwards as from side to side. The fold which surrounds the neck is also much less significant, and the head is narrower and less formidable in aspect. The tuberculation of the shields is more slightly marked, and each tubercle is proportionately smaller in diameter.

It is a specimen of this Javan Rhinoceros (*R. sondaicus*), a nearly full-grown male from Java itself, which the Zoological Society has succeeded in purchasing, and which is now exhibited in the same house as the Indian species, so that every opportunity is at last afforded for a more minute study of the differences which will most probably be found to distinguish the two species.

The other species of Asiatic Rhinoceroses, namely, the Sumatran Rhinoceros (*R. sumatranus*), and the Hairy-eared Rhinoceros (*R. lasiotis*), are both two-horned, and have been divided off as a separate genus, that of *Ceratorhinus*, by Dr. J. E. Gray. The skin is not divided into shields, and is thinner than in the one-horned species. The type specimen of the Hairy-eared Rhinoceros, the only example known, is now living in the Zoological Gardens. About a year ago the Sumatran animal was also represented, and rumour says that the gap caused by its loss will not be long unfilled.

NEIL ARNOTT, M.D., F.R.S.

WE have this week to record the death of this well-known man of science, which took place at his residence in Cumberland Terrace, Regent's Park, on the 2nd inst. He was born at Arbroath in May 1788, and had consequently reached his eighty-sixth year.

While Neil was yet young his father died, and the family removed to Aberdeen. Neil went to the Aberdeen Grammar School, being there with Lord Byron, and succeeded so well in the one thing then taught, Latin, that he gained a bursary by a competition in Marischal College, which he entered in 1801. In his third year he came under Patrick Copeland, Professor of Natural Philosophy, renowned for his admirable course of lectures, and especially for his power of experimental illustration. Arnott was one of Copeland's best pupils, and afterwards turned to full account the careful notes that he had made of the lectures.

He began the study of medicine in Aberdeen, and in 1806 he went to London to prosecute the study. Young Arnott, while his medical education was still incomplete, went aboard an Indiaman, as assistant-surgeon, making the usual voyage of a trading East Indiaman in those days. He was the intellect and soul of the ship, associating with everyone that could learn or teach anything; he was the resource in all serious emergencies, of whatever kind.

On his return to England, in 1811, he settled as a medical practitioner in London. He was the chief medical adviser to a colony of French refugees who settled in Camden Town, and also became physician to the French and Spanish Embassies, his fluency in languages serving him in good stead. It was about 1823 that he first turned to account his studies in natural philosophy, by giving in his own house a course of lectures both on the general subject and on its applications to medicine. These lectures formed the basis of the "Physics," the first volume of which appeared in 1827, and gained for the author an instantaneous and wide-spread reputation. The first edition was sold in a week after being reviewed by the *Times*. In a few years five editions were exhausted, and the work was translated into all the languages of Europe. The freshness and popular character of his style recommended the book to the general public, and did not prevent its favourable reception by the highest scientific authorities; Herschel and Whewell both gave emphatic testimonies to its accuracy and originality. The author was thenceforth recognised as a man of science and an inventor of no mean order. His practice as a physician was extended, and he became a Fellow of the Royal Society. On the foundation of the University of London in 1836 he was nominated a member of the Senate, and in 1837 he was named Physician Extraordinary to the Queen.

In 1838 he published a treatise on warming and ventilating, and in this he described the stove since called by his name. He introduced the water-beds, and made many other useful applications of physics to medical and surgical practice. For many years he had withdrawn from medical practice. He had a large circle of friends in and out of the profession. His conversational powers, his large range of scientific knowledge, and his geniality of manner, will be long remembered by those who now regret his loss.

OZONE[*]

II.

SOME of the properties of ozone have already been referred to. At the common temperature of the atmosphere, it may be preserved, if dry, for a very long time in sealed tubes, but by slow degrees it becomes changed again into ordinary oxygen. This conversion goes on more rapidly as the temperature is raised, and at 237° C. it is almost instantaneous ("Phil. Trans." for 1856, p. 12). The alteration of volume which occurs at the same time has been already sufficiently described. A similar effect to that of heat is produced by several oxides, such as the oxide of silver or the peroxide of manganese, which by contact, or, as it is termed, catalytically, instantly change ozone into ordinary oxygen. Ozone is also destroyed by agitation with water, provided the ozone is in a highly diluted state. But the most interesting fact of this kind is one which I have recently observed, and which I hope to be able to exhibit to the Society. Dry ozone, even if present in such quantities as freely to redden iodide of potassium paper, is readily destroyed by agitating it strongly with glass in fine fragments, although, as we have seen, it may be preserved for an almost indefinite period in sealed glass tubes. This experiment, as it appears to me, forms a new and closer link than any hitherto observed between a purely mechanical action and a chemical change.

Ozone is a powerful oxidising agent. It attacks metallic mercury and silver with great energy, and converts them into oxides. The experiment with mercury is very striking, and is a delicate test for ozone, either in the dry or moist state. A few bubbles of oxygen containing not more than $\frac{1}{300}$th part of ozone will alter wholly the physical characters of several pounds of mercury, taking away the lustre and convexity of the metallic surface and causing the mercury to form an adhering mirror to the surface of the glass vessel in which it is contained. If ozone in a diluted state is slowly passed through a tube filled with silver leaf, the metal will be oxidised to the distance of 2 or 3 millimetres, but the oxidation will not proceed further, although the ozone reactions are wholly destroyed. This striking result is due to the catalytic action of the portions of oxide which are first formed. So small is the amount of oxide produced in this case that, in a glass-tube through which many litres of electrolytic ozone had been passed, the increase in weight from the formation of oxide only amounted to a scarcely appreciable fraction of a milligramme.

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[*] An Address delivered before the Royal Society of Edinburgh on December 22, 1873, by Dr. Andrews, LL.D., F.R.S., Honorary Fellow of the Royal Society of Edinburgh. (Continued from p. 349.)

Ozone is absorbed by oil of turpentine, oil of lemon, and other essential oils. These oils have also, like phosphorus, the power of changing oxygen into ozone, while they are slowly oxidising; so that if oil of turpentine is shaken for some time in a flask filled with air or oxygen, the oil will acquire ozone properties.

Ozone decomposes a solution of iodide of potassium, liberating the iodine, which may be discovered by its red colour, or its blue compound with starch. If the action is continued sufficiently long, the free iodine disappears from the formation of iodate of potassium and the solution becomes colourless. Reddened litmus paper moistened with a solution of iodide of potassium is turned blue, when exposed to the action of ozone, in consequence of the caustic alkali formed by the decomposition of the salt. In employing this test it will often be found advantageous to remove the free iodine by washing the paper with strong alcohol. This form of the iodide of potassium test has been proposed by Houzeau for the discovery of ozone in the atmosphere. Ozone produces other reactions of a similar character which it will be sufficient here barely to mention. Paper moistened with sulphate of manganese becomes brown when exposed to this agent from the formation of the hydrated peroxide. Solutions of thallous oxide are in like manner converted into the brown peroxide; the black sulphide of lead into the white sulphate, and the yellow ferrocyanide of potassium into the red salt. The action of ozone on tincture of guaiacum, which it turns blue, was made a subject of special study by Schönbein.

The bleaching properties of ozone are highly characteristic and have attracted a great deal of attention. It deprives indigo of its blue colour, converting it into isatin, and bleaches readily litmus and other vegetable colouring matters. Attempts have been made to apply this property of ozone in the arts, and particularly to the refining of sugar and the bleaching of linen. It has been even stated that these and other applications of ozone, as a decolorising or bleaching agent, have been successful; but the results of my inquiries on this point have, I regret to say, been unfavourable, and it remains yet to be seen whether this singular body can be made subservient to the useful purposes of life. For the preparation of ozone on the large scale from ordinary air, a modification of the tube-generator of Siemens has been proposed by Beanes, and is an efficient and powerful instrument.

I will not detain the Society by an account of the history or properties of the problematical body to which Schonbein gave the name of antozone. He considered this body to be oxygen possessing permanently positive properties, while ozone itself he regarded as negative oxygen. Ordinary or inactive oxygen, according to him, is formed by the union of ozone and antozone. These views have not been supported by recent investigations, which leave little doubt that the antozone of Schonbein is identical with the peroxide of hydrogen of Thénard. From ozone the peroxide of hydrogen can be readily distinguished by the solubility of the latter in water.

Soon after the discovery of ozone, Schonbein having observed that the air of the country frequently coloured a delicate ozone test-paper in the same manner as ozone itself, inferred that ozone is a normal constituent of our atmosphere. He concluded that the amount of this body present in the air is different in different localities, and in the same locality at different times, and with great boldness he attempted to connect its presence or absence with the prevalence or rarity of certain catarrhal affections. A new field for investigation was thus opened up, which has been assiduously cultivated by a large and zealous band of observers. Before referring however to their labours, it will be necessary briefly to allude to the present state of our knowledge regarding the existence of ozone in the atmosphere.

Schonbein always maintained that ozone is a constituent of atmospheric air, and his various papers on this subject alone would, if collected, fill a large volume. In his last memoir he observes that the active substance in the air acts in a parallel manner on iodide of potassium and sub-oxide of thallium papers, although more slowly on the latter; and that the thallium paper, which has been coloured brown by the air, behaves towards reagents in the same manner as that which has been exposed to artificial ozone. From these facts he infers that the active substance in the air is neither peroxide of nitrogen nor sulphuretted hydrogen. He further states that the atmosphere never contains free nitric acid, although nitrate of ammonium in small quantities is frequently present; and that neither chlorine nor bromine can be present in the free state in air, on account of their affinity for hydrogen. Houzeau also maintained that the existence of ozone in the air was proved by the alkaline reaction of iodide of potassium paper, which had been decomposed by exposure to the atmosphere. Although experiments and arguments of this kind were sufficient to give probability to the view that the active substance in the atmosphere which produces these reactions is ozone, they were at the same time far from conclusive, and some of the ablest chemists of Europe accordingly considered the question doubtful, while others attributed the effects observed to the presence of oxidising agents altogether different from ozone. I will only cite on this point the opinion of M. Frémy, whose researches in conjunction with M. Becquerel on ozone have already been referred to. "Without denying," he remarked at a meeting of the Academy of Sciences in 1865, "the importance of the indications given by the paper of M. Schonbein, or by that of M. Houzeau, I do not find that these reactions demonstrate with sufficient certainty the existence of atmospheric ozone. I am of opinion that the presence of ozone in the air must be established anew by incontestable experiments"

In 1867 I made a set of experiments which I had contemplated some years before for the purpose, if possible, of finally settling this important question. The method I proposed was to ascertain whether, in addition to the power of decomposing solutions of iodide of potassium and of certain other salts, the active body in the atmosphere possessed the other properties of ozone, some of which are highly distinctive. The inquiry was a delicate one, in consequence of the very minute quantity of the active body which is present, even under the most favourable conditions, in atmospheric air. The results of this investigation are given in a short note which was published in the "Proceedings of the Royal Society" for 1867. (1) By passing a stream of atmospheric air, which gave the usual reaction with iodide of potassium paper, for some hours over the surface of mercury in a U-tube, the metal was distinctly oxidised. (2.) The ozone reactions disappeared when the air was passed through a tube containing pellets of dry oxide of manganese. The experiment was continued till 80 litres of air had traversed the manganese tube without producing the slightest discoloration of a delicate test-paper. (3.) But the crucial experiment was to ascertain whether the active body in the air loses its characteristic properties, or is destroyed, at the same temperature (237° C.) as ozone. To determine this point, a stream of atmospheric air, which gave strong ozone reactions, was passed through a globular glass vessel (Fig. 5), covered with wire gauze, of 5 litres capacity, and afterwards through a U-tube 1 metre in length, whose sides were moistened internally with water, while the tube itself was kept cool by being immersed in a vessel of cold water. After traversing the globular vessel and the moistened U-tube, the air was blown over a slip of delicate test-paper, in order to ascertain the presence or absence of ozone. When the atmospheric air was drawn through this apparatus at a uniform rate by means of an aspirator raised by clockwork, the iodide of potassium paper was distinctly reddened in two or three minutes, provided no heat was applied to the glass globe. But on heating the air as it passed through the globe, to a temperature of about 260° C., not the slightest action was produced on the paper, however long the current of air continued to pass. On the other hand, when air free from ozone, but containing traces of chlorine or of the higher oxides of nitrogen, was drawn through the apparatus, the test-papers were equally affected, whether the globe was heated or not. These experiments have since been successfully repeated by Dr. C. Fox.

The identity of the active body in the atmosphere with ozone we may now assume to be established beyond dispute, and the accuracy of Schonbein's views on this subject to be fully confirmed. To determine, however, the actual amount of ozone in the atmosphere is a problem of surpassing difficulty, on account of the extremely small proportion in which it exists, even when at a maximum. Its presence can be easily discovered by any of the ordinary iodised starch-papers, or even more readily by white bibulous paper which has been moistened with a dilute solution of iodide of potassium, and allowed to dry spontaneously in a dark room. If a slip of this paper is exposed for five minutes to a current of air, which will be often supplied by the wind, or may be produced by walking briskly, it will be found to have acquired a delicate red tint, if ozone be present even in the smallest quantities. The tint will be best observed by comparing the slip after exposure with another slip of the same paper which has not been exposed. The action of the diffused light of day on the paper is rarely perceptible after so short an exposure, but this source of error can be easily avoided by enclosing the paper in a hollow cylinder of wood.

Although with the experimental resources now at our command, we can scarcely venture even to estimate the actual amount of ozone at any time present in the atmosphere, yet it may be possible, as Schonbein long ago proposed, by applying a chromatic scale to the indications of the test-papers, to ascertain approximately its relative amount in different localities, and its variations in the same locality. Such estimates must, however, be most uncertain, since the shades of colour produced on test-paper hardly admit of being defined by numbers; and in this particular case they are liable to a special source of error, as there can be little doubt that a large but unknown part of the ozone in the air which comes into contact with the paper is catalytically destroyed, and produces no chemical effect whatever. At the same time the ozonometer, especially when used with an aspirator, does unquestionably give indications of value regarding the ozone states of the atmosphere, and till more accurate methods are devised these observations ought certainly to be continued.

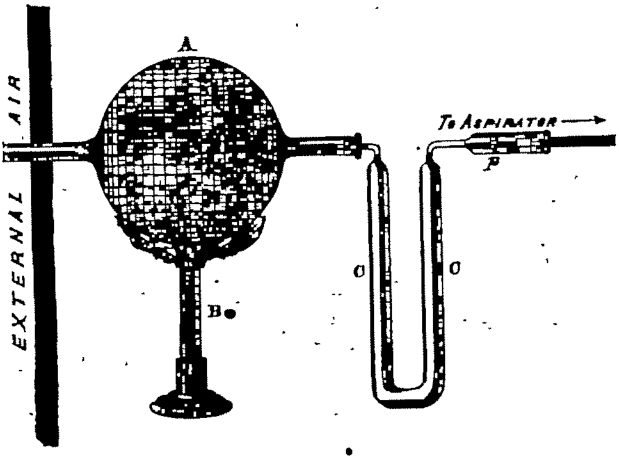

Fig. 5.

Ozone is rarely found in the air of large towns, unless in a suburb when the wind is blowing from the country; and it is only under the rarest and most exceptional conditions that it is found in the air of the largest and best ventilated apartments. It is, in fact, rapidly destroyed by smoke and other impurities which are present in the air of localities where large bodies of men have fixed their habitation, and I have often observed this destructive action extending to a distance of one or two miles from a manufacturing town, even in fine and bright weather.

Ozone is rarely, if ever, absent in fine weather from the air of the country, and it is more abundant, on the whole, in the air of the mountain than of the plain. It is also said to occur in larger quantity near the sea than in inland districts. It has been found to an unusual amount after thunderstorms—a fact which is favourable to the view that the presence of ozone in the atmosphere is due to the action of the free electricity of the latter on the oxygen of the air. The amount of ozone in the air is greater, according to some observers, in winter than in summer, in spring than in autumn; according to others, it is greater in spring and summer than in autumn and winter. As regards the influence of day and night, the observations do not all tell the same tale. Ozone has usually been found more abundantly in the air at night than by day, but some careful observers have found the reverse of this statement to be true.

Schönbein was the first who attempted to connect the fluctuations of atmospheric ozone with the prevalence or absence of epidemic disease; and since this suggestion was first published, numerous observations have been made in different countries with the view of ascertaining whether there is really any connection between the indications of the ozonometer and the health of a district. It has been asserted, for example, as the result of observation, that an outbreak of cholera is accompanied by a marked diminution of atmospheric ozone; but this statement has been disproved by later and more trustworthy observations. On the whole, I think it may be safely asserted that no connection has yet been proved to exist between the amount of ozone in the atmosphere and the occurrence of epidemic or other forms of disease.

The permanent absence of ozone from the air of a locality may, however, be regarded as a proof that we are breathing, if I may venture to use the phrase, adulterated air. Its absence from the air of towns, and of large rooms, even in the country, is probably the chief cause of the difference which every one feels when he breathes the air of a town, or of an apartment however spacious, and afterwards inhales the fresh or ozone-containing air of the open country. It is, indeed, highly probable that many of the most important actions, by which the products of vegetable and animal waste are removed by oxidation from the air, are due to the action of ozone, and could not be effected by ordinary or inactive oxygen. If the amount of ozone in the atmosphere appear too small to produce such large results, we must remember that, from its powerful affinities, ozone is being continually used up, and must, therefore, be constantly renewed.

The physiological action of ozone on the animal system is a subject of interest, and I am able to state the general results of two independent inquiries—one conducted a few years ago, by Dr. Redfern, in Queen's College, Belfast, the other recently communicated to this Society by Mr. Dewar and Dr. McKendrick. Dr. Redfern's experiments have not been published, but he has kindly supplied me with the following note on the subject:—"The general results," he says, "I obtained from about forty experiments conducted from May to September, 1857, to find the effects of oxygen and ozone on different animals, are as follows. The respiration for a very short time of oxygen, containing about $\frac{1}{240}$th part of ozone, is certainly fatal to all animals. The same gas, when passed over peroxide of manganese and freed from ozone, is comparatively harmless, even when respired for long periods. Respiration of such a mixture of ozone for thirty seconds kills small animals, some dying after respiring it only fifteen seconds, whilst similar animals will live in good health for months after respiring oxygen alone for thirty-seven hours, the carbonic acid being removed during the experiment. Death is not due to the closure of the glottis, for it occurs when a large opening has been made in the trachea. Ozone causes death by producing intense congestion of the lungs with emphysema, and distention of the right side of the heart with fluid or coagulated blood, frequently attended by convulsions. If ozone be respired in a dilute form, the animals become drowsy and die quietly from coma, the condition of the lungs and heart being the same, except that the emphysema is less marked. Animals which have respired oxygen for more than twelve hours will now and then die suddenly from the formation of coagula in the heart, even after they have appeared in good health for some days."

The following are the conclusions which Mr. Dewar and Dr. McKendrick have deduced from their researches. Inhalation of an atmosphere highly charged with ozone diminishes the number of respirations per minute, and reduces the cardiac pulsations in strength, the temperature of the animal being at the same time lowered from 3° to 5° C. After death the blood is found to be in a venous condition. Neither the capillary circulation nor the reflex activity of the spinal cord is appreciably affected. The same remark applies to the contractility and work-power of the muscles. Ozone acts on the coloured and colourless corpuscles of the frog like carbonic acid. Ciliary action is not affected by ozonised air or oxygen, but if the layer of liquid be very thin, the cilia are readily destroyed.

The thermal changes which accompany many of the reactions of ozone are well marked, and their investigation, which has been undertaken by Mr. Dewar, promises to yield a valuable addition to our thermo-chemical knowledge.

THE COMMON FROG[*]

XI.

THE eye of the frog is a beautiful and brilliant object, and relatively large. It is furnished with two eyelids, but, unlike those of man, it is the inferior one which is the more movable. In addition to these it is defended by a third eyelid, called the victitating eyelid, which is similar to that one which may be seen (if watched for) so frequently and rapidly to cross the eye of birds, *e.g.* of a hawk.

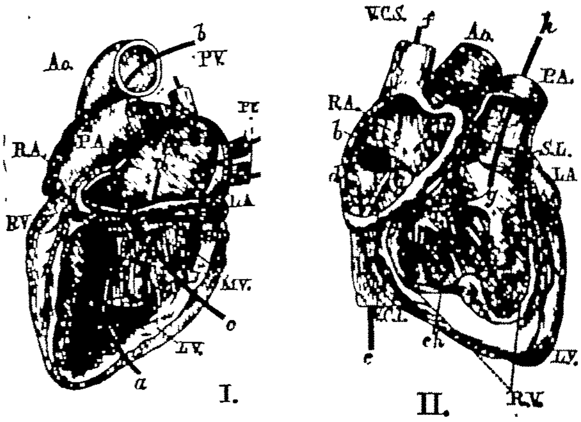

FIG. 74.—I. The left side; and II. the right side of the Heart dissected. I.—*LA*, the left auricle, *PV*, the four pulmonary veins; *cd*, a style passed through the auriculo-ventricular aperture; *MV*, the mitral valve; *ab*, a style passed through the left ventricle into the aorta; *RA*, *RV*, parts of the right side of the heart; *PA*, pulmonary artery.
II.—*RA*, the right auricle, *VCS*, superior vena cava; *VCI*, inferior vena cava, the styles *fe*, *cd*, being passed through them into the auricle; *ab*, style passed through the auriculo-ventricular aperture, *TV*, tricuspid valve; *RV*, right ventricle, *SL*, semi-lunar valves at the base of *PA*, the pulmonary artery, through which the style *gh* is passed; *LA*, *LV*, auricle and ventricle of the left side of the heart.

This structure, however, is no mark of affinity to birds, as it is one which reappears, when wanted, in widely different forms. Thus we find it in the whale, *i.e.* in the highest class of the Vertebrate sub-kingdom, and in certain sharks, *i.e.* in the lowest class of the same.

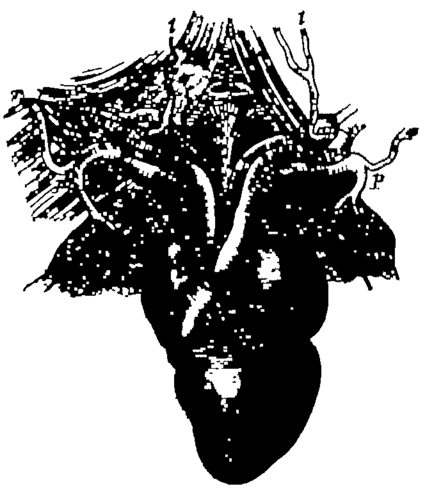

FIG. 75.—The Frog's Heart. The ventricle is below *s*, the aortic bulb is on the left of *s*, and ends in six aortic trunks, three on each side. The first of these (1), ends in the carotid gland (*a*), whence spring the lingual (*l*), and the carotid (*c*), arteries. The second trunk (*s*), is the root of the great dorsal aorta. The third trunk (3), ends in the pulmo-cutaneous artery (*r*), and the pulmonary artery (*p*), which is shown sending ramifications over each lung.

Eyelids do not exist in all members of the frog's class. Even in its order they are extremely minute, in *Pipa* and *Dactylethra*, which have very small eyes. In *Amphiuma* they are completely wanting, and in *Proteus* and in the *Ophiomorpha* the minute eyeballs are covered with the ordinary and unchanged skin of the head.

The ear of the frog's class presents us with the incipient condition of that part as an organ destined to respond to sonorous vibrations conveyed to it by the atmosphere.

In man the internal ear (enclosed in the densest bone of the skull, named, from its density, "petrous") is a very complex organ. The aperture, surrounded by the folds of the external ear, leads by a canal towards a cavity called the tympanic cavity, which cavity is shut off from the exterior by the tympanic membrane (or drum of the ear), which stretches across the canal at a considerable distance from its external aperture. On the inner side of the tympanic cavity lie the convoluted tubes (richly supplied with nerves) which constitute the real organ of hearing or internal ear.

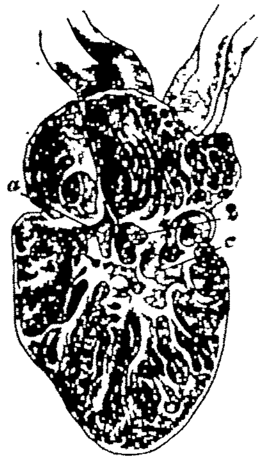

FIG. 76.—Section of heart. *a* and *b*, openings of the auricles into the ventricle; *c*, opening of the aortic bulb into the ventricle.

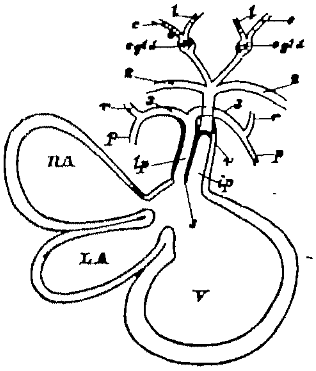

FIG. 77.—Diagram of section of Frog's heart. *LA*, left auricle; *RA*, right auricle; *V*, ventricle, *s*, movable septum dividing the left aortic passage *lp* from the right aortic passage *rp*; *v*, valve; 3, 3, aortic trunks leading to *p*, pulmonary artery and *r*, cutaneous respiratory artery, *s*, *s*, aortic trunks going to form the great dorsal aorta; *cgl*, carotid gland interrupting the flow of blood into *l*, the lingual artery, and *c*, the carotid artery.

Although the tympanic cavity is shut off from the exterior by the tympanum, it nevertheless is not altogether shut off from the exterior, since it communicates with the back of the mouth by a long and narrow canal termed the Eustachian tube.

It is the existence of these Eustachian openings into the ear from the mouth which causes people when intently listening to keep their mouth slightly open.

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[*] Continued from p. 307.

In the frog there is no such external canal, but the tympanum is plainly to be seen in the way already described, on the side of the head, covered only by a slightly striated portion of the skin of the body. The Eustachian tube, however, still exists in the frog, though it is short and wide, and the opening of each is to be seen on one side of the back of the mouth.

This condition of things, however, does not exist in all the members of the frog's order, still less of his class. But in *Proteus*, *Siren*, and *Menobranchus* there is no tympanic cavity whatever, and the organ of hearing is simply imbedded in the skull, and probably responds but to sonorous vibrations conveyed to it by the denser aquatic medium, and not at all, or but very imperfectly, to those of the atmosphere.

In the ordinary efts we still meet with an Eustachian canal, but the tympanum is absent.

In the frog's own order, as in *Pelobates* and *Bombinator*, we may fail to find any tympanum, while the Eustachian tubes are all but obliterated, being reduced to the most minute dimensions.

Another condition, however, may be presented which offers a singular contrast, and is the more remarkable from the widely separated geographical situations of the forms which present it. In the South American *Pipa*, as well as in the South African *Dactylethra*, the two Eustachian tubes run together and open at the back of the mouth, by a median and common aperture.

Strange to say, this is the very condition which exists in birds, though most certainly it cannot be taken as any sign of affinity. In the crocodile these tubes have also a common median opening, but, unlike birds, each tube has also its own lateral opening into the throat, so that there come to be three Eustachian openings.

Can the resemblance between *Pipa* and *Dactylethra* in this matter be taken as a serious indication of genetic affinity, in spite of the wide, deep, and probably ancient Atlantic which rolls between the two species now?

This is a question which cannot be confidently answered, seeing in how many other instances structural peculiarities have evidently had an independent origin. Nevertheless, the fact that these two genera agree also in the small size of the eyes, rudimentary eyelids, and vastly expanded sacral transverse process would seem to point to some ancestral and fundamental relationship. If so, however, it is remarkable that no other such forms, or no intermediate ones should have been preserved, seeing that neither kind can be suspected of having migrated to its own habitat from the existing habitat of the other; and therefore that forms similar to that from which we may, if we please, conceive both to have been derived must have had a more or less widely extended geographical distribution and have been numerous in order to have given origin to genera in many respects so different as the two in question.

The Circulation of the Frog

Not only every animal, but every living being, requires, in order to carry on the functions of life, to interchange some of the gaseous elements of its body with gases of the medium (air or water) in which it happens to live.

Another function of extreme generality is that of conveying to all the parts and organs of the body nutritious matter for their growth or for the repair of those destructive effects which the processes of life inevitably produce in them.

In all members of the highest sub-kingdom of animals (*i.e.* in all Vertebrata) these processes of gaseous interchange and nutrition are effected by means of closed vessels, along which the stream of nutritious fluid (the blood) is continually carried in a definite and constant course. During some or other part of that course the blood becomes exposed to conditions specially favourable to the gaseous interchange, the blood parting with carbonic acid gas and obtaining in its place an increased supply of oxygen.

This process of blood oxygenation is termed respiration, and the organs which subserve it are termed respiratory or breathing organs. Such organs in man are the lungs. The central organ of circulation in man is, as all know, the heart, which is a muscular organ, divided into four chambers, or cavities.

These chambers are called "auricles" and "ventricles," and there are two of each—there being an auricle and a ventricle on the right side and also on the left.

Blood that has performed its nutritive functions, and is therefore charged with carbonic acid gas, is called venous blood, and is conveyed by the veins to the right auricle, whence it passes into the right ventricle, which sends it to the lungs for purification.

The oxygenated, or arterial blood, is returned from the lungs to the left auricle, and hence it is directly transmitted to the left ventricle, whence it is driven through the great artery (the *aorta*) into other arteries, and so distributed all over the body. The aorta passes downwards in front of the backbone, when it is called the *descending aorta*. Before turning downwards, however, it gives off great arteries to the arms and head, the *carotid arteries* carrying blood to the latter.

Now it is very important that the blood should not proceed in a direction the reverse of that indicated, and to prevent such misdirection, or regurgitation, special valves are placed at different openings; these valves freely allowing the blood to flow in the proper direction, but instantly opposing an effectual obstacle to a contrary flux.

The openings of the auricles into the ventricles are guarded by valves, as also is the opening of the left ventricle into the aorta, and that of the right ventricle into the artery going to the lungs.

The valve which guards the entrance into the right ventricle is called *tricuspid*, and consists of three flaps attached by delicate tendinous cords in such a way as to hinder the tending backwards of the flaps into the right auricle, and so allowing the blood to flow back into that chamber.

The valve which guards the entrance into the left ventricle is called *mitral* (from a fancied resemblance to a bishop's mitre), and consists of two flaps. The aperture leading from the left ventricle to the aorta is guarded by three crescentric flaps—called the "*semilunar*" valves of the aorta.

In man the whole of the blood is sent to the lungs for purification during each circuit of this most important fluid, and every organ is supplied with oxygenated blood.

If in any animals the process of purification is incomplete it is manifestly desirable that these organs of the body, the functions of which are the most important, should be supplied with that part of the blood which is pure. This consideration eminently applies to the brain, the director and controller of the entire body.

Now all birds and beasts without exception, share with man this perfect aëration of the entire blood, the whole of the blood in the classes *Mammalia* and *Aves* being purified in the lungs before being distributed to the body.

The conditions by which the frog, at the various stages of its existence, oxygenates its blood and directs the purified stream in the most desirable manner, are curious and instructive.

It is generally known that the lower air-breathing Vertebrates (Reptiles and Batrachians) have the heart less completely divided than in the higher classes, so that the oxygenated (or arterial) blood and the unoxygenated (or venous) blood become mixed in the single or imperfectly divided ventricle.

It might well be supposed, and in fact has generally been so, that in animals with a heart so imperfectly divided, the blood sent to the lungs would be necessarily a mixture of venous and arterial fluid, and similarly that the blood distributed by it to all the organs and parts of the body is alike a mixture of pure and impure fluid.

In fact, however, this is by no means the case, and in the frog, in spite of the reception into a single chamber of both venous blood from the body, and of arterial blood from the lungs, special mechanical arrangements effect such a definite distribution of the two sorts of blood, that the unoxygenated fluid from the body is sent to the purifying respiratory surfaces (lungs and skin), and that the pure oxygenated blood alone goes to the head and to the brain.

For the detection of this beautiful mechanism, we are indebted to the careful investigations of Ernst Brücke.*

The heart of the frog consists of a right and left auricle (divided by a delicate septum), both opening into a single ventricle. From the latter proceeds an aortic root (bulbus aortæ) which gives rise to three arterial trunks on each side.

The first of these, or carotid trunk (1), ends in an enlargement (*a*) termed the carotid gland, of spongy structure, which gives rise to two arteries, one the lingual (*l*), the other (*c*), the carotid which goes to the head and brain.

The second, or systemic trunk (2) meets its fellow of the opposite side beneath the spine, and thence passes backwards as the great dorsal (in man descending) aorta, giving off arteries to all parts of the body.

The third, or pulmo-cutaneous trunk (3) ends by dividing into two arteries. The anterior of these (*r*) goes to the skin (which, as we have seen, is in the Frog an important agent in respiration), the posterior one (*p*) goes to the lungs.

The heart itself is of a more or less spongy texture, but the main cavity of the single ventricles open at its extreme right into that of the aortic bulb (*c*). In close proximity to the opening are the openings from the right (*b*) and the left (*a*) auricles respectively.

The aortic bulb is constitutionally divided by a movable septum (Fig. 77, *s*) in such a way, that the passage on the right side of it leads to the carotid and systemic arterial trunks, while the passage on the left side of it leads to the third pair of trunks—namely, those ending in the pulmonary and cutaneous arteries; moreover, there is a valve in the first of these two passages which tends to retard the flow of blood (*v*).

The consequences of these arrangements are as follows:—

When the auricles contract, the venous blood from the right auricle (*RA*) is sent into both right and left passages of the bulb, but by the action of the valve (*v*), and by the structure of the carotid gland, the blood is checked on the right side (*ip*), while on the left it runs freely into the pulmo-cutaneous trunks (*r* and *p*), and thus the respiratory structures receive unmixed venous blood for purification.

As the lungs get gorged with blood, the resistance on the two sides of the septum of the bulb becomes at first equalised and soon becomes the greater on the left side; then the septum is forced over to the left, and the blood, now mixed with pure blood, flowing in from the left auricle, flows freely along the systemic arteries (2 and 2). The obstruction of the carotid glands (*c gld*) being the greatest and the last to be overcome, the carotid and lingual arteries (*c* and *l*) receive the very last of the blood—that, namely, which coming from the left auricle is purely arterial—and in this way oxygenated blood only is supplied to the head, sense organs, and brain.

It should be borne in mind that in order to develop this most beautiful and complex apparatus, the co-ordinate development in due proportion of these beneficial obstructions and checks must have been simultaneously effected in order that their purpose should be duly served. In other words, to account for its formation by an indefinite series of minute happy accidents would seem to require such a successive occurrence of coincidences as to become an improbability so great as to be indistinguishable from impossibility.

St. George Mivart

(*To be continued.*)

THE "CHALLENGER" EXPEDITION

Bermuda

FROM the two visits made by the *Challenger* to Bermuda we learn a good deal about the vegetation of that island. Along the coast, which in some parts is irregular and rocky, and in others of a sandy nature, frequently with heaps of drifted sand, may be seen in abundance a species of *Borrichia*, a low shrub belonging to the compositeæ, *B. arborescens* D.C. being common in the West Indian Islands, and noted for having both glabrous and silvery leaves on the same plant, as well as the two forms on separate plants. In close proximity to the *Borrichia* was seen *Tournefortia gnaphalodes* R.Br., a Boragineous shrub from 2 to 6 feet high, with white flowers and downy leaves, and *Ipomœa pes-capræ* Sw. with its long stem, which frequently creeps to 100 feet or more, and its purple flowers. In the crevices of the rocks grow *Euphorbia glabrata* V., a shrubby glabrous plant common to the West Indies, and on the shores of Florida, Honduras, &c. A species of Tamarix is also abundant, as well as *Conocarpus erectus* L., and *Coccoloba uvifera* Jacq., known in the West Indies as the seaside grape, from the violet-coloured, pulpy acid-flavoured peiranth; an astringent extract like kino is likewise prepared from the bark, and the bark itself is used for tanning leather.

Many trailing plants scramble about on the sand dunes, assisting to bind the loose sand together. Amongst the most important of these is a hard, prickly grass, probably a species of *Cenchrus*, *Cakile æqualis* L'Her, a singular cruciferous plant allied to our Sea Rocket, and a species of *Scævola*. The Mangrove (*Rhizophora mangle* L.) occurs in swamps similar to those which have been so often described by travellers; but beside the true mangrove swamps, there are others occupied by trees of *Avicennia*, *A. nitida* Jacq. being known in the West Indies as the black, or olive mangrove.

In the peat bogs, or marshes, which are surrounded by low ranges of hills, the most striking character of the vegetation is the ferns; species of *Osmunda* are abundant, as well as *Pteris aquilina* L. Some of the marshes, however, have their special character of fern vegetation some species, as for instance *Acrostichum aureum* L. (*Chrysodium vulgare* Fee), being found only in particular spots. The Junipers (*Juniperus bermudiana* Lun.) also thrive in the marshes, but none of the trees at present standing approach in size those that are occasionally found below the surface. These large trunks usually lie at a depth of about two feet. The average diameter of the trunks of existing trees may be taken at from two to three feet, and these are mostly unsound in the centre owing to the marshy ground in which they grow. The largest known living trees in the island measure respectively fifty-nine inches and thirty-nine inches in diameter; the first is hollow, but the second is apparently sound. Amongst other noticeable marsh plants are *Myrica cerifera* L., a shrub the berries of which, in Central America, yield wax from which candles are made, and *Rhus toxicodendron* L., the Poison Oak of North America.

In the fresh-water ponds or lakes inland, some of which are a quarter-mile long, and often are in close contiguity to a peaty marsh, though the waters appear not to be affected by the peat but are said to be salt at certain periods, occur abundance of confervæ and minute algæ, as well as a species of *Ruppia*. In the shady damp hollows, at the entrances of the caves, is usually seen a rich growth of ferns, jessamine, and coffee trees of good size.

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* "Beiträge zur vergleichenden Anatomie und Physiologie der Gefäss-Systemes." In the third volume of the "Denkschriften der Mathematisch-Naturwissenschaftlichen classe der Kaiserlichen Akademie der Wissenschaften." Vienna: 1852.

The general features of the indigenous vegetation of the islands are the Junipers, *Lantana camara* L., a verbenaceous shrub which grows in dense masses, and the Oleander, which also grows in abundance and is used for hedges. A few trees of the Date and Cocoa-nut palms may occasionally be seen, but their fruit produce is not sufficiently abundant to be of any importance. One of the greatest pests in the island in the form of a weed is *Leucæna glauca* Bth., which sends down its tap roots to a great depth, and is difficult to eradicate. It is a leguminous plant, and in its native state forms an ornamental tree.

The least cultivated part of the island is at Paynter's Vale, where orange and lemon-trees luxuriate in their wild state. From the prevailing dampness of the atmosphere all over the island, a species of Nostoc abounds not only in the caves and on the rocks near the seashore, but also amongst the roots of grass on lawns. Out of about 160 flowering plants collected in Bermuda *Morus rubra*, *Hibiscus arborea*, and *Chrysophyllum cainito* are the only three that do not occur in an absolutely wild state. Perhaps not more than 100 are true Bermuda plants. Many of the plants of the island were no doubt originally brought from the West Indies by the Gulf Stream, or the cyclones. The presence of American plants is perhaps to be traced more to the migrations of birds, which come in large numbers, more especially the American Golden Plover. Then, again, to account for the presence of other plants, there is the fact of the annual importation of large quantities of hay, and also of seeds, such as onion seed from Madeira and potato seed from America, with which other seeds are, no doubt, constantly introduced. Shipwrecks, also, which occur on the coast, are probably fruitful sources from whence new plants arise; as a proof of this, it is stated that a vessel with a cargo of grapes was recently wrecked and the boxes of grapes washed ashore, the seeds of which, being saved, were sown, and produced an abundance of young plants.

INDUSTRIAL CHEMISTRY

THE Society of Arts seems to be increasing its efficiency every year, "lengthening her cords and strengthening her stakes;" quite recently a Chemical Section has been added, which we believe will be productive of much practical benefit. At the opening of this Section on the 6th inst., the chairman, Dr. Odling, gave a valuable and interesting address, which, by the courtesy of the secretary of the Society, we are able to present to our readers:—

I have been desired by the Council to say a few words at this introductory meeting on the importance of Industrial Chemistry, but really to do so is to urge upon you a theme which requires no advocacy, I should think, on the part of anyone, and I am afraid it would be as tedious as thrice-told tales. If we look at the objects with which we are surrounded and consider how very few of them are in a state in which they are presented to us by nature, we shall find that the metamorphoses to which they have been subjected are essentially chemical ones; that is to say, wherever we find one kind of matter in nature, and somehow or other the matter is turned into another kind of matter, we submit it to a chemical change; and how very few indeed of the different kinds of matter with which we are surrounded are really in their primitive forms. When we have mentioned wood and stone, I mean building stone, we have mentioned almost all. When we consider the gas which, though now gas, was a short time ago in the form of coal, or the glass of our windows which a short time back was in the form of sand, soda, and limestone, or if we look at the plaster of our rooms, which was originally limestone, which has undergone varied metamorphoses, and more particularly I might direct your attention to the metallurgical industries, especially iron, which was a short time before in the ironstone—all these are instances of the chemical metamorphosis to which we subject the different natural objects, and so change one kind of matter into another.

Among all these metamorphoses which are of a chemical nature there are some to which we more particularly apply the name of chemical manufactures. In reality, a brick is as much a product of chemical change; it was not originally the same matter it now is, but was produced by a change of chemical composition of its elements. But among these more particularly called chemical manufactures, the production of which is conducted in works which are called chemical works, are those performed in so-called alkali works; and I think I need have no hesitation in saying that these works have proceeded to a far greater development in this country than in any other, notwithstanding the fact that among the constituents received and metamorphosed by these works are many which are of foreign extraction, more particularly the pyrites, or other sources of sulphur, and the manganese or other sources indirectly of the chlorine manufactured at these works. And we see, that in the course of lectures which has been provided for us, three have reference especially to these manufactures, which are conducted exclusively at works which are denominated chemical works. We have a process for the manufacture of soda by Mr. Vincent; another on pyrites, as a source of sulphur, copper, and iron, by Dr. Wright; and another on the manufacture of chlorine, by Mr. Weldon.

Starting from the crude substances, coal and limestone, and pyrites and common salt, we have a production of soda which will be treated of more particularly in Mr. Vincent's address. Then we have the further manufacture of copper, sulphur, iron, and chlorine, which are the necessary economical concomitants. It is indeed remarkable, at the present day, how much the production of chemical manufactures takes in the working up of what were formerly waste products. Perhaps we could not have a more singular instance of this than in the utilisation to which that class of refuse, which was formerly known as burnt pyrites, is now put. Not only do we obtain from the original pyrites sulphur in a form which was formerly thrown away on a very large scale, but, moreover, copper and iron, which were also formerly thrown away in the burnt pyrites. And we have also one very remarkable product now obtained from pyrites on a comparatively large scale, and I may say, with regard to the manufacture of copper from pyrites, that the amount now produced—as Mr. Wright will tell you—from a material which was formerly thrown away, constitutes a very large proportion of the entire quantity now manufactured in the United Kingdom.

But in addition to that there is a very considerable manufacture of silver now going on also extracted from these waste pyrites. This extraction of silver from these pyrites, in which it occurs in an exceedingly minute proportion, has an essential interest for chemists in this point of view, that the processes which are adopted for its extraction really resemble most closely the processes which purely scientific chemists adopt in the laboratory. The pyrites are first of all heated with common salt, whereby the copper is converted into chloride of copper soluble in water, and the silver into the state of chloride of silver, which is soluble in the common salt solution; and not only so, but in this process of removing the soluble copper and the soluble silver from these pyrites, the arsenic and the sulphur, which formerly prevented the burnt pyrites being put to any use, are got rid of, so that what remains is useful in a further stage of the iron manufacture. But with regard to the extraction of the silver, we find how important a knowledge of even delicate chemical processes is, in order to allow the extraction to be pursued with advantage. By the ingenious process of Mr. Clandet and Mr. Phillips, it is first of all examined by the nicest chemical means to see the exact amount of silver it contains, by a process rivalling in delicacy that which is pursued in laboratory research, and having ascertained exactly the quantity of silver contained in the solution, the exact quantity of extremely expensive reagent, iodide of potassium, which is required, is added to it to precipitate the amount of silver; and when the iodide of silver is thrown down the iodine is recovered to be used over and over again, and the silver itself is set free by means of metallic zinc, which forms iodide of zinc, thus setting free the silver. In this way, a considerable portion of silver is extracted.

I mention this as an illustration of the remarkably close association which is every day taking place between sure chemistry in the laboratory, and manufacturing chemistry in the factory. Now-a-days we have such out-of-the-way bodies, as they were formerly considered, as these different aniline products, as alizarine and chloral, which were formerly barely obvious in the laboratory, now made on a manufacturing scale. On the other hand, we find these different products of estimation, formerly confined to the laboratory, are now carried on in the manufactory, and thereby such an element as silver is produced by processes which are essentially laboratory processes. In this way it happens that we find many improvements in manufacturing chemistry are now produced by men who have obtained a reputation in other fields. For instance, I need scarcely refer to the names of Hoffmann, Perkin, and Nicholson, gentlemen known as scientific chemists and men of the highest eminence, before their attention was directed to manufacturing operations, and they realise on a manufacturing scale the results of their laboratory experience. In mentioning them, I ought not certainly to dissociate from them our lecturer this evening, Mr Field, who was so long and so highly esteemed in purely scientific circles for his admirable researches into a great number of compounds, more especially connected with mineral chemistry, before he devoted his great ability to the elucidation and improvement of the manufacture of aniline dyes, and subsequently to these metamorphoses of the bodies which we now use for illuminating purposes in the form of paraffine and ozokerit, and also the other candles which are composed of stearic acid, palmitic acid, and so on.

But while in this way manufactures derive a very great advantage from the light thrown on them by purely scientific chemists in one way or another, I do not think we ought to overlook the benefit which pure chemistry derives, on the other hand, from manufacturing operations. I do not mean the mere material gain that purely scientific chemists have enjoyed by the opportunity of examining minutely a great number of bodies, which previously it was almost impossible for them to obtain, but I think they have gained a very much greater knowledge of the especial subject of their studies — I mean chemical phenomena. We chemists take in our province every change by which one kind of matter becomes metamorphosed into another kind of matter, whereby that which was ironstone, for instance, becomes iron, whereby that which was sand, chalk, and soda becomes glass, and which takes place wherever one kind of matter is metamorphosed into another; but, after all, a great number of the metamorphoses which we must study take place in the test-tube and small vessels of similar character; and we are rather too apt, I say, to shut our eyes to those metamorphoses which take place on a large scale around us.

Those changes manifest themselves particularly in two forms. We have those by which the different forms of agricultural produce are furnished us by the vegetable kingdom, and by which they are metamorphosed into the animal kingdom. Here we have one great illustration of industrial chemistry—the chemistry by which crops are produced, and by which stock is fed and flesh is made. This feeding of stock and production of crops is one very large function of industrial chemistry, and I would venture to say that any scientific chemist who devotes his attention entirely to what takes place in the test-tube, and who neglects those changes which are constantly taking place around him, has a very imperfect notion of the subjects which he professes to investigate. And in addition to these changes thus taking place in natural processes, modified to a certain extent by art, we have three other processes which take place on a grand scale, by which from such substances as ironstone we produce metallic iron, from common salt, on the one hand, carbonate of soda, applied to the manufacture of glass and other useful purposes, and by which we provide also chlorine in its different combinations, applicable to so many purposes, more particularly in the preparation of our wearing apparel, and in our linen and fabrics of every description.

I think, then, that when we have the advantage of having these industrial subjects brought under our notice by men like our friend here, who are familiar, on the one hand, with the most recondite points of theoretical chemistry, and, on the other hand, with the greatest practical achievements which have been obtained in manufacturing chemistry, it will be of immense benefit to those who wish to study chemistry in its pure aspect, as they will see what can be done on a large scale, and what habitually is done, and what perseverance, assisted with chemical knowledge, has obtained for us. It must also be interesting to practical men, by throwing out suggestions capable of improvement in various branches of manufacturing art. I think, then, that the Society of Arts has really done a very useful work in bringing together men engaged in the purely scientific pursuit of chemistry on the one hand, and, on the other, men who are pursuing the application of the science with a view to the practical good of their kind. I do not know that I need trouble you with any further remarks, but I have attended here this evening with the greatest pleasure, because I feel how much advantage is likely to be derived by all classes of the community by the discussion of these problems which are so interesting to all, and I would venture to say as much in a purely scientific as in a practical point of view.

NOTES

SUFFICIENT attention has not been attracted to the fact that one of the recommendations of the Committee on Scientific Instruction has borne early fruit. Mr. Phillips Jodrell, desirous to promote research in physiology, has attached to the professorship of that science in University College, London, an endowment of 7,500*l*. to enable the professor to devote to biological investigation whatever time is not needed for the discharge of his duties as lecturer. This endowment is accompanied by the further sum of 500*l*., to be expended in the purchase of the necessary apparatus. It is difficult to speak in terms sufficiently high of Mr. Phillips Jodrell's intelligent munificence, which, we have no doubt, will bear good fruit. It is gratifying that the recommendations of the Commission have so far had such an excellent result, and we only hope that Mr. Jodrell's handsome example will be largely followed by others who have enough and to spare.

OUR readers will no doubt learn with surprise and regret that M. Alglave, editor of the *Revue Scientifique*, and Professor of

Law in the faculty of Douai, has been suspended from his professorial duties for one month, and has received notice that he must either resign his position as professor or give up the editorship of the *Revue Scientifique*. The reason for this vexatious proceeding we have not learned; but to outsiders it must seem a wanton and mischievous exercise of authority, although the fearless way in which the *Revue* states scientific facts and conclusions may have something to do with it. The *Revue Scientifique* holds the first rank among French scientific journals, contains from week to week the cream of scientific work both French and foreign; and any interference with its efficiency would be a great blow to the cause of Science in a country where a knowledge of the methods and solid results of Science is much needed. If the threat with which M. Alglave is menaced be carried out, those who thus abuse their little brief authority will be despised by the whole educated world. We read in the *Progrès du Nord* that M. Terrat has been appointed to take M. Alglave's place, and that when the former entered the class-room the students retired silently and in perfect order, leaving M. Terrat to lecture to the walls of the amphitheatre and two members of the *Cercle Catholique* who did not deem it prudent to join in the protest of their class-fellows; the students, we believe, have presented to their professor a unanimous address of sympathy. Let us hope that before the expiry of the month those who are responsible for this treatment of M. Alglave will think better of it, and restore the professor to the position he appears to have filled so well, permitting him at the same time to retain charge of the journal which is among the things that reflect the highest credit on France.

The Chimpanzee, which during the last two and three-quarter years has been an endless source of instruction and amusement to visitors at the Zoological Gardens, after an illness of two months' duration, died on Friday last. The post-mortem examination showed that the cause of death was acute tuberculosis of the peritoneum, almost exclusively confined to the serous membrane covering the liver and spleen, the omentum and small intestine being slightly affected. A large bronchial gland was on the verge of suppuration, but the lungs were healthy. There were no symptoms of hectic during life, and much subcutaneous and omental fat were found after death.

The French Meteorological Society has resolved to hold an extraordinary *réunion* during Easter-week, a time when a considerable number of French and foreign meteorologists are in Paris. This meeting has for its object to strengthen the relations which exist between the Society and provincial observers, and to study in common questions of general interest in meteorology.

The Meteorological Office has resolved, in compliance with the wish of the majority of subscribers to the lithographed sheets of hourly readings from their observatories, which are about to appear, to issue the sheets in monthly, not quarterly parts.

In addition to the *Bulletin Météorologique du Nord*, of which we lately announced the publication, we are glad to learn that Capt. Hoffmeyer, the Director of the Meteorological Institute of Denmark, has commenced the issue of a daily lithographed chart, for his own country, Norway, Sweden, and North-west Russia. He has also published an explanation of the chart for the use of subscribers. This chart is most valuable, as it supplements our own daily weather charts and those of the *Bulletin International*, for a district whence accurate information is seldom obtainable by telegraph in Western Europe.

In a recent number we intimated that the Perthshire Society of Natural Science had interrogated the Parliamentary candidates for the county and city of Perth as to their opinions on the questions of State help to Science, a responsible Minister of Education, and the promotion of Scientific Exploring expeditions. Answers—favourable, we are glad to say—were returned at the time only by the two Conservative candidates, one of whom, Sir W. Stirling Maxwell, is now M.P. for Perthshire. We are now glad to give place to the somewhat tardy reply, addressed to the secretary, of the Hon. Arthur Kinnaird, Member for the City of Perth:—"1, Pall Mall East, 18th Feb. 1874.—Dear Sir,—I was surprised to find copied into a London paper from a Scotch journal the questions put in your letter of the 29th January last, with the statement that they had not been answered by me. The fact of my being, as I believe I am, one of the patrons of the Perthshire Society of Natural Science should have been, it appears to me, a sufficient guarantee of my approval of the objects of your institution; and my active co-operation with Capt. Wells in his efforts during the last session of Parliament to obtain the sanction of Government to a proposed grant for the furtherance of Arctic exploration, further approves my appreciation of the objects you advocate, in my willingness to support State expenditure for well-devised schemes of scientific research and educational purposes.—Yours truly, A. Kinnaird."

H. N. Martin, B.A., Cantab, D.Sc. Lond., has been appointed Lecturer on Physiology at Christ's College, Cambridge. Dr. Martin obtained an open scholarship for Natural Science at Christ's College, and graduated in the Natural Sciences Tripos, obtaining the first place. Lately he has acted as assistant to Dr. Michael Foster in the Physiological Laboratory of the University. A paper by Dr. Martin on the "Structure of the Olfactory Mucous Membrane" appeared in the last number of the *Journal of Anatomy*, and was reprinted in the Studies from the Physiological Laboratory of the University, Cambridge, edited by Dr. Michael Foster.

There will be offered, at the Matriculation exhibition at New College, Oxford, beginning on Wednesday, May 21, at 9 A.M., two Exhibitions, tenable for three years, of the annual value of 50*l*. each. These Exhibitions are open to all persons who have not already been matriculated at another College. In the election to one of the Exhibitions a preference will be given to proficiency in Natural Science, if there is any candidate of sufficient merit in the judgment of the examiners. Further account of the examination will be supplied on application to the Warden.

The Science and Art Department has published a catalogue of apparatus for teaching chemistry, containing 112 items, with prices from which a deduction of 50 per cent. is given. We should advise all our readers who are interested in the subject to obtain the catalogue. The same Department has also formed a collection of travelling apparatus for illustrating instruction in Physical Geography, which will be lent, for a short time, to teachers of the subject referred to. The apparatus consists of a set of physical maps of the world and the various continents, by Prof. Sydow, models of Mount Vesuvius, of Mont Blanc, and of the Thames Valley.

We regret to record the death of Dr. Forbes Winslow, which took place at Brighton on the 3rd instant, at the age of 63 years.

We formerly announced the proposal for an international memorial to Captain Maury, which is to take the shape of a Lighthouse on the Rocos, to the importance of which Maury called attention in his "Sailing Directions." We learn from *Ocean Highways* that the President of the Board of Visitors of the Virginian Military Institute addressed a letter to the Governor of Virginia on January 23, requesting him to lay the question of the Maury memorial before the General Assembly, for such moral support as may fitly be given by the representatives of a State which gave Maury to the world. A joint committee of members of the Senate and House of Delegates has since been appointed; and the hearty co-operation of the Governments and Scientific Societies in Europe is confidently expected; for Maury's services have benefited not his own country only, but the maritime interests of the whole world.

On the 19th of February, Dr. Peters, of Hamilton College, New York, discovered a planet in 11^h 19^m right ascension plus 4 deg. 25^m declination.

M. Charles Sainte Claire Deville, the meteorologist, announced publicly before the French Institute, that the week from March 9 to 16 would be a very cold one. The prognostication is in a fair way of being fulfilled.

The last scientific letter written by the lamented M. Quetelet was to General Myer, the Director of the American Meteorological Service. Its purpose was to inaugurate the daily intercommunication of meteorological news between the States and Belgium. The scheme so originated is working regularly and was not interfered with by the death of the learned and respected Belgian astronomer.

The *Lancet* says that the authorities of the University of Aberdeen have now under consideration a proposal to institute a new degree in arts—that of Bachelor of Science.

Herr von dem Borne writes to the *Deutsche Fischerei Verein*, in Berlin, that he is at present occupied upon an exhaustive treatise, on the most recent and best methods and implements of fishing with the hook and line, especially as used in England and North America, and is desirous of receiving information on these subjects from dealers and others, to be embodied in his proposed work.

Velocipedes are becoming an institution in Paris for forwarding messages from the Exchange (Bourse) to the central telegraphic office, rue de Grenelle. The rates charged by "velocemen" are two shillings. The run there and back, including delivery of messages, takes about 25 minutes for a distance of 3 miles 1,320 yards. It is contemplated by some speculators to establish a public company. When Marshal Bazaine's trial was going on, velocipedes were used for conveying messages from Versailles for the *Moniteur*, one of the Parisian papers. The single run was charged a pound sterling, and was accomplished in 45 minutes for a distance of 12½ miles, at a quicker rate than the railway trains. But the road descends all the way, Versailles being on a higher level than Paris, and the railway is circuitous; stoppages are also very frequent on the line.

Carrier-pigeons are largely used by Parisian periodicals for carrying latest intelligence. They start from Versailles from two o'clock in the afternoon till three. The average number is thirty pairs, and the charge four shillings each pair. The journey is accomplished in twelve minutes when fogs are not frequent. It is not legal for newspaper editors to hire a wire for their private use.

M. Henry Giffard, the inventor of the *Injecteur*, has constructed a railway carriage with a patent suspension of his invention, which prevents the passengers from feeling any inconvenience from oscillation. The first public experiments will be on the Versailles railway, just after the impending parliamentary holidays.

The election of a successor to the late Dr. Nelaton has given rise to a severe contest in the Secret Committee of the French Academy of Sciences; for more than a month it has obstructed the usual routine of the Academy. The reports for yearly prizes, which are ready for adoption, were not read over. The number of competitors is greater than usual, amounting to seven.

The Parisian Municipal Corporation has decided upon the building of a large bridge on the Seine, which in point of length will be equal to Blackfriars or Waterloo Bridge, London; but instead of being placed at right angles to the current, it will be placed in an oblique direction. This extraordinary step is taken to connect the rue des Etats on the left bank with the old arsenal quarter on the right bank.

M. Figuier has issued through Hachette his *Année Scientifique*. It is the 17th volume of the whole series.

An International Agricultural Exhibition is to be held on a grand scale at Bremen, under the patronage of the Crown Prince of Germany, from June 13—21 next. The North German Lloyd will grant special facilities to the English exhibitors for the conveyance of implements from London, Southampton, and Hull.

The enormous extent of the destruction of buffaloes on the Western plains of the United States seems to have undergone no diminution during the present winter, and there is every reason to fear that, should this continue a few years longer, the animal will become as scarce as is its European congener at the present day. At present, thousands of buffaloes are slaughtered, every day, for their hides alone, which, however, have glutted the market to such an extent that, whereas, a few years ago, they were worth three dollars apiece at the railroad stations, skins of bulls now bring but one dollar, and those of cows and calves sixty and forty cents, respectively. A recent short surveying expedition in Kansas led to the discovery of the fact that, on the south fork of the Republican, upon one spot, were to be counted six thousand five hundred carcases of buffaloes, from which the hides only had been stripped. The meat was not touched, but left to rot on the plains. At a short distance hundreds more of carcases were discovered, and, in fact, the whole plains were dotted with putrefying remains of buffaloes. It was estimated that there were at least two thousand hunters encamped along the plains, hunting the buffalo. One party of sixteen stated that they had killed twenty-eight hundred during the past summer, the hides only being utilised.

A copy of the Calender for 1873-4, of the Imperial College of Engineering, Tokei, Japan, has been forwarded us. The course of study prescribed, both general and special, theoretical and practical, and the regulations for the government of the College, appear to us to be all that at present could be desired.

We are asked to state that the Annual Dinner of the members of the Institution of Civil Engineers has been appointed to take place at Willis's Rooms, St. James's, on Saturday, March 21. Mr. T. E. Harrison, the president, will occupy the chair.

We are glad to see that Guido Cora's well-conducted Italian geographical journal, *Cosmos*, is to be henceforth issued monthly, instead of every two months.

An aëronautical society of Paris, the "Aerial Sport," has published a programme of an aerial spring meeting to be held in the neighbourhood of Paris, very likely Vesinet. The object is to send in the air small fire balloons carrying *des mèches illuminées*, whose length has been calculated so that the cargo of the balloon may fall close to a post chosen. Every champion is to choose his own wind; but nobody has a right to approach closer to the post than three miles. It is a kind of drill for shelling a place with balloons by taking advantage of the wind.

The additions to the Zoological Society's Gardens during the last week include a Javan Rhinoceros (*Rhinoceros sondaicus*) from Java, purchased; a Negro Tamarin (*Midas ursulus*) from North Brazil, presented by Mr. W. Thomson; two Goshawks (*Astur palumbarius*), European, one presented and the other deposited by Mr. G. Lascelles; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. S. Waight; two Verreaux, Guinea Fowl (*Numida eduardi*) from East Africa; two Crowned Pigeons (*Goura coronata*) from New Guinea, and a Common Cassowary (*Casuarius galeatus*) from Ceram, purchased.

SCIENTIFIC SERIALS

Ocean Highways, March.—The following are the original articles in this number:—"Dr. Livingstone and the Cameron Relief Expedition;" "Francis Garnier—In Memoriam," a highly and deservedly eulogistic memoir (by Colonel Yule, C.B.) of this brave and high-minded soldier and explorer, whose untimely death we recently noticed; "Bhawalpur;" "An Account of the Early Jesuit Missions in the La Plata," by A. A. Geary; and "The British India Steam Navigation Company."

THE *Geological Magazine*, March.—The following are the original articles in this number:—"The Leinster Coal-field," by J. M'C. Meadows (with a page map); "On a new Species of *Dithyrocaris* from the Carboniferous Limestone, &c.," by Henry Woodward, F.R.S., and Robert Etheridge, jun. (with a plate); "Glacialoid or Rearranged Glacial Drift," by G. H. Kinahan; "On some new Devonian Fossils," by Prof. H. Alleyne Nicholson (with a woodcut and plate); "Reply to Mr. Poulett-Scrope," by Robert Mallet, F.R.S.

Quarterly Journal of the Meteorological Society, January.—This number contains the following papers:—"Notes on Meteorology of Vancouver Island," by R. H. Scott, F.R.S.; "The Thunderstorm at Brighton on October 8, 1873, and its effects," by F. E. Sawyer; "Some of the Considerations suggested by the Depressions which passed over the British Islands during September 1873," by F. Gaster; "On an improved form of Aneroid for determining Heights, with a means of adjusting the Altitude Scale for various Temperatures," by Rogers Field; "On the Hurricane of August 1873, which moved in a curved track round Bermuda between the 20th and 23rd, and passed on to Nova Scotia and Cape Breton on the 24th, doing extreme damage both at sea and on land," by Capt. H. Toynbee (with a plate); "On a Mercurial Barometer for the use of Travellers, filled by the spiral cord method," by Staff-Commander C. George, R.N.; also an account of the discussion on the best form of Thermometer Stand, which took place at the meeting of November 19, 1873.

Archives des Sciences Physiques et Naturelles, Jan. 15, 1874.—The principal article in this number is an exhaustive study, by Prof. Forel, of the *seiches*, or peculiar tidal phenomena, which have long been observed on the lake of Geneva. The subject is treated in five sections, as follows: the seiches at Geneva and at Morges compared; oscillatory movement in the harbour at Morges, analogous to seiches; the movement of oscillation of seiches; experimental study (with special apparatus) of the laws of oscillation of libration; and lastly, comparisons and conclusions. Prof. Forel adheres to the theory generally accepted in explanation of the phenomenon, viz., that it is due to variations of atmospheric pressure; the pressure diminishing at one part and increasing at another, the surface of the water rises in the former case and sinks in the latter. Thus a swinging undulation is produced. Some of the larger seiches are attributed to earthquakes. The amplitude differs in different seiches; and in the same seiche it varies from one point of the lake to another. The duration of different seiches also varies in the same locality; and the duration of seiches is longer at Geneva than at Morges. These and other effects the author seeks to explain, harmonising them with physical phenomena studied in his apparatus.—In a note on the surface of waves, by M. Charles Cellerier, it is sought to show that there is no real disagreement between the laws of double refraction, as furnished by observation, and the theory based on molecular movements. It is probable, he thinks, that the ordinary ray, whether in uniaxial crystals, or in principal sections of crystals with two axes, has not quite the direction generally assigned to it; though the deviation, without disagreeing with theory, may be so small as to escape observation.—In the department of zoology, we may notice a review of recent researches by Haeckel, Bütschli, and others, on Infusoria.

Memorie della Soc. degli Spectroscopisti Italiani, September, 1873.—Prof. Tacchini contributes a paper on his observations on the magnesium lines and 1,474 line seen bright on the sun's limb, from which it appears that the 1,474 line is always visible in a magnesium region; and further, that it frequently appears by itself where no magnesium is seen. Two beautifully-executed chromolithographs of the chromosphere on the 15th and 16th of July last accompany the paper, showing the relative intensities of the magnesium and 1,474 lines, together with their positions. The intensity of the magnesium lines seems the greater of the two, though not covering so extensive a region.—Father Secchi gives a note on the spectrum of iron, and other metals, obtained by volatilisation with fifty Bunsen's cells. He appears to find that the 1,474 line is not due to iron, and that different kinds of iron give slightly different spectra. He also gives a drawing of the carbon spectrum from the electric light, which appears similar to that of cyanogen, with the addition of five equidistant bands in the yellow and red.—The tables of Mr. E. Loomis, containing the maximum years of sun-spots, the maximum years of magnetic declination, and the maximum years of auroral display from 1778 to 1870, are given, from which, at a glance, it is seen that the maxima of all three occur in the same years with very small exceptions, and the years of minima correspond even better.

Justus Liebig's Annalen der Chemie u. Pharmacie, Band 170, Heft 3. This number contains the following papers:—"Communications from the Chemical Laboratory of the Polytechnic School at Delft: iv. Researches upon Podocarpic Acid," by A. C. Oudemans, jun. This acid is obtained from the resin of *Podocarpus cupressina var. imbricata* Blume.; a tree growing in Java. The formula assigned to the acid is

$$C_6H_4\left\{\begin{array}{l}OH\\COOH\\CH_3\\C_9H_{13}\end{array}\right.$$

$= C_{17}H_{22}O_3$. Some of the salts are described, and also the mono- and di-nitro substitution products. A sulpho-acid, amidated, and brominated derivatives have been obtained, likewise an acetyl derivative. The author has studied exhaustively the decomposition products of the new acid, and these have led him to the constitutional formula above given.—Upon Cymene, by F. Beilstein u. A. Kupffer. The authors have examined cymenes from cumin-oil and from camphor, and find them to be identical. The same authors contribute a paper on oil of wormwood. This oil yields by distillation a terpene, absinthol ($C_{10}H_{18}O$), and a deep blue oil.—"Crystallographic researches on the calcium salts of cymene-hyphersulphonic acid," by M. Jerofejew.—"Cumic acid," by F. Beilstein u. A. Kupffer. The authors obtain the potassium salt of this acid by acting on cumin-oil with fused potash.—A lengthy paper on the salts of ethylaldehyde-sulphurous acid and the action of the sodium-sulphites upon ethylidene chloride, by Hans Bunte.—On the formula of silicates, by Prof. V. Wartha.—The concluding paper is by Otto Sigel, on the constituents of arnica water and of the essential oil of arnica.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, March 5.—Special General Meeting, G. Busk F.R.S., vice-president, in the chair. After the chairman had, in a short conciliatory address, stated the reasons which had induced the Council to summon the present meeting, he called on Mr. W. Carruthers, F.R.S., who moved "That a Committee be appointed to consider the Bye-laws and to suggest to the Council such alterations, omissions, or additions as they may think desirable." The motion having been seconded by Mr. W. S. Dallas, Major-General Strachey, F.R.S., moved as an amendment, which was seconded by Mr. C. J. Breese, "That, inasmuch as it appears that there are differences of opinion in the Society as to the legality of the alterations of the Bye-laws made at the meeting of January 15 last:—(1) This meeting, retaining complete confidence in the President and Council of the Society, requests them to obtain the opinion of some legal authority, whether these alterations are legally binding on the Society or not; (2) That if the opinion be that the said alterations are legally binding, no further steps be taken in reference to them; (3) That if the opinion be that the said alterations, or any of them, are not legally binding, the Council be requested to take the necessary proceedings for setting aside the vote of January 15."—A second amendment was moved by Mr. J. E. Harting;—"That the case having been already submitted to Council, the opinion thereon be read for the information of the meeting." After much discussion, in which Sir John Lubbock, Dr. Thos. Thomson, Dr. Trimen, Prof. Thiselton-Dyer, Mr. H. G. Seeley, and others took part, Mr. Harting's amendment was withdrawn, and the vote taken on Major-General Strachey's amendment, which was carried, and was afterwards adopted as a substantive resolution.—Sir John Lubbock, Bart., F.R.S., then moved and Mr. W. Carruthers, F.R.S. seconded a resolution expressive of the high sense entertained by the meeting of the eminent services both to the Society and to Science rendered by the President during his long tenure of the chair, which was carried unanimously by acclamation; and the meeting closed with a vote of thanks to the chairman.

Zoological Society, March 3.—Dr. E. Hamilton, vice-president, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February, 1874, and called especial attention to a Malayan Hornbill (*Buceros malayanus*) new to the Society's collection, acquired by purchase; a Python, presented by Mr. C. J. Noble, of Hong Kong, having been captured in his garden on the Chinese mainland; and a young male of an undescribed species of Deer from Northern China.—A letter was read from Sir Henry Barkly, K.C.B., Governor of the Cape Colony, announcing that he had obtained a pair of young Eared Seals (*Otaria pusilla*) for the Society's collection.—A communication was read from Mr. W. H. Hudson, of Buenos Ayres, describing the parasitical habits of the three species of *Molothrus* found in Buenos Ayres, namely, *M. bonariensis*, *M. badius*, and *M. rufoaxillaris*.—Mr. Sclater read an account of a small collection of Birds obtained by Sir Graham Briggs in the island of Barbados, West Indies.—A second paper by Mr. Sclater contained the description of an apparently new form of the family *Icteridæ*, which he proposed to call *Centropsar mirus*.—A communication was read from Dr. J. E. Gray, F.R.S., containing some remarks on *Crocodilus johnstoni* Krefft, from Northern Australia, of which he proposed to form a new genus, *Phylas*.—Mr. W. Saville Kent, F.L.S., read a paper on a huge Cephalopod or Cuttle Fish, announced by the Rev. M. Harvey as lately encountered in Conception Bay, Newfoundland, and of which a tentacle 16 feet long has been secured for the St. John's Museum (NATURE, vol. ix. p. 332). Mr. Saville Kent contributed the additional evidence of an arm 9 feet long preserved in the British Museum, in proof of the gigantic dimensions occasionally attained by certain members of this order of the Mollusca, and proposed to institute the new generic title of *Megaloteuthis* for their especial reception; he further suggested distinguishing the Newfoundland example as *Megaloteuthis harveyi*, in recognition of the service to science rendered by Mr. Harvey, in his record of and steps taken to preserve so valuable a trophy.

Geological Society, Feb. 25.—John Evans, F.R.S., president, in the chair. The following communications were read:—Geological Notes on a Journey from Algiers to the Sahara, by George Maw. The author commences by describing the details observed on his journey from Algiers to L'Aghouat, on the borders of the Sahara. The distance traversed was 285 miles, or about 210 miles in a straight line, and in a direction nearly north and south. No eruptive rocks were observed. The oldest rock is a boss of mica-schist and gneiss behind the city of Algiers; it forms a low anticlinal, with a north and south strike. The pass through the gorge of the Chiffa in the Lesser Atlas shows hard slaty rocks dipping south at a high angle; they are repeated as an anticlinal on the south side of the higher part of the Tell plateau, and are probably Mesozoic. In the plain separating the Tell from the Hauts Plateaux, and on the south side of the latter, red and yellow sandstones form anticlinals; these rocks resemble the Bunter in mineral characters, and are overlain by red marls resembling the Keuper. In the northern escarpment of the Hauts Plateaux saliferous marls are exposed, interstratified between the sandstones and below the red and grey marls. Crystals of salt and gypsum are intimately mixed with the grey marls, and the so-called "Rochers de Sel" are capped with great blocks of rock tumbled about in confusion, the position of which the author ascribes to the failure of support due to the solution of the salt in the underlying salt-marls. A thin series of bright red and green marls is seen to overlie the red sandstones in several places; and above this is an immense series of dark grey marls, interstratified with argillaceo-calcareous bands, forming a great synclinal of the Hauts Plateaux, and a contorted mass on the Tell plateau. These are probably cretaceous. At L'Aghouat they are overlain by fossiliferous beds, probably of Miocene age. Other Tertiary beds observed are soft yellow calcareous freestones on the flanks of the promontory of Algiers and of the Lesser Atlas, and some red and grey marls and ferruginous freestone capping the Tell plateau, the former at a height of 100–900 feet, and the latter of 2,500–4,000 feet above the sea-level. The plain of the Mitidja, between the Lesser Atlas and Algiers, consists of grey loam with shingle-beds, of post-tertiary age. A similar loam covers the great plain of the northern Sahara, and rises to a height of 2,700 feet. Raised beaches occur on the coast up to an elevation of 600 feet above the sea-level; and similar beaches are found inland, south of the Tell plateau, at a height of 2,000 feet. The oldest land in the line of section is the anticlinal of mica-schist near Algiers, the strike of which is nearly at right angles to that of the other rocks. The upheaval of the Mesozoic rocks was contemporaneous with the first upheaval of the Lesser Atlas; it was followed by a long period of denudation, and this by a subsidence of at least 3,000 feet in Tertiary times, during which the Miocene deposits were formed. The Tell plateau was thus elevated at least 4,000 feet, and the district north of the Lesser Atlas at least 1,000 feet, the north face of those mountains probably marking a post-tertiary line of fault of 3,000 feet. This operation was followed by a long period of denudation, and this by a post-tertiary depression, which the author terms the "Sahara Submergence," after which the land was re-elevated at least 3,000 feet, but perhaps considerably more. A gradual subsidence appears to be still taking place.—On the Trimerellidæ, a Palæozoic family of the Palliobranchs or Brachiopoda, by Thomas Davidson, F.R.S., and Prof. William King. In this memoir the authors describe in detail certain Brachiopoda, for which they propose to establish a distinct family, discuss the characters and affinities of the family, and indicate certain geological considerations which arise from their study of its numbers.—Note on the occurrence of sapphires and rubies *in situ* with Corundum, at the Culsagee Corundum Mines, Macon Co., North Carolina, by Col. C. W. Jenks. Communicated by David Forbes, F.R.S.

Chemical Society, March 5.—G. C. Foster in the chair.—A paper on the spontaneous combustion of charcoal, by Mr. A. F. Hargreaves, in which he pointed out the best wood for charcoal for the manufacture of gunpowder, and also the best method of charring it. It appears that if it is ground too soon after being burnt the charcoal is liable to take fire spontaneously.—The other communications were—Researches on the action of the copper-zinc couple on organic bodies: Part V. On the bromides of the olefines; Part VI. On ethyl bromide, by Dr. J. H. Gladstone and Mr. A. Tribe.—Researches on the preparation of organo-metallic bodies of the C_nH_{2n} series of hydrocarbons, by Dr. D. Tommasi.—Note on the action of trichloracetyl chloride on urea, by Messrs. R. Meldola and D. Tommasi; and the agglomeration of finely-divided metals by hydrogen, by Mr. A. Tribe.

Royal Microscopical Society, March 4.—Chas. Brooke, F.R.S., president, in the chair.—A paper was read by Mr. Alfred Sanders, entitled "A Contribution towards a Knowledge of the Appendicularia," in which he minutely described specimens found at Torquay and Weymouth, and illustrated the subject by diagrams. A short discussion ensued as to the best methods of observing and preserving these delicate organisms.—Two papers by Dr. Royston Pigott were afterwards read by the secretary, the first "On the Verification of Structure by means of Compressed Fluid," the second being entitled "A Note on the President's remarks on Dr. Pigott's Aplanatic Searcher."—Dr. Pigott subsequently gave an extended explanation of the contents of his papers, and also detailed a new method of determining the refractive index of covering glass.

Entomological Society, March 2.—Sir Sidney S. Saunders, president, in the chair.—Mr. M'Lachlan exhibited two male examples of an Orthopterous insect belonging to the family *Locustidæ*. They were said to be sold in the streets of Shanghai, confined in ornamental wicker cages, and bought for the sound they produced. The species appeared to be undescribed, and to pertain to a new genus allied to *Xiphidium*. Mr. M'Lachlan also exhibited a series of examples illustrating the natural history of *Oniscigaster wakefieldi*, from New Zealand, described and figured by him from the female imago in the *Entomologist's Magazine* for October 1873. The series now exhibited comprised the male imago, female sub-imago, adult nymph, and larva. The lateral wing-like horny expansions of the terminal segments of the abdomen in the imago and sub-imago are continued in the aquatic conditions on each segment of the abdomen, and in addition there are similar formations along the back of the abdomen, placed longitudinally and vertically. The adult nymph appears to possess no external gills or laminæ, but they are conspicuous in the less mature larva on each side of the ventral surface of the abdomen.—The Rev. A. E. Eaton exhibited some Arctic insects which he had brought from Spitzbergen; and also some excellent photographs illustrating the scenery of the country.—A further communication was received from Mr. Gooch respecting the injury to the coffee-trees in Natal from the Longicorn beetle, *Anthores leuconotus* Pascoe.—Papers were communicated, "On some new Species of South African *Lycænidæ*," by Mr. Roland Trimen, and "Descriptions of new Species of *Lycænidæ*," from his own collection, by Mr. W. C. Hewitson.

Society of Biblical Archæology, March 3.—Dr. Birch, F.S.A., president, in the chair.—The following papers were read:—Translation of an Egyptian fabulous romance, "The Tale of the Doomed Prince," from the Harris Papyri, by C. W. Goodwin. The translator drew attention to the peculiar features of this ancient story, resembling in so many points the romances of the mediæval period, which may have had a common origin.—Translation of an historical narrative belonging to the reign of Thothmes III., by C. W. Goodwin.—Observations upon the Assyrian verbs *Bâsu* and *Qabah*, by Prof. William Wright. This paper consisted of a critical analysis of the roots of the above verbs, and their cognate analogues in other Semitic languages.

Geologists' Association, Feb. 6.—Henry Woodward, F.R.S., president, in the chair.—On the probability of finding Coal in the Eastern Counties, by John Gunn, F.G.S. Mr. Gunn gave the preference to a boring on the south of Essex, and proceeded to state the grounds on which he recommended another boring at Hunstanton, or along the outcrop of the Kimmeridge clay, in Norfolk. He detailed the several papers which he had read at the meetings of the British Association at Nottingham, Brighton, and Bradford, in proof of the existence of a forest bed in Norfolk and Suffolk, which he called the Anglo-Belgian basin, or a succession of growth of forests, and of alternate elevations and depressions which have taken place in that region, and argued thence, by analogy, the extreme probability that such existed in the carboniferous epoch. Mr. Gunn represented that if the southerly dip of the Harwich slaty rocks extended in a northerly direction it must have been reached at the Norwich boring, which was sunk considerably lower than that at Harwich, and did not pierce through the gault. Mr. Gunn dwelt especially upon this as the most serious objection to the prospect of reaching coal at Hunstanton, or rather carboniferous beds, expressed so strongly by Prof. Hall at the Brit. Ass. meeting at Brighton. Mr. Gunn also referred to the evidence of local subterranean movements in proof of the proximity of disturbances acting upon what he regarded as a thin envelope of tertiary or secondary deposits probably not exceeding 1,000 feet, and perhaps much less. He referred to the evidence of boulders, which he hoped to adduce on a future occasion.—On the Geology of Nottingham, by the Rev. A. Irving, F.G.S. Part I.

EDINBURGH

Geological Society, Feb. 26.—David Milne Home, F.G.S., vice-president, in the chair.—The following papers were read:—Notice of large striated boulder in Tynecastle Sandpit, a quarter of a mile west of Dalry Cemetery, Edinburgh, by D. Milne Home.—On glacial phenomena in the neighbourhood of Edinburgh—(1) the Pentland Hills; (2) Bruntsfield Links; (3) Blackford Hill; (4) Tynecastle—by D. J. Brown.—Notice of a section in the building excavations at Tynecastle, by Ralph Richardson.—On glacial phenomena in the Pentland Hills and neighbourhood of Edinburgh, by John Henderson.—Mr. Milne Home's paper, which was illustrated by diagrams, described the boulder as being well rounded on the sides, and its greatest length as 4½ ft., its greatest width 4 ft., its thickness about 2 ft. Its upper and under surfaces were distinctly grooved, and most deeply in the line of the longer axis, which lay N.E. by E. There were some fainter striæ oblique to that line. From a comparison of the striations, he concluded that the superior and lateral striæ had been made after the stone was laid in the bed where found. The stone, which was of greenstone, lay on a bed of compact muddy sand, containing stones which were mostly angular. Above the stone was a considerable deposit of sand, and over that a series of gravels with clayey and sandy beds, all stratified, above which was the soil—the whole deposit being a bank from 20 to 30 ft. thick. This great bed of sand and gravel, in the upper part and west side of which the boulder was found, had been originally a submarine bank. Its height above the present sea-level was about 200 ft. How much above this level the sea stood when this bank was formed was, of course, only matter of conjecture. The nearest rocks similar to the boulders were situated to the westward; most probably, therefore, it had been rafted on ice from that quarter; and, by reason of the ice stranding on this sandbank, the boulder had been deposited there. The deep striæ on the under side showed that the boulder after being deposited on the sea bottom, had been pushed forward easterly. After it had stuck fast it had been striated on the top and exposed sides, by hard and sharp rocks pushed over it, probably by icebergs. The under striæ evidently indicated that they were begun to be formed from the east side, whilst the upper striæ indicated that they had been begun to be formed by some agent passing over from the westward by the pressure of floating ice. Mr. Milne Home stated that a boulder had been recently found on Sir Thomas Hepburn's property in East Lothian which also bore evidence of having been at one time subject to the action of floating ice.

PARIS

Academy of Sciences, March 2.—M. Bertrand in the chair.—The following communications were read:—On the proper nature of the principle of correspondence, by M. Chasles.—On the descending motion of solar and terrestrial cyclones, and on the formation of their opaque envelopes, by M. Faye. This is a reply to a paper by Dr. Reye, and is a defence of the cyclone theory of sun-spots.—On the acid waters which rise in the volcanoes of the Cordilleras, by M. Boussingault. The author considers the simultaneous occurrence of chlorides and sulphates in the igneous rocks the cause of the formation of hydrochloric, sulphurous and sulphuric acids in volcanic emanations, thermal waters, &c.—Meteorology of the month of January 1874 at Tougourt, by M. Ch. Sainte-Claire Deville.—Observations on solar prominences during the last quarter of the year 1873. Results furnished by the employment of diffraction gratings instead of prisms in the spectroscopic observation of the prominences, by P. A. Secchi. The author has observed the coincidence of spots with eruptions on the sun's limb on eighty-nine occasions. Eight times only were spots seen without an eruption. A remarkable case is recorded of the outburst of an eruption during the course of an observation.—On the reduction of bilinear forms, by M. G. Jordan.—On the refraction of gases, by M. Mascart.—Organogenesis compared with androgenesis in its relations to natural affinities (*class Personatæ*), by M. Ad. Chatin.—New species of the genus Dipterocarpus, by M. J. Vesque. Twelve species are described, all from Borneo.—Gnomonic projection of the terrestrial surface upon an octohedron and upon a cube circumscribing the sphere, by M. J. Thoulet.—On a new symptom of death derived from the pneumatosis of the veins of the retina, by M. E. Bouchut.—Geometrical demonstration of some theorems, by means of the consideration of an infinitely small rotation, by M. A. Mannheim.—Apparent orbit and period of revolution of the double star η Coronæ, by M. G. Flammarion.—On the mode of production of certain induction currents, by M. A. Gaiffe.—On the influence of albuminous substances upon electro-capillary phenomena, by M. Onimus.—New researches upon the physiological decomposition of beer-yeast, and remarks on a recent communication by M. Schutzenberger, by M. A. Béchamp.—On the action of chloral upon albumen, by M. H. Byasson.—Of the anæsthesia produced in man by the injection of chloral into the veins, by M. Oré.

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THURSDAY, MARCH 19, 1874

THE CHEMICAL SOCIETY'S JOURNAL

THE man who jokingly said that he had to give up the study of chemistry when the science became so bulky that its Handbook required a wheelbarrow for its conveyance, expressed a truth which has been painfully felt by many scientific workers. With continual fresh additions to our knowledge, anything like a comprehensive grasp of a large science must become daily more and more difficult; but while this difficulty is generally felt, it occurs with special force in the science of chemistry. Chemistry, of all sciences, has perhaps the most unlimited capacity for development. Its subject is enormous, including the whole of nature, animate as well as inanimate. Nor is the chemist satisfied with studying the properties of matter as they are exhibited in the natural operations of the world around us, even this wide and attractive field of observation does not content him; he has made the grand discovery that the elements are his servants; that he can at will take to pieces in his laboratory the compounds found in nature, and construct therefrom a multitude of new bodies. Chemistry may thus be said to produce the matter upon which it feeds; the extent to which the production of new compounds can be carried seems practically unlimited, and these become, in most cases, the starting points of fresh investigations. We have here the principal cause of the wonderful development of modern chemistry; armed with such power, it cannot but abound in valuable discoveries, and furnish, at all times, copious results. As a consequence of this rapid development of the science, it has become a matter of the greatest difficulty for the investigator, the teacher, or the manufacturer, to keep pace with the daily progress of discovery; and improvement, and ignorance of the results already obtained in any department, naturally necessitates a loss of valuable time and labour to those engaged on the subject. The bulk and variety of chemical literature are not, however, the only obstacles to the student; the difficulty is greatly increased to an Englishman by the fact that the greater part of this literature is published on the Continent, and appears in a variety of languages with which the average Englishman has but little acquaintance.

With such difficulties to encounter, the individual student has certainly little prospect of successfully keeping abreast with modern chemistry. We are therefore exceedingly glad to find that the matter has been taken up by the Chemical Society of London, and that they now publish in their monthly journal* carefully prepared abstracts of all the original papers which appear in foreign and English periodicals. The abstracts are classified for facility of reference, and are divided into Physical, Inorganic, Mineralogical, Organic, Physiological, Agricultural, Analytical, and Technical Chemistry; it is, therefore, quite easy to ascertain what has been recently done in any department of the science. When we mention that the volume for last year consists of 1,300 pages, and contains, besides the papers and lectures read before the Society, about 1,500 abstracts of chemical papers published in other journals, we shall give some idea of the magnitude of the work which the Society has undertaken.

Looking carefully through the journal we find that nearly 40 periodicals are regularly abstracted; and as many of these periodicals reprint papers from other less known publications, the extent of literature brought under contribution is very considerable. The periodicals abstracted are German, French, Italian, American, and English, the first two preponderating. The preparation of the abstracts is of course laborious, and demands considerable care. It is accomplished by a body of twenty-six abstractors, chiefly Fellows of the Society, whose initials are appended to their respective work. We are bound to say that the abstracting so far as we have had an opportunity of judging, is exceedingly well done.

A work of this kind is far too expensive to be permanently carried on by a Society destitute of endowment, unless the scientific public in our own and other countries cordially support the enterprise. We understand that the sale of the journal outside the circle of the Society is at present very small, and that the expenses of publication are largely borne by a guaranteed fund raised to give the journal a fair start, and also by a grant from the British Association. We feel sure that the enterprise needs only to be widely known to obtain the support of all lovers of Science. What the Chemical Society is now doing is indeed exactly what we most need in the present day to assist the multitude of workers who are employing scientific facts and methods. It is a kind of work which must sooner or later be carried further, and extended to all the principal sciences, if ourselves and successors are to cope with the ever-increasing accumulation of facts. While such abstracts are, from their early intelligence and their widely gathered and condensed information, an unspeakable boon even to the independent and educated philosopher, they are of still greater value to the ordinary worker, who has not the advantages of a large and costly library, or of an education embracing many languages; to him these abstracts, obtainable at moderate cost in his own language, supply as far as possible the absence of fuller means of information. The work which the Chemical Society has taken up receives, therefore, on many grounds our warmest sympathy. It would indeed be a disgrace to the intellect of our country if such a genuine effort were allowed to drop for lack of support. We would especially invite the attention of our American readers to this monthly journal; supplying, as it does, in their own language a summary of the chemical literature of Europe, we should think it would exactly meet their wants. The Germans have long had a yearly volume of abstracts treating of chemistry and its allied sciences; up to the commencement of the present publication the German *Jahresbericht für Chemie* was indeed the only available work giving a summary of recent investigations. This annual periodical has lately fallen so behind in date (the volumes for 1870 were only obtainable in the middle of last year), that it has really become a chronicle of the past, rather than of the present state of science, and can hardly compare with the new English work. The subscribers to the "Journal of the Chemical Society" possess indeed at the present time abstracts of about 4,000 papers, all of later date than those noticed in the last German *Jahresbericht*. Our German fellow-workers may therefore, with advantage to themselves, give their support to this English work.

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* "The Journal of the Chemical Society," containing the papers read before the Society, and Abstracts of Chemical Papers published in other Journals. Edited by H. Watts, F.R.S. (J. Van Voorst, 1873.)

We trust that the appreciation of all interested in chemical science for this most useful work will be so decidedly shown that the Chemical Society will soon have no further anxiety as to the success of their undertaking. The circle of readers appealed to is a very wide one; not only is it an absolute necessity for those who work at Science and those who profess it, but the medical man, the agriculturist, the manufacturer, and the geologist will all find an abundance of matter interesting to their special pursuits.

TODHUNTER'S "MATHEMATICAL THEORIES OF ATTRACTION"

A History of the Mathematical Theories of Attraction and the Figure of the Earth from the time of Newton to that of Laplace. By I. Todhunter, M.A., F.R.S. Two vols. (London: Macmillan, 1874.)

I.

THE late Prof. de Morgan, in his "References for the History of the Mathematical Sciences," divides the written histories into two classes, those which are written on the plan of Montucla, Bossut, &c., in which a general account is framed out of the writer's notes or remembrances of miscellaneous reading; or in that of Delambre, Woodhouse, &c., in which the successive writings of eminent men are examined and described one after the other, so that each chapter or section is a description of the progress of Science in the hands of some one person, and is complete in itself. This latter plan is the one he considers the most favourable to accuracy and the most interesting to students who are desirous of being the critics of the historians, and of amending their works, if need be. The admirable two volumes before us would certainly be placed under this head. As to the utility of such works, our author remarks: "A familiarity with what has been already accomplished or attempted in any subject is conducive to a wise economy of labour; for it may often prevent a writer from investigating afresh what has been already settled; or it may warn him, by the failure of his predecessors, that he should not too lightly undertake a labour of well-recognised difficulty." Mr. Todhunter is no novice in this style of writing; his "History of the Calculus of Variations" appeared in 1861, and at once placed him in the foremost rank of mathematical historians; this work was followed, in 1865, by the "History of the Theory of Probability." The principles upon which these earlier works were written have been adopted in the work under consideration. Experience has improved his already first-rate powers of analysis and of graphic representation of the contents of the works he considers; all that he wants is leisure; possibly a time may come when the University of Cambridge will appoint an historian (or historians) to fill up the painfully patent void which now exists in this department of literature. The acknowledged high merits of his published histories would suggest Mr. Todhunter as a most fitting first occupant of such a chair; the liberality of the syndics of the University Press in defraying the expenses of the printing of this last work affords evidence that the work is appreciated. In his recent volume of "Essays" (p. 151), our author mentions his taste for the history of Mathematics; we heartily hope that the union of such taste and mathematical powers will result in the begetting a numerous progeny all equally comely with, and of as good disposition as, the elder members of the family.

There is one feature in these histories that especially commends them to our own mind, and that is the writer's *candour*. We cannot better express our own views upon this point than by citing the following passage from the late Sydney Smith's writings: "There is nothing more beautiful in science than to hear any man candidly owning his ignorance. It is *so* little the habit of men who cultivate knowledge to do so—they so often have recourse to subterfuge, nonsense, or hypothesis, rather than to a plain manly declaration, either that they themselves do not understand the subject, or that the subject is not understood—that it is really quite refreshing to witness such instances of philosophical candour, and it creates an *immediate* prepossession in favour of the person in whom it is observed."* It is the absence of this candour which has been productive of so much confusion in this subject of mathematical history: the straining after completeness leads to the insertion of second- and third-hand descriptions; the right rule seems to be that of De Morgan and our author, "to give no opinion or account of any book whatever unless such as is derived from personal acquaintance with its contents." Extreme care and painstakingness are manifest throughout without any sign of flagging. Interesting as Mr. Todhunter's histories are, even to the general student, from the many "sidelights" they contain, and which are especially numerous in the present work, they are exceedingly valuable to the special student, on account of the investigations with which they abound. These are not mere reproductions, but they translate, as it were, the old and now almost obsolete language of the earlier writers into the language of modern analysis: thus in § 443 it is remarked of D'Alembert's notation, "It is not very inviting, and he leaves it to explain itself." Some idea of the extent of these investigations may be got from the fact that 475 out of the 1,632 articles are devoted to them.

The author's design is to write the history of the Mathematical Theories of Attraction and of the Figure of the Earth; for this purpose, he says, he has endeavoured to include all the memoirs and works which relate to these subjects. Such has been his diligence in his seven years' research, that we should suppose few books have escaped his notice: certainly none that would materially affect the conclusions he has arrived at. That he would have added a few to his list had he consulted the British Museum library, or had access to that bequeathed by the late Mr. Graves† to University College, we shall probably show in the course of this notice.

Mr. Todhunter shows that the subjects treated of are of no common importance and influence. Researches into both theories have been fertile in yielding new resources for mathematicians: it will suffice to instance the Transformation of Multiple Integrals, the theory of the Potential, and the functions of Laplace. A knowledge of the figure and dimensions of the earth forms the basis of all the numerical results of Astronomy. In § 25 he carefully defines the several terms made use of in the two subjects, equating in a useful way the varying terms employed by different writers.

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* "Conduct of the Understanding."

† We are informed that the liberality of a gentleman who has already been a great benefactor to the College will shortly enable students to get an accurate idea of the treasures contained in the above library.

The foundation of our subject, as all our readers know, is "great Newton's own ethereal self," Newton, "the crown and glory of his race." "The propositions on Attraction are numerous, exact, and beautiful; they reveal his ample mathematical power. The treatment of the figure of the earth is, however, still more striking, inasmuch as the successful solution of a difficult problem in natural philosophy is much rarer than profound researches in abstract mathematics. Newton's solution was not perfect; but it was a bold outline, in the main correct, which succeeding investigators have filled up but have not cancelled. Newton did not demonstrate that an *oblatum* is a possible form of relative equilibrium; but, assuming it to be such, he calculated the ratio of the axes. This assumption may be called Newton's *postulate* with respect to the figure of the earth; the defect thus existing in his process was supplied about 50 years later by Stirling and Clairaut" (§ 44). Newton appears to have arrived at his theorems in attraction in 1685; the first edition of the "Principia" made its appearance in 1687. (De Morgan, in his "Budget of Paradoxes," p. 81, discusses some of the sources of the apple story.)

Mr. Todhunter nowhere takes account of theories maintained before the time of Sir Isaac Newton; these were, for the most part, if not entirely, non-mathematical. A sketch is given in Book III. of Maclaurin's "Account of Sir Isaac Newton's Philosophical Discoveries, in Four Books."* We draw attention to this work because no reference is made to it in the *History*, whereas great part of Books III. and IV. is devoted to the subject of gravitation.

The same reason (for we cannot suppose Mr. Todhunter not to have consulted the work) has possibly induced him to pass over in silence the "Theoricæ Mediceorum Planetarum ex causis Physicis Deductæ"† of Borelli, though Libri, in his Catalogue, states that this writer "uses the principles of the law of Attraction as afterwards promulgated by Sir Isaac Newton."

Hardly a subsequent chapter but contains from one or another writer an acknowledgment of Newton's high powers; we shall here content ourselves with citing only Laplace's warm eulogy:—"Cet admirable ouvrage contient les germes de toutes les grandes découvertes qui ont été faites depuis sur le système du monde;" and further, he says, that the first step thus made by Newton in the theory must appear immense.

Huygens next appears on the scene. Our author (§§ 64, 65) clears up one or two points, more especially the rightful claim to priority of Newton over Huygens; an error which crops up in Barlow's "Mathematical Dictionary" and Svanberg's work on the Lapland Operations.

From § 48 we gather that Mr. Todhunter has not seen the *Opera reliqua*. S'Gravesande, in the preface to vol. ii. (we quote from the edition Amstel, 1728, 4to.), says, "Tractatus de lumine et dissertatio de gravitate quæ ambo scripta gallice dedit auctor, quamvis primum ut ipse in hujus præfatione monet, in linguam latinam vertere sibi proposuerat:" the second is turned into Latin with title "De causâ gravitatis." The *præfatio* occupies pp. 95, 96, *dissertatio* pp. 97-116 with an *additamentum* down to p. 136, and there is a plate: the *De vi centrifugâ* occupies pp. 107-134.

In the *Opuscula postuma* (Lugd. Bat. 1703, 4to.) the treatise *De vi centrifugâ* (pp. 401-428) is founded, if we mistake not, on a different view of gravitation from that assigned to him by Mr. Todhunter in § 50.

Miscellaneous investigations, up to the year 1720, are then considered: Burnet's "Theory of the Earth" is glanced at, Keill's examination of the same, David Gregory's writings (which contribute nothing new), Hermann, Mairan ("Misapplied Mathematics and Misplaced Ingenuity") and the Cassinis, under whose powerful influence doubts arise as to the real shape of the earth, are more fully discussed.

We proceed to Maupertuis, a memoir* by whom is said (§ 128) to be the first example of the adoption of the principle of attraction by French mathematicians. We offer here a collation of the first editions of two of his works with the second editions which Mr. Todhunter discusses (§ 143).

The *avertissement* of 3 pp. in the 1738 edition of the *Examen désintéressé* is not reproduced in the 1741 edition: the 82 pp. of text, as also the 3 pp. of contents, at end, and the one page of errata, appear to be identical with the matter in the later edition. For the *Examen des trois*, &c., the bookseller's *avertissement* (4 pp.) is common to both: the 42 pp. of text appear to be the same; there is no list of errata; the foot-notes of the later edition appear as side-notes in the 1738 copy. The copy we consulted had the two essays bound up together, and is a duodecimo volume.

In the "Philosophical Dissertations on the Uncertainty of Human Knowledge" by the Marquis d'Argens, author of the "Jewish Spy," to which is added M. Maupertuis' "Dissertation upon Gravity,"&c., translated from the French edition, in 2 vols. 1753, with the running title "The Impartial Philosopher," we have the following:—"After M. Maupertuis had examined the Newtonian system and after he had undergone infinite dangers and difficulties in the frozen regions of the North, in verifying a particular part of it, he concludes that we may look upon gravity as a power diffused through all parts of matter by which all its particles attract each other. The concurrence of all the force of matter which composes the earth, attracts and causes bodies to fall towards its surface, keeps the moon in her orb, and produces with regard to the other planets, and with respect to the sun, the like phenomena, always in proportion to the quantity of their force, their direction, and their distance" (pp. 255-263).

Whilst treating of Maupertuis, we think we have seen in the Graves' library the English translation of his "La figure de la terre . . . au cercle polaire," 1738: possibly the extract cited above is taken from it. In § 149 Mr. Todhunter says "Childrey seems" &c.: Joshua Childrey, 1623-1670, was of Magdalen College, Oxford, Archdeacon of Salisbury, 1663. He wrote "Britannia

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* London, 1775. 3rd edit. 8vo

† Florentiæ, 1666. 4to.

* "Sur les loix de l'attraction:" cf. Bailly.

Baconica, or the Natural Rarieties of England, Scotland, and Wales, according as they are to be found in every Shire." (London, 1660. 8vo.) The passage referred to in the *Examen désintéressé* we presume was taken from the Paris edition (1667. 12mo.), under Carnarvonshire (pp. 244, 245 of the French, pp. 147, 148 of the English edition). In his dedication he writes:—"The calling I have entred into, and the capacity * wherein I have the honour to serve your Lordship, wil (I fear) offend the weake tendernesse of some, who think these deep searches into reason misbecoming a Preacher of Faith, and the contemplation of the works of Nature very impedimentall (if not destructive) to the work of Grace," &c.

Stirling (whom his rival Clairaut calls "one of the greatest geometricians I knew in Europe") enunciated without demonstration approximate propositions respecting the magnitude and the direction of the attraction of an homogeneous *oblatum* at its surface and *implicitly* (§ 156) established Newton's *postulate*.

We now proceed to give an account of the original work (not seen by Mr. Todhunter) entitled "Dégré du méridien entre Paris et Amiens." (Paris, 1740.) It has 6 pp. of contents:—lvi. pp. of Part I. in nine chapters, with 3 pp. of plates. Part II. is "Mesure de la terre" par M. l'Abbé Picard, 106 pp., with 10 pp. on Aberration of fixed stars, and 5 pp. of fixed plates. On p. vi. we find, "le dégré comparé à celui que nous avons mesuré au cercle polaire, que nous avons trouvé de 57437, 9 toises donne la terre aplatie vers les pôles; et le rapport de l'Axe au diametre de l'equateur, comme 177 à 178." We had some difficulty in finding the book in the Museum from Mr. Todhunter's description; at last we found it catalogued under the heading Picard.

The Museum copy of the Essay by Celsius (§§ 198, 739, not seen by our author), entitled "De Observationibus," &c., Upsaliæ, 1738, is bound up with several other tractates on our subject, but all the rest partake of the character of the ante-Newtonian writers. Thus Nicolaus Winterberg (1596) heads his chapters—"Rotundam esse (terram) liquido apparet;" "Terram cum aquâ conjunctam σφαιροειδῆν asserimus;" and he further maintains the earth to be the centre of system of universe. We need not give an analysis of Celsius's work here; for this is undoubtedly the original from which the German translation, discussed by Mr. Todhunter, was taken. He styles Newton "vir immortalis," and, deciding against James Cassini, thus ends his ten-page tract—"Spero itaque me jam æquo et candido lectori satis superque ostendisse observationes Cassinianas, tam cœlestes quam terrestres in Gallia præcipue meridionali habitas, adeo incertas esse et inde figura telluris nullo modo deduci queat."

We have now arrived at the period when the question between the Newtonians and Cassinians was decisively settled, and the victory of the *oblates* over the *oblongs* acknowledged even by the Cassinis. This result was brought about by the expedition to Lapland in 1736-37, and won for its ruling spirit, Maupertuis, Voltaire's witty compliment of having "aplati les pôles et les Cassinis."

R. TUCKER

(*To be continued.*)

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* He was chaplain to the "Rt. Hon. my most noble Lord and Master Henry Somerset Lord Herbert, &c."

OUR BOOK SHELF

Elements of Chemistry, Theoretical and Practical. By William Allen Miller, M.D., LL.D. Part II. Inorganic Chemistry. Revised by Herbert McLeod, F.C.S., Professor of Experimental Science, Indian Civil Engineering College, Cooper's Hill. 5th edit., with additions. (London: Longmans, 1874.)

IT will of course be superfluous to say anything in the way of criticism concerning this well-known manual. The death of its lamented author has necessitated the placing of the fifth edition in other hands, and it could not have fallen into better than those of its present editor. The principal changes so far have been a re-arrangement of the articles in accordance with the modern method of study and the removal of certain parts, such as those on gas analysis and the description of certain carbon compounds, to the appendix preparatory to their removal to the third part, to which they more strictly belong.

Some of the constitutional formulæ, now so much in use, have been introduced, and the kind selected have been those used by Frankland in his well-known "Lecture Notes." We are, however, glad to see that these have not been used to the exclusion of the notation adopted in former editions.

Great credit is due to Prof. McLeod for the thorough and conscientious way in which he has performed his task, and the only fault we have to find is that there is occasionally a certain amount of confusion caused by the use of different names for the same body, a fault for which, however, the science itself is largely responsible.

R. J. F.

Zones of Parallel Lines of Elevation in the Earth's Crust. By Angus Ross, sec. and mem. com. N. S. Inst. of Nat. Science. (Halifax, Nova Scotia, 1872.)

HE is a bold man who will predicate that no future discovered fact will disturb even the most widely accepted hypothesis. This being so, all hypotheses being in fact tentative only, and valuable in so far as they enable us to classify and deduce laws from such facts as we know, we ought to welcome every generalisation which groups known facts under some new aspect. In the above pamphlet we have such a generalisation. Whether it will prove to be supported by future discovery, or even whether it can be rigidly applied to explain actual facts will require much close criticism to determine. We can only say that it is ingenious and novel. The author claims to have discovered the method of distribution of the various mountain chains or lines of anticlinal elevation. These he asserts are arranged in parallel lines along certain belts or zones which girdle the earth, each zone following approximately the course of a great circle, and each having for its medial line or axis a line of volcanoes. Of these zones he describes seven, and we may extract one as a type of the rest. "Zone No. 1 on the Rocky Mountain system has its axial line in the volcanic belt extending from the middle Andes, inclusive, across Central America along the Rocky Mountains, Alaska, the Aleutian Islands, Kamtschatka, the Kurile Islands, Japan Islands, Loochoo Islands, Philippine Islands, Palawan, and Borneo. The Islands of Amsterdam and St. Paul, the Kerguelen Islands, the South Sandwich Islands, and South Georgia seem to indicate the completion of the more southerly part of the (approximately) great circle." The author, as we have said, describes seven such zones or belts which intersect one another, and argues that the points of intersection are foci of volcanic energy. He argues also that the great mountain-chains in their direction follow the course of one or other of these zones, and thus describes their arrangement:—"In each zone the proximity and elevation of the anticlinals diminish gradually from the axial line outwards, and if zone No. 1 be considered the most recent, and the others as successively less recent in the order in which I have named them, and comparing similar parts of any two zones, the height of the anticlinals is greater, the dip less, and the difference between their axes greater in the more recent." The pamphlet is ably written and very deserving of study.

HENRY H. HOWORTH

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Animal Locomotion

IN NATURE, vol. ix. p. 301, there is a letter from Mr. Wallace on a very important point connected with the Theory of Flight. The question he discusses is "whether a bird's wing during onward flight moves *downwards and backwards* or *downwards and forwards*;" and Mr. Wallace supports Mr. Pettigrew in affirming that the movement is *downwards and forwards*.

As this is a subject to which I have paid long and close attention, I desire to express my conviction that neither of the two motions thus described by Mr. Wallace is the true motion of a bird's wing in forward flight.

The true motion is one strictly vertical to the axis of the bird's body; and as that axis is ordinarily horizontal in flight, the wing-stroke is a vertical stroke, that is simply downwards, and neither "downwards and forwards" nor "downwards and backwards."

This is not a question of theory, but a question of fact, to be determined by observation. The wing-stroke of most birds is indeed so rapid that the eye cannot distinctly follow the operation. But there are birds whose wing is so large and whose flight is so slow, that the wing-stroke can be followed with the greatest distinctness. Such is the common heron—common, alas, no longer in most parts of England, but numerous on the west coast of Scotland. When at home I am in the daily habit of watching their flight; and the truly vertical character of the wing-stroke is a fact which I have verified by the eye under every possible condition which could supply the evidence.

There are indeed two slight modifications of the perfect perpendicularity of the stroke which result (1) from the attachment of the wing to the body of the bird, and (2) from the structure of the wing-feathers. The first of these two modifications consists in this—that as the wing moves upon a hinge, its extremity must move downwards, not absolutely vertically, but describing an arc. The segment of a circle, however, through which the wing thus moves, is generally a very short one: and in so far as the movement of the extremity departs from the vertical, it departs therefrom neither "backwards" nor "forwards," but (as it were) "inwards,"—that is, in the direction of a circle encompassing the axis of the bird's body as with a hoop. Pigeons, as an amusement and in play, often complete this circle—making their primary quills clash against each other over their backs, and downwards again under their breasts. But in ordinary forward flight, when birds are intent only on progression, the wings move through a very small arc indeed of the complete circle referred to.

The second modification of the perpendicularity of the stroke arises from the "set" of the wing-feathers—which curve backwards and downwards from the wing-bones. In some birds, and notably in the heron, and all the storks, the concavity thus formed is very deep, and of course a surface which is thus not a plane surface, but a concave one, however truly it may be struck downwards, cannot have a purely vertical reaction on the air.

When we observe, however, that in the case of many birds, and some of these the most powerful fliers in the world, this concavity of the wing-feathers is very slight indeed, and that the whole vane is very narrow, flat, and "taut," it is obvious that a purely vertical stroke, or one as near it as possible, is the really essential stroke for flight.

The great secret of flight is the exquisite and complicated adaptation of structure in the feathers of a bird's wing which derives from this one simple action the resultant of a force which is both sustaining and propelling. It is an adaptation which, when thoroughly grasped and understood, at once dispenses with as needless, and condemns as mechanically erroneous, all the explanations which assume either a "downward and forward" or a "downward and backward" movement.

I venture to think that Mr. Wallace is certainly in error when he ascribes to Mr. Pettigrew the merit of having been the first to show that "horizontal forward motion is a general resultant of the upward and downward action of the wings under the influence of gravitation."

In February 1865 I published in *Good Words* a paper on the mechanism of flight, in which this effect of the wing-stroke was fully explained, and elaborately illustrated. This paper subsequently appeared as chap. iii. in the "Reign of Law" published in the end of 1866. Mr. Pettigrew's lecture before the Royal Institution (in which I believe his views were first promulgated) was delivered on March 22, 1867. I had the pleasure of hearing that lecture, and the amusement of recognising parts of it (including even a poetical quotation) as taken directly from my chapter on flight. The pleasure, however, was somewhat abated by the strange mixture of much that was quite correct, with a great deal more which I believed then, and believe now, to be wholly erroneous.

ARGYLL

March 11

MR. WALLACE has well said that the question, How a bird's wing moves in flight, "is a very important question." In these days, when scientific attention is being directed to the problem of aerial navigation, it is especially important. I have the less hesitation, therefore, in troubling you with some further remarks in reply to the strictures of this very accurate observer.

At the outset I must deny that I assumed either that a bird's wing is inflexible or that it is a plane. Of its flexibility I had no cause for speaking at all; but so far from regarding it as a plane, I expressly objected to Dr. Pettigrew so representing it in his supposed refutation of the orthodox view. The point in dispute is entirely concerning the down stroke; against Mr. Wallace's account of the up stroke I make no objection.

First, what may we infer *à priori* concerning the down stroke? (1) Its efficiency is independent of the velocity of the bird: this is simply a consequence of the second law of motion. We have to suppose a bird fixed in still air, and to ascertain the effect which ensues on a downward blow of the wing. The subsequent forward velocity of the bird, so far as that depends on the down stroke, is but a consequence or an accumulation of these effects. It is thus only needful to analyse the single effect itself. To this end the shape and varying flexibility of the wing must be noted. Along the exterior margin we have a rigid area, comparable to the blade of an oar, and formed for the most part of bone, in the *top side* of which the rigid tubes of the primary and secondary feathers are inserted. On the under side of this, which we may term the oar part of the wing, there is thus a considerable concavity, the direction of which when the wing is extended is decidedly backward. The area towards the middle line of the wing is flat and horizontal, approximately so at all events, when the bird is freely suspended in the air. Of the posterior, the larger, half of the wing it is true, as Dr. Pettigrew says, that the aspect is forward, more especially in heavy birds with broad and rounded wings. The flexible extremities of the feathers readily turn upwards like vanes in the manner so well shown in Fig. 80 of Dr. Pettigrew's work. We may thus roughly distinguish four areas, beginning from the front: (*a*) the oar area; (*b*) the plane or flapping area; (*c*) the kite area; (*d*) the vane area. (2) Now we may inquire what will be the effect of each when the wing is struck downward. The reaction from the oar area will be (*a*) a force directed upwards and forwards; that from the plane area (*b*) a force directed upwards simply. Against the kite area will impinge the air sent backwards and rebounding from the blow of the oar area; the effect of this (*c*) is all that corresponds to what Dr. Pettigrew calls the kite action of the wing. Lastly, the same air in escaping through the feathers, and especially in raising the tips in the vane area, will produce the forward motion (*d*) to which Mr. Wallace refers, besides contributing something (*e*) to support the bird's weight. The horizontal component of (*a*) together with (*d*) will carry the bird forward. The slighter horizontal component of (*c*)—slighter because proceeding only from the rebounding air and from a yielding surface—will tend to hinder the forward motion: hence the absence, more or less complete, of this area in quick fliers. The forces (*b*), greater part of (*c*), and (*e*) will sustain the bird against gravity.

Neither Dr. Pettigrew, nor apparently Mr. Wallace, distinguishes the motion consequent on a surface striking against the air from that of a surface gliding through it. If I incline a sheet of paper to the horizon and let it slip from my hand it will descend with a similar incline towards the ground; but if, having stiffened it, I strike it against the air at the same inclination it will tend to rise in a direction at right angles to that inclination. The *blow* directed downwards and backwards must give an impetus upwards and forwards; the *still surface* so directed will glide downwards and forwards. I do not deny that if the down stroke of the bird's wing be directed backwards, *beyond a certain angle*, the resultant motion will be, as Mr. Wallace says, "obliquely downwards." But why? Because all the sustaining forces above enumerated are so seriously diminished—the horizontal and forward forces, with the exception of (*d*), being increased—that, to use Mr. Wallace's words, "the surplus vertical reaction of the down stroke over the up stroke is no longer able to overcome gravity," which converts the bird's wings for the nonce into kites as it comes sailing downwards, making but an occasional strike, now that the horizontal effect of the wings is so great, to increase the obliquity of its descent.

But within the limits of this angle, whatever they be, the effect of a downward and backward blow must, on mechanical grounds, be in general such as I have said. For clearness' sake it may not be superfluous to note an ambiguity in the expressions "downwards and backwards," "downwards and forwards;" they may apply either to the direction of the surface of the wing or to the direction of the anterior margin. I maintain only that the direction of the surface—in some wings, merely that of the anterior portion of the surface—is downwards and backwards. The anterior margin, by the contraction of the great pectoral muscle, is drawn downwards and forwards, in which, by the way, there is the further advantage that less air will escape from under the wing in front.

But, secondly, what can we *observe* as to the down stroke? (1) A fact, pointed out to me by an anatomical friend—that the great pectoral muscle which depresses the wing is inserted into a crest situated on the upper and forward side of the head of the humerus, so as to tilt the under surface of the wing slightly upwards, *i.e.* give it a backward direction. (2) If the flight of rooks, or still better of pigeons, be watched from a window towards sunset, the position of the shadows on the under side of the wings will be found pretty conclusive as to their direction. (3) The forward inclination of the wings of a bird about to alight, which shows that the motion of the wings in such a position retards flight. (4) The action of heavy land or water birds, that have to attain some momentum by the use of both feet and wings before they can rise; here surely a forward blow against the air is manifestly absurd. (5) "The highly-inclined position of a hovering bird," noticed by Mr. Wallace, and not of the bird only, but of his wings.

Mr. Wallace's closing remark is both true and sound:—"A bird's wing is a highly complex apparatus, subject to a variety of flexures and motions in every feather." Still it is possible, even probable, that all this variety is referrible to a few simple principles. It is with these alone that I have ventured to concern myself.

JAMES WARD

Trin. Coll., Camb., March 3

WITH reference to Dr. Pettigrew's letter in NATURE, vol. ix. p. 362, I cannot do better than ask him to read the two papers that I refer to in my former reply, which he has evidently not done.

March 16 A. H. GARROD

The Moon's Want of Atmosphere

YOUR very suggestive review of Messrs. Nasmyth and Carpenter's work on "The Moon" leads me to propose an explanation of the absence of a lunar atmosphere, which I do not remember to have seen anywhere. The many arguments in favour of the temperature of the lunar surface being near or at the *absolute* zero, when added to the equally probable supposition that at the *absolute* zero all matter assumes the solid form, makes nothing more probable in my mind than that it is the consolidation from cold of all the previously existing gases and vapours of the Moon which has caused its atmosphere to disappear. Prof. Frankland's theory of the frozen condition of the lunar surface is evidently different from the above, and Lord Rosse's observations on lunar radiation apply only to the direct reflection of the solar rays.

A. H. GARROD

On Volcanic Eruptions

A PASSAGE in Nasmyth's work on the "Moon" suggests, as a consequence, an explanation of volcanic eruptions that I have often given in lectures. The point to be explained is, why they are sudden and intermittent. Processes of cooling and expansion are gradual.

I postulate (1) that a solid crust is shrinking as it cools; (2) that the liquid interior expands on solidifying; (3) that the melting-point of lava is lowered by pressure.

Let us start with a volcanic vent in which the aperture has become partially stopped by cooled or solidified lava. In the region below, pressure sets in from the cooling and ultimate solidification of part of the liquid mass. Hence the melting-point of the rest is lowered by (3). The process continues until the pressure becomes sufficient to relieve itself through some vent, old or new; a lava rises in the vent. But this relieves the pressure, and it follows from (3) that more rock will solidify, *suddenly*, and in so doing force liquid rock *rapidly* up the vent.

A volcano is, in fact, a geyser of lava.

I do not remember to have seen this in any book; and it perhaps would have been hazardous to assert postulate (2) as certainly true previous to Nasmyth's experiments; but I have thought it probable: and if it is true, postulate (3) follows, I believe, from the laws of heat, and the explanation will be sound. I shall be glad to hear what is thought of it by authorities.

Rugby, March 13 J. M. WILSON

Remarks on Ozone

HAVING perused Dr. Moffatt's interesting communication on Ozone in the *Scottish Meteorological Journal* of October last, and also noticed the paragraph on the subject in the *Medical Times* drawing attention to it, I beg to send the following remarks respecting some points in it open to criticism from outsiders:—

1. The numbers, and special *years of records*, are not stated in the statistical tables, which might be of importance for comparison with other persons' records.

2. The occurrence of ozone with *hail*, and not with snow, may be explained by its happening in warmer weather, and not in winter, and in the warmer stratum of air through which the hail falls from the cold stratum above.

3. The larger quantity of ozone in Table II. in *winter over that in summer* is anomalous, and inconsistent apparently with the records in Tables VI. and VII., where it is stated to increase with the temperature.

4. If ozone be thought to increase in quantity with *increase of elevation* above the level of the sea, it may be asked how that is to be reconciled with the greater prevalence of it at the sea-shore than inland.

5. If there be only an apparent connection between *electrical storms* and ozone, explanation may be required to account for the production of artificial ozone by electrical action, and whether the two be identical in constitution if not in origin.

6. The paragraph—"The *air is drier* near the tropics than about the equator," might be more clearly defined by adding the "tropical circles of cancer and capricorn," as within those lines it certainly gradually gets more and more humid.

7. In one paragraph there is stated to be an intimate connection between *humidity* of the atmosphere and the manifestation of ozone, and in another this is stated to be purely accidental, which is ambiguous, while the testimony in support of it is not in accord with that in Tables IX. and X., where the adverse record is apparent.

8. That the absolute *humidity of the air* diminishes with increase of elevation may be true in the case of lofty balloon ascents, away from any terrestrial influences of mountains, but, as pointed out in a note, the *relative humidity* increases, as we may see in Westmoreland or Dartmoor, where the heights are always misty and damp.

9. The tropical or *trade winds* only chance to be land winds in some such regions as the North Indian Ocean, whereas they are generally said to be sea-breezes, as in the South Atlantic Ocean, in the ordinary acceptation of the term.

10. The connection of the production of ozone by the means of *turpentine* will bring to the mind of the tourist the freshness of the air of hills planted with pine forests.

11. Accepting the theory that ozone is connected with the equatorial winds, it may be asked how the increase of ozone in the *calm belts* is to be accounted for, where there are only Polar winds, converging to ascend into the upper regions of the air from north and south.

12. The table of *observations at sea* on board ship would require to be supplemented by a note of the period of the year and number of days on record, as the quantity of ozone is already stated to vary with the *seasons of the year* (Table II.), and

the winds, temperatures, and barometrical indications might have been added for the like reason

13. Table XI. also requires a note of the season of the year and the number of days of observations. It may here be asked, how is the discrepancy to be reconciled between the lessening of ozone as you sail *to the Polar Regions*, and the increase of ozone as you ascend in the air, when the temperature as regularly falls in the one case as in the other.

14 The records showing the connection between *phosphorescence* and manifestation of ozone are very satisfactorily drawn out, and may probably become of much value in a new investigation.

15. The less prevalence of ozone in the higher *extratropical latitudes* may be due, as suggested in another case, to the dryness of the atmosphere impairing the *sensitiveness of the test papers*, so that for the present such deductions are under suspicion.

16. The idea that the prevalence of ozone is coincident generally with a *low barometer* seems well supported by the observations recorded, but some explanation will be required to account for its maximum occurrence with *south-east winds* in Tables IX. and XI., if one should accept the theory of its connection only with equatorial winds.

17. That its presence may be connected with *warm temperatures* of the air seems better established at sea than on land, as also its coincidence with *humidity of the air*, though this is somewhat vitiated by the conscientious suggestion that its manifestation may be due to the increased susceptibility of the test-papers when moist.

18. In the statement that ozone increases as you ascend *mountainous elevations*, it is not stated what winds were blowing at the time, which would appear to be necessary, if the idea of its prevalence with any particular wind were considered essential.

19. The key to the origin and prevalence of ozone in the atmosphere seems still undiscovered, and we do not yet appear to have determined if it belongs to aqueous vapour or a special wind, or whether it be an additional constituent of the air, like carbonic acid, or a floating entity, like a cloud.

NUBIBUS

The Limits of the Gulf Stream

As one of those engaged in the compilation of the Atlantic pilot-charts published by the Admiralty, on which are given the limits, velocity, and general features of the Gulf Stream, as well as the boundaries of the regions in which ice and icebergs may be fallen in with in the North Atlantic, I cannot allow the letter in NATURE (vol. ix. p. 343), by W. W. Kiddle, of the White Star Mail steamship *Oceanic*, to remain unchallenged.

The Gulf Stream and ice boundaries, delineated on the North Atlantic chart, referred to in that letter, are in their details transcripts from the Atlantic pilot-charts.

These details were the result of much patient investigation, and obtained from many sources probably unknown to Captain Kiddle; among the most valuable were the painstaking and sound observations made by members of the United States Coast Survey, and to be found embodied in the annual reports between 1843 and 1859; and especially from the exhaustive and learned work on currents, so well known to cultivators of nautical science, by the late Major Rennell.

If the average boundaries of the Gulf Stream cannot be laid down within reasonable limits from the authorities I have quoted, aided, too, by the many observations of ships of war, extending over the present century, I fear that Captain Kiddle's results will not assist us in a more accurate delineation.

It is, however, to be hoped that Captain Kiddle's information on the currents may be more reliable than that he has ventured upon giving with regard to the limits of iceberg-drift; here recorded facts are irresistibly against him. He has only to consult any North Atlantic memoir on the subject, and he will find that icebergs have been fallen in with so far south as 36° 10′ N., or 7° south of the high authority he quotes. I would refer him on this interesting subject, as well as how icebergs are found on the southern edge of the Gulf Stream, and why it is possible "that bergs could drift square across the heated waters of the Gulf Stream to lat. 39° N.," to a paper by the well-known W. C. Redfield, of the United States (reprinted in the *Nautical Magazine* for 1845), who gathered the facts that have simply been utilised in the Admiralty charts.

London, March 11 THOMAS A. HULL

The Great Ice-Age

MR. GREEN, reviewing Mr. J. Geikie's work on the "Great Ice-Age" (NATURE, vol. ix. p. 318), expresses the opinion that a glacial period must have been one of intense cold. This is the general opinion, and yet I think it can be shown to rest on a misconception. If the climate at any given elevation is cold enough to form glaciers, no decrease of the winter temperature will increase their magnitude; while on the other hand a low summer temperature is shown by the facts of physical geography to be eminently favourable to glaciation. This last may almost be called an identical proposition, for permanent snow means snow which lasts through the summer.

As Mr. Croll has pointed out, there have been periods where the sun's greatest and least distances were respectively greater and less than now. He thinks that a glacial period occurred when, in the course of the precession of the equinoxes, the sun's greatest distance occurred in the winter, so as to cause a *cold winter*. I think the true theory of the glacial climate is exactly the reverse of this: that is to say, it was caused by the *cold summer* which occurred when the sun's greatest distance was in the summer.

I have stated these views at greater length in the *Journal of the Geological Society of London*, 1869, p. 350.

Old Forge, Dunmurry, Co. Antrim, J. J. MURPHY
March 8

Mars

IN a most interesting article on the planet Mars, in your issue of NATURE for Feb. 19, which has just been shown to me, the Rev. T. W. Webb directs attention to the question of the colours of Mars being due to effects of contrast or not, and says—"Nor does it seem to have been noticed that no effect of contrast has been traced in the Polar snows."

Kindly permit me to inform Mr. Webb that, in a paper on Mars in the last volume of the "Monthly Notices of the Royal Astronomical Society," I expressly state that, "on May 14, 1873, the south Polar ice appeared (in an 8½-inch silvered glass reflector, by Browning) of quite a pale sky-blue colour, evidently by contrast," and I may add that this effect I noticed also on two or three subsequent occasions.

Burton-on-Trent, March 12 EDWARD B. KNOBEL

POLARISATION OF LIGHT[*]

VI.

MENTION was made in the previous article of the bands produced in the spectra of polarised light. Beside the fact of the existence of these bands it has been found upon examination that the state of polarisation at different parts of the interval between two successive bands varies; and such an examination may be made by means of a quarter-undulation plate or a Fresnel's rhomb.

If we carefully examine the spectrum of light which has passed through a selenite, or other ordinary crystal, we shall find on turning the analyser that, commencing with two consecutive bands in position, the parts occupied by the bands and those midway between them are plane-polarised, for they become alternately dark and bright; while the intermediate parts, *i.e.* the parts at one-fourth of the distance from one band to the next, remain permanently bright. These are, in fact, circularly polarised. But it would be incorrect to conclude from this experiment alone that such is really the case, because the same appearance would be seen if those parts were unpolarised, *i.e.* in the condition of ordinary light. And on such a supposition we should conclude, with equal justice, that the parts on either side of the parts last mentioned (*i.e.* the parts separated by one-eighth of the interval between two bands) were partially polarised. But if we introduce a quarter-undulation plate between the selenite and analyser, with its axis inclined at 45° to that of the selenite, circular polarisation will be converted into plane and plane into circular. This being so, the parts which were originally banded ought to become bright and to remain bright, while those that were originally bright ought to become banded during the rotation of the analyser. The effect to the eye will consequently be a general shifting of the bands through one-fourth of the space which separates each pair. Further, as on the one hand plane polarisation is converted into circular right-handed or left-handed by two positions of the plate at right angles to one another; so on the other right-handed circular polarisation will be converted by the plate in a given position into plane polarisation having the vibrations in one direction, and left-handed into plane polarisation having the vibrations in a direction at right angles to the former. Hence, if the plate be turned through a right angle from the position first described, the band will be shifted in a direction opposite to that in which they were moved at first. In this we have evidence not only that the polarisation on either band is circular, but also that on the one side it is right-handed, while on the other it is left-handed.

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[*] Continued from p. 326.

All the phenomena hitherto described manifestly depend upon the internal structure of the crystal plate, in virtue of which it affects the vibratory movement of the ether within it differently in different directions. And seeing that most crystals, when broken, divide themselves naturally into smaller crystals having the same form, *i.e.* having their planes and edges similarly inclined, we are naturally led to conclude that the structure of these bodies may differ not so much in different parts, as along different lines or planes connected with the forms into which they break, or (as it is also described) with their planes of natural cleavage. And this suggests the question whether an uncrystalline body might not, by pressure, or strain, or other mechanical distortion, be caused to affect the motions of the ether within it in a manner dependent upon their direction, and in that way to exhibit chromatic effects with polarised light analogous to those described above. Experiment answers this question in the affirmative.

The simplest experiment in this branch of inquiry consists in taking a rectangular bar of ordinary glass; and having crossed the polariser and analyser so as to give a dark field, to strain the bar with both hands as if we were trying to bend it or to break it across. The side towards which it may be supposed to be bent is of course compressed, while the opposite is stretched out. Between these two there must be an intermediate band, more or less midway between the two, which is neither compressed nor stretched. The moment the strain is put upon the bar light will be seen to pass through the parts of the bar nearest to both sides, while a band remains dark midway between the two.

This shows that the mechanical strain has imparted to portions of the glass a structural character analogous, at all events optically, to that of a crystal. The effects may be increased and rendered more striking by placing the glass in a frame furnished with a screw, by which the rod may be firmly held and considerable pressure applied at particular points. When this is done the structural character becomes more completely developed, and the dark band is fringed with colours which appear to flow inwards or outwards according as the pressure is increased or diminished. A slightly different, but more effective, exhibition of chromatic polarisation is produced by squeezing a thick square plate of glass in a vice. In this case the pressure may be carried further without fear of fracture, and the chromatic effects heightened.

It is, however, well known that molecular forces, such as those due to heat and cooling, in many cases far transcend in intensity those which we can exert by mechanical arrangements. And, in fact, if a block of glass be unequally heated to a very moderate degree, the internal structural effects immediately reveal themselves by dark bands, which indicate the border land between strain and pressure. As the block cools, these landmarks gradually disappear, and the field becomes again uniformly dark. But by far the most splendid effects (and these are permanent) are produced by unannealed glass; that is, by glass which has been rapidly and therefore unequally cooled. When a mass of glass has been cast in a mould in the form of a thick plate, then whatever be the contour line, the outside will cool first and become a rigid framework to which the interior of the mass must accommodate itself. The nature and direction of the pressure at each point of the interior will be primarily dependent upon the form of the contour; and by adopting various forms of contour the most beautiful and varied figures with coloured compartments may be produced. The forms and colours of the figures produced by transparent bodies when submitted to polarised light have been conversely used as a means of measuring, with almost unparalleled accuracy, the mechanical pressures which such a body is undergoing.

Besides glass many other substances may be used as reflectors so as to produce polarisation; for example, leaves of trees, particularly ivy, mahogany furniture, windows, shutters, and often roofs of houses, oil paintings, &c., and last but not least the surface of water. In each of these cases when the reflected beam is examined with a Nicol the alternations of light and darkness are most strongly marked, and the colours (if a crystal plate be used) are most vivid, or in technical language the polarisation is most complete, when the light is reflected at a particular angle. In proportion as the inclination of the incident light deviates from this angle the colours become fainter, until when it deviates very greatly all trace of polarisation disappears.

It will be found very interesting to examine the polarisation of sunshine reflected from ripples on the surface of a lake, or better still from the waves of the sea, and its different degrees of completeness produced at the variously inclined portions of the waves. But without having recourse to nature on so large a scale, an artificial piece of water may be placed in our room. A tea tray will serve as well as anything else to form our little sea; and a periodic tap at one corner will cause ripple enough for the present purpose. The waves appear bright, and although brighter in some parts than others they are nowhere entirely dark. But on turning the Nicol round the contrast of light and darkness becomes much stronger than before. In parts the light is absolutely extinguished, or the polarisation is complete; in others it is incomplete in various degrees. And if a selenite or other crystal plate be introduced we have the beautiful phenomena of iris-coloured rings playing over the surface of our miniature sea.

Suppose that we now turn our attention to the sky, and on a clear bright day we sweep the heavens with a polariscope, or even with a mere Nicol's prism, we shall find traces of polarisation in many directions. But if we observe more closely we shall find that the most marked effects are produced in directions at right angles to a line drawn from our eye to the sun, when in fact we are looking across the direction of the solar beams. Thus, if the sun were just rising in the east or setting in the west, the line of most vivid effect would lie on a circle traced over the heavens from north to south. If the sun were in the zenith, or immediately overhead, the most vivid effects would be found on the horizon; while at intermediate hours the circle of strongest polarisation would shift round at the same rate as the shadow on a sun-dial, so as always to retain its direction at right angles to that of a line joining ourselves and the sun.

Now, what is it that can produce this effect, or indeed, what produces the effect of light from all parts of a clear sky? The sky is pure space with no contents, save a few miles of atmosphere of the earth, and beyond that the impalpable ether, supposed to pervade all space, and to transmit light from the furthest limits of the stellar universe. The ether is however certainly inoperative in the diffusion of light now under consideration. But a very simple experiment will suffice to show that such a diffusion or, as it has been better called, a scattering of light, is due to the presence of small particles in the air. If a beam from an electric lamp or from the sun be allowed to pass through a room its track becomes visible by its reflection from the motes of floating bodies, in fact by the dust in the air. But if the air be cleared of dust by burning it with a spirit lamp placed underneath, the beam disappears from the parts so cleared, and the space becomes dark. If, therefore, the air were absolutely pure and devoid of matter foreign to it, the azure of the sky would no longer be seen and the heavens would appear black; the illumination of objects would be strong and glaring on one side, and on the other their shadows would be deep and unrelieved by the diffused light to which we are accustomed. Now, setting aside the dust, there are always minute particles of water floating in the atmosphere. These vary in size from the great raindrops which fall to earth on a sultry day, through intermediate forms of mist and of fine fleecy cloud, to the absolutely invisible minuteness of pure aqueous vapour which is present in the brightest of skies. It is these particles which scatter the solar rays and suffuse the heavens with light. And it is a remarkable fact, established by Prof. Tyndall, while operating with minute traces of gaseous vapours, that while coarser particles scatter rays of every colour, in other words scatter white light, finer particles scatter fewer rays from the red end of the spectrum, while the finest scatter only those from the blue end. And in accordance with this law clouds are white, clear sky is blue.

But the point which most concerns us here is the fact, also discovered by Prof. Tyndall, that light scattered laterally from fine particles is polarised. The experiment by which this is most readily shown is as follows: Allow a beam of solar or other strong light to pass through a tube about thirty inches long filled with water, with which a few drops of mastic dissolved in alcohol have been mixed. The fluid so formed holds fine particles of mastic in a state of suspension, which scatter the light laterally; and if the scattered light be examined with a Nicol traces of polarisation will be detected. But better still, instead of using the scattering particles as a polariser and the Nicol as an analyser, we may polarise the light before it enters the tube and use the particles as an analyser, and thus produce the same effect as before, not only upon the particular point of the beam to which the eye is directed, but upon the whole body of scattered light. As the Nicol is turned the light seen laterally begins to fade; and when the instrument has been turned so as to cut off all vertical vibrations, the only parts remaining visible in a horizontal direction will be those reflected from the larger impurities floating in the water independently of the mastic. The direction of vibration of the light polarised by lateral scattering is easily remembered by the fact that the vibrations must be perpendicular both to the original and to the scattered beam; if, therefore, the latter be viewed horizontally, they must be perpendicular to two horizontal straight lines at right angles to one another, *i.e.* they must be vertical.

An effect still more beautiful, and at the same time perhaps more instructive, may be produced by interposing a plate of quartz between the Nicol and the tube. The whole beam then becomes suffused with colour, the tint of which changes for a given position of the spectator with the angle through which the Nicol is turned.

And not only so, but while the Nicol remains at rest the tints are to be seen scattered in a regular and definite order in different directions about the size of the beam. But this radial distribution of colours may also be shown in a more striking manner, by using a bi-quartz, which as explained before distributes the colours in opposite directions. The beam should in every case be viewed at right angles; the more obliquely it is viewed the less decided is the polarisation.

The colours here seen are those which would be observed upon examining a clear sky in a position 90° from that of the sun; and the exact tint visible will depend upon the position in which the Nicol is held, as well as upon that of the sun. Suppose, therefore, that a Nicol and quartz plate be directed to that part of the sky which is all day long at right angles to the sun, that is, to the region about the north pole of the heavens (accurately to the north pole at the vernal and autumnal equinox), then if on the one hand the Nicol be turned round, say, in a direction opposite to that of the sun's motion, the colours will change in a definite order; if, on the other, the Nicol remain stationary while the sun moves round, the colours will change in a similar manner. And thus, in the latter case we might conclude the position of the sun, or in other words the time of the day, by the colours so shown. This is the principle of Sir Charles Wheatstone's Polar clock, which is one of the few practical applications which this branch of polarisation has yet found.

Figs. 18 and 19 represent general forms of this instrument described in the following passage by the inventor.

"At the extremity of a vertical pillar is fixed, within a brass ring, a glass disc, so inclined that its plane is perpendicular to the polar axis of the earth. On the lower half of this disc is a graduated semicircle divided into twelve parts (each of which is again sub-divided into five or ten parts), and against the divisions the hours of the day are marked, commencing and terminating with VI. Within the fixed brass ring, containing the glass dial plate, the broad end of a conical tube is so fitted that it freely moves round its own axis; this broad end is closed by another glass disc, in the centre of which is a small star or other figure, formed of thin films of selenite, exhibiting when examined with polarised light strongly contrasted colours; and a hand is painted in such a position as to be a prolongation of one of the principal sections of the crystalline films. At the smaller end of the conical tube a Nicol's prism is fixed so that either of its diagonals shall be 45° from the principal section of the selenite films. The instrument being so fixed that the axis of the conical tube shall coincide with the polar axis of the earth, and the eye of the observer being placed to the Nicol's prism, it will be remarked that the selenite star will in general be richly coloured, but as the tube is turned on its axis the colours will vary in intensity, and in two positions will entirely disappear. In one of these positions a smaller circular disc in the centre of the star will be a certain colour (red, for instance), while in the other position it will exhibit the complementary colour. This effect is obtained by placing the principal section of the small central disc 22½° from that of the other films of selenite which form the star. The rule to ascertain the time by this instrument is as follows:—the tube must be turned round by the hand of the observer until the colour star entirely disappears while the disc in the centre remains red; the hand will then point accurately to the hour. The accuracy with which the solar time may be indicated by this means will depend on the exactness with which the plane of polarisation can be determined; one degree of change in the plane corresponds with four minutes of solar time.

"The instrument may be furnished with a graduated quadrant for the purpose of adapting it to any latitude; but if it be intended to be fixed in any locality, it may be permanently adjusted to the proper polar elevation and the expense of the graduated quadrant be saved; a spirit-level will be useful to adjust it accurately. The instrument might be set to its proper azimuth by the sun's shadow at noon, or by means of a declination needle; but an observation with the instrument itself may be more readily employed for this purpose. Ascertain the true solar time by means of a good watch and a time equation table, set the hand of the polar clock to correspond thereto, and turn the vertical pillar on its axis until the colours of the selenite star entirely disappear. The instrument then will be properly adjusted.

"The advantages a polar clock possesses over a sun-dial are :—1st. The polar clock being constantly directed to the same point of the sky, there is no locality in which it cannot be employed, whereas, in order that the indications of a sun-dial should be observed during the whole day, no obstacle must exist at any time between the dial and the places of the sun, and it therefore cannot be applied in any confined situation. The polar clock is consequently applicable in places where a sun-dial would be of no avail; on the north side of a mountain or of a lofty building for instance. 2ndly. It will continue to indicate the time after sunset and before sunrise; in fact, so long as any portion of the rays of the sun are reflected from the atmosphere. 3rdly. It will also indicate the time, but with less accuracy, when the sky is overcast, if the clouds do not exceed a certain density.

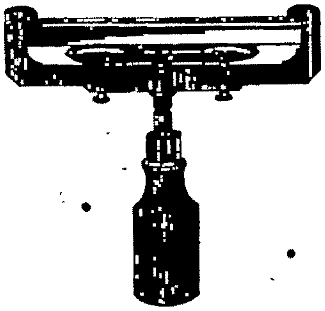


FIG. 16.

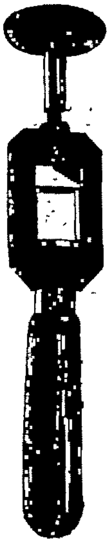


FIG. 17.

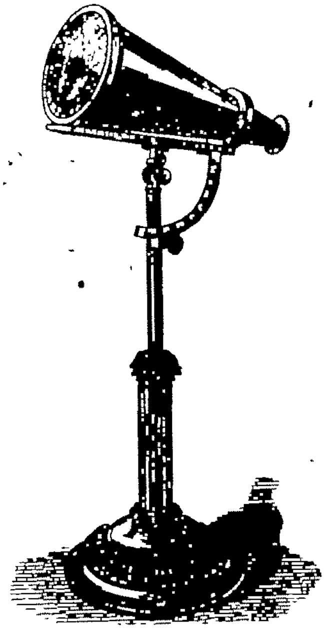


FIG. 18.—Wheatstone's Polar Clock.

"The plane of polarisation of the north pole of the sky moves in the opposite direction to that of the hand of a watch; it is more convenient therefore to have the hours graduated on the lower semicircle, for the figures will then be read in their direct order, whereas they would be read backwards on an upper semicircle. In the southern hemisphere the upper semicircle should be employed, for the plane of polarisation of the south pole of the sky changes in the *same* direction as the hand of a watch. If both the upper and lower semicircles be graduated, the same instrument will serve equally for both hemispheres."

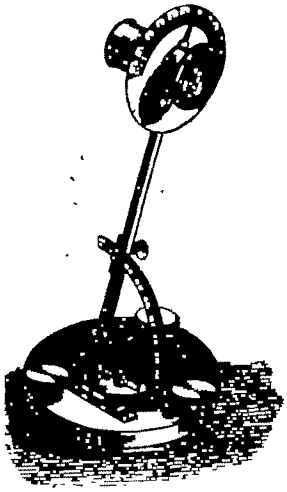


FIG. 19.—Wheatstone's Polar Clock.

The following is a description of one among several other forms of the polar clock which have been devised. This (Fig. 20) though much less accurate in its indications than the preceding, beautifully illustrates the principle.

"On a plate of glass twenty-five films of selenite of equal thickness are arranged at equal distances radially in a semicircle; they are so placed that the line bisecting the principal sections of the films shall correspond with the radii respectively, and figures corresponding to the hours are painted above each film in regular order. This plate of glass is fixed in a frame so that its plane is inclined to the horizon 38° 32′, the complement of the polar elevation; the light passing perpendicularly through this plate falls at the polarising angle 56° 45′ on a reflector of black glass, which is inclined 18° 13′ to the horizon. This apparatus being properly adjusted, that is so that the glass dial-plate shall be perpendicular to the polar axis of the earth, the following will be the effects when presented towards an unclouded sky. At all times of the day the radii will appear of various shades of two complementary colours, which we will assume to be red and green, and the hour is indicated by the figure placed opposite the radius which contains the most red; the half-hour is indicated by the equality of two adjacent tints."

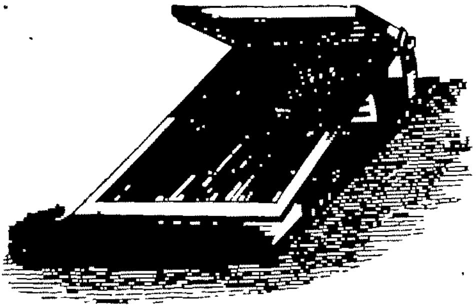


FIG. 20.—Polar Clock.

W. SPOTTISWOODE

(*To be continued.*)

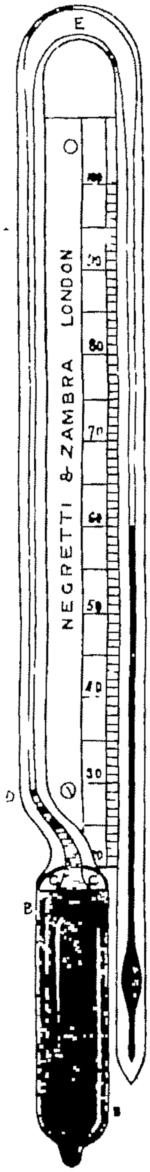
A NEW THERMOMETER

OUR readers will doubtless recollect a recent discussion in our pages relative to the priority of the invention of protected bulbs for deep-sea thermometers. The discussion has done something more than establish priority of invention, it has been the means of producing what, we believe, will prove to be a new and valuable meteorological instrument, for we have before us a paper by Messrs. Negretti and Zambra, communicated to the Royal Society by Dr. Carpenter at their last meeting, describing a new thermometer of such novel construction that it cannot fail to interest all scientific persons, meteorologists especially. We regret our inability, owing to want of space, to reproduce the paper in its entirety. The following are the main points of this communication.

In Prof. Wyville Thomson's "Depths of the Sea," p. 299, occurs the following passage:—"I ought to mention that in taking the bottom temperature with the Six's thermometer the instrument simply indicates the lowest temperature to which it has been subjected; so that if the bottom water were warmer than any other stratum through which the thermometer had passed, the observations would be erroneous."

Undoubtedly no other result could be obtained with the thermometers now in use, for unfortunately the only thermometer available for the purpose of registering temperature and bringing those indications to the surface, is that which is commonly known as the Six's thermometer—an instrument acting by means of alcohol and mercury, and having movable indices with delicate springs of human hair tied to them. This form of instrument registers both maximum and minimum temperatures, and as an ordinary out-door thermometer it is very useful; but it is unsatisfactory for scientific purposes, and for the object for which it is now used (viz. the determination of deep-sea temperatures) it leaves much to be desired. Thus the alcohol and mercury are liable to get mixed in travelling, or even by merely holding the instrument in a horizontal position; the indices also are liable either to slip if too free, or to stick if too tight. A sudden jerk or concussion will also cause the instrument to give erroneous readings by lowering the indices if the blow be downwards, or by raising them if the blow be upwards. It was on reading the passage in the book above referred to that it became a matter of serious consideration with Messrs. Negretti and Zambra, whether a thermometer could be constructed which could not possibly be put out of order in travelling, or by incautious handling, and which should be above suspicion and perfectly trustworthy in its indications. This was no very easy task. But the instrument submitted to the Fellows of the Royal Society seems to fulfil the above onerous conditions, being constructed on a plan different from that of any other self-registering thermometer; and containing, as it does, nothing but mercury, neither alcohol, air, nor indices. Its construction is most novel, and may be said to overthrow our previous ideas of handling delicate instruments, inasmuch as its indications are only given by upsetting the instrument. Having said this much, it will not be very difficult to guess the action of the thermometer; for it is by upsetting or throwing out the mercury from the indicating column into a reservoir at a particular moment and in a particular spot, that we obtain a correct reading of the temperature at that moment and in that spot.

The thermometer in shape is like a syphon with parallel legs, all in one piece, and having a continuous communication, as in the annexed figure. The scale of the thermometer is pivoted on a centre, and being attached in a perpendicular position to a simple apparatus (which will be presently described), is lowered to any depth that may be desired. In its descent the thermometer acts as an ordinary instrument, the mercury rising or falling according to the temperature of the stratum through which it passes; but so soon as the descent ceases, and a reverse motion is given to the line, so as to pull the thermometer to the surface, the instrument turns once on its centre, first bulb uppermost, and afterwards bulb downwards. This causes the mercury, which was in the left-hand column, first to pass into the dilated siphon bend at the top, and thence into the right-hand tube, where it remains, indicating on a graduated scale the exact temperature at the time it was turned over. The woodcut shows the position of the mercury *after* the instrument has been thus turned on its centre. A is the bulb; B the outer coating or protecting cylinder; C is the space of rarefied air, which is reduced if the outer casing be compressed; D is a small glass plug on the principle of Negretti and Zambra's Patent Maximum Thermometer, which cuts off, in the moment of turning, the mercury in the column from that of the bulb in the tube, thereby ensuring that none but the mercury in the tube can be transferred into the indicating column; E is an enlargement made in the bend so as to enable the mercury to pass quickly from one tube to another in revolving; and F is the indicating tube, or thermometer proper. In its action, as soon as the thermometer is put in motion, and immediately the tube has acquired a slightly oblique position, the mercury breaks off at the point D, runs into the curved and enlarged portion E, and eventually falls into the tube F, when this tube resumes its original perpendicular position.



The contrivance for turning the thermometer over may be described as a short length of wood or metal having attached to it a small rudder or fan; this fan is placed on a pivot in connection with a second; on the centre of this is fixed the thermometer. The fan or rudder points upwards in its descent through the water, and necessarily reverses its position in ascending. This simple motion or half turn of the rudder gives a whole turn to the thermometer, and has been found very effective.

Various other methods may be used for turning the thermometer, such as a simple pulley with a weight which might be released on touching the bottom, or a small vertical propeller which would revolve in passing through the water.

Messrs. Negretti and Zambra in their paper merely mention the new thermometer as being available for deep-sea temperatures; but we believe it will prove to be of great value on land; for with this thermometer we are at once provided with the means of making observations which will solve some of the most interesting questions connected with atmospheric temperature. At present we do not possess a *simple* instrument, in fact none at all which will automatically record *out of doors* the exact temperature at fixed periods; we read of the temperature being so many degrees of heat or cold yesterday or last night, but we have no means of recording how cold it was (say) at midnight, or how warm at midday, except by actually watching the instrument at those hours. With the new thermometer in connection with an inexpensive time-piece, we can ascertain and record the exact temperature at any hour it may be deemed desirable, and by its means, and with experiments carried over some period of time, we may be able to determine with a degree of accuracy hitherto only approximately arrived at, which are the coldest or warmest periods of the days or nights.

ON SOME RECENT ASTRONOMICAL SPECULATIONS IN THEIR RELATION TO GEOLOGY*

I HAVE called my subject *speculations*, because in the present state of the inquiry there are so many questions that can be looked upon in no other light. At the same time it appears to me very desirable that certain facts should be examined from this new point of view, if only to lead to researches which otherwise would not have claimed attention. What I then propose is to consider the bearing on certain geological questions of the new views of Mr. Lockyer respecting the constitution of matter, as indicated by a comparison of the spectra of the various classes of stars, and the probable effects of a change in the constitution of our sun.†

Sir W. Thomson has contended that the sun cannot have continued to give out heat and light for so long a period as has been assumed by many geologists, and has concluded that it was "on the whole most probable that it has not illuminated the earth for 100 millions of years, and almost certain that it has not done so for 500 millions."‡ Prof. Huxley made this question the subject of his address to the Geological Society in 1869, but the argument on both sides was on the supposition that the constitution of matter is such that from the earliest epoch the heat and light given off had been derived mainly, if not entirely, from the simple cooling of a heated body. If, however, Mr. Lockyer's views be true, the sun at the earliest period must have consisted of matter in a more dissociated condition than at present, and, as he points out, in combining so as to give rise to other so-called elementary substances, probably a large extra amount of heat and light would be set free. The result of this appears to me to be that when the general temperature was that at which such a dissociation occurs, the sun's energy would continue nearly the same for a period which in the present state of our knowledge cannot be determined, but which would probably be of vast duration; and not only so, but the cooling would be more uniform from the first, and not subject to so great a variation as would occur in the case of an intensely-heated body cooling without any physical change in its constituents. If this be so, the length of time during which our globe may have been receiving such an amount of heat and light as would be compatible with the existence of animals and plants may well have been as great as that demanded by any of the supporters of evolutionary theories.

Though there would be such an approximate uniformity for a vast period, yet still at the earliest epoch, the physical state of the sun would not have been the same as now, and it becomes important to consider what effect this may have produced on life on the globe. According to Mr. Lockyer's views the sun at an early period had much the same physical constitution as the stars of the type of Sirius, giving off light of a whiter or bluer character, *i.e.* the rays at the blue end of the spectrum were relatively stronger than at present, whilst in future ages they would become more feeble, and the sun pass into the condition of stars of the red type. What then would be the effect of the greater intensity of the rays at the blue end of the spectrum on animals and plants at early geological epochs? This question clearly indicates the importance of future experimental inquiries, directed to this particular subject, but at the same time it may be well to consider the bearing of what is already known. In the present state of our knowledge no facts seem more likely to help towards a conclusion than those connected with the distribution in plants of the more important of the coloured substances which absorb different rays of light. I have found that there is an intimate relation between their optical and chemical characters, and that these are also related to the development of the individual plants, and to the structural development of mature plants of different classes. Taken as a whole, in advancing from a more rudimentary condition, there is in each case a farther and farther departure from such colouring-matters as can be formed artificially, and a relatively greater and greater production of those which are more and more easily decomposed by light, when not protected by the constructive energy of the living plants. This destructive action is due relatively more to the rays at the blue end of the spectrum, whilst, at all events in the case of chlorophyll, the production depends more upon the yellow rays. Hence, by relatively increasing the intensity of the blue rays the destructive force would be relatively increased, and the constructive force relatively diminished. We may, perhaps, therefore conclude that bluer light would be relatively more favourable to the higher classes of plants when in the early stage of their growth, and to the lower than to the higher classes when in the mature condition requisite to insure permanent reproduction. The former conclusion is borne out by Mr. Robert Hunt's experiments, which showed that whilst the rays at the blue end of the spectrum quicken the germination of the higher classes of plants, it is the rays at the extreme red end which facilitate their flowering and the perfecting of the reproductive organs.* The effect of differently coloured light on the growth of the cryptogamia has not, I believe, been examined; but, if the principles involved in the above arguments be correct, they would lead us to conclude that at an early epoch in the history of our globe the bluer light of the sun would be relatively more favourable to the growth of larger cryptogams than to that of phænogams. The arguments I have used do, however, involve so many new and imperfectly-tried general principles, that it would be very premature to say that the characteristic peculiarities of the vegetation of the earlier geological periods depended on this cause, and all that I contend is that the question deserves to be examined from this new point of view, since it may at all events assist in arriving at a true explanation.

THE "CHALLENGER" EXPEDITION†

II.

FERNANDO NORONHA

THIS group of islands was visited by the *Challenger* on September 1 and 2, 1873. They consist of a principal island, about four miles long, and three-and-a-half broad, stretching about N.E. and S.W., and several smaller ones at the eastern extremity, known as Platform Island, Booby Island, St. Michael's Mount, Egg Island, and Rat Island. They are situated in the Atlantic, in lat. 30° 50′, about 200 miles from the nearest point of the South American coast, their entire length being about seven miles. The principal island is generally of a volcanic character, and hilly, the highest hill being about 600 feet. On its northern coast rises to a height of 1,000 feet what is known as the Peak. It is a peculiar-looking mass of bare rock, the summit of which is entirely devoid of vegetation, and quite inaccessible. The cliffs are composed of columnar basalt. At the eastern end of the island are some sand rocks, like that of Bermuda, and dunes of calcareous sand also occur. St. Michael's Mount is a cone 300 feet high, composed of a mass of phonolith. The remaining islands are flat, composed of sandstone, with volcanic particles.

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* An abstract of a paper read before the Sheffield Literary and Philosophical Society, Feb. 3, 1874, by H. C. Sorby, F.R.S.

† *Comptes Rendus*, Dec. 8, 1873.

‡ Brit. Ass. Report, 1861, p. 28.

* Brit. Ass. Report, 1843, p. 35.

† These Notes are founded on letters sent home by Mr. H. N. Mosely.

It was in the dry season, which extends from July to December, that the *Challenger* visited these islands; this season, however, is not one of absolute drought, parching up everything, but there are occasional heavy rains. Trees abound on the higher parts of the island, where the land has not been cleared for cultivation, or where the convicts have not felled them for making their fishing-boats or rafts, the largest trees, it is said, having all disappeared for this purpose. Numerous creepers cluster together in the branches of the trees. At the western extremity of the island the vegetation is thickest and richest, and apparently of a virgin character. *Jatropha gossypifolia* L., a large shrubby plant, common in the West Indies, and also growing in Bahia, Mexico, and New Granada, was very abundant; it was in full flower, but its only foliage were tufts of young leaves just beneath the inflorescence, so that its bare stems were conspicuous among the green creepers. The plant was also found on St. Michael's Mount and Rat Island. Another euphorbiaceous-looking plant, with stout thorns, was found on the principal island, but not on any of the others. A thorny acacia also grew on the shore; and climbing round almost every tree was *Abrus precatorius* L., one of the commonest of tropical plants, and so well known for its pretty brilliant scarlet and black seeds, which are used everywhere for necklaces, and other ornamental purposes, and in India as a standard weight. This plant, however, grew only on the main island. *Ipomœa pes-capræ* Sw. is abundant on the sand hills, and upon it and most of the other low-growing plants, *Cuscuta americana* L., spreads amazingly. A species of *Cereus* was abundant on the cliffs, but only one grass (*Oplismenus colonus* H.B.) was found on the main island.

Trees, bushes, and creepers cover the upper part of St. Michael's Mount, which is, to a certain extent, inaccessible, and, moreover, being so small, offers no room for cultivation; therefore there is no reason to suspect that the plants found upon it are attributable to any other than a natural origin. *Capparis cynophallophora* L. grows in abundance on the summit of the mount. It is a tree with a stem 8 or 9 inches in diameter, and dark green oval lanceolate leaves. A species of Ficus with aerial roots grows in favourable spots, and there forms a tree of considerable size; one is mentioned as having a trunk 30 feet high, and 18 inches in diameter. On Rat Island the same species of Ficus was also found down near the sea level, where, instead of forming a tree, it becomes a low spreading bush, not more than 5 or 6 feet high. From the natural exposure of this island to the full force of the wind, all the plants growing here, which are mostly leguminous and euphorbiaceous, mingled with cucurbitaceous creepers, are stunted in their growth. Although shady moist places occur about St. Michael's Mount, neither on this nor on the main island were any ferns, mosses, or hepaticæ found. Lichens also are very scarce.

Among the principal cultivated fruits are bananas and melons, the latter being very plentiful, and of a peculiarly fine flavour. Grapes grow well, but are not cultivated at the present time. Sugar-cane, cassava, maize, sweet potatoes, &c. are also grown in large quantities.

Animal life is singularly scarce, two lizards being the only animals recorded from Fernando Noronha, one of which is peculiar to the island, the other being found also in North America.

NOTES

THE following intelligence with regard to the late Dr. Livingstone, sent by Dr. Kirk, appears in the *Times* of Tuesday:—"Lieut. Murphy, in a note addressed to me from M'pwapwa, a place about ten days' journey from the coast, and dated the 20th of January last, says that he was then accompanying the body, and expected to reach Bagamoio, a seaport, on or about the 14th ult. Capt. Sheffe, of the Austrian ship-of-war *Helgoland*, had proceeded to the coast, and would at once convey the body and Lieut. Murphy's party to Zanzibar on their arrival. Lieut. Cameron had set out for Ujiji to recover papers left there by Dr. Livingstone. Lieut. Murphy had been in communication with him subsequent to the death of Dr. Dillon, and was sorry to find that great difficulties impeded his onward progress, owing to the antagonism of native chiefs and the desertion of many of his followers on the road from Unyanyembe to Ujiji. Chuma, who for eight years accompanied the Doctor in his wanderings, I learn had been into Zanzibar. He seems to place the position of Dr. Livingstone's death at the north of Lake Bangweolo, on or about the 4th of May, 1873. He was probably on his way westward. A reply to the official telegram, regarding the disposal of the body on arrival, was anxiously expected."

WE learn that Mrs. Arnott, the widow of the late Dr. Neil Arnott, has written to Dr. Lyon Playfair, the member for the University of Edinburgh, offering 1,000*l*. for the promotion of Natural Philosophy in that University.

THE trustees of the late Dr. Andrew Bell, the founder of the Madras School, St. Andrews, have placed at the disposal of the Senatus of the University of St. Andrews, his native city, a considerable sum towards the endowment of an Education Chair during the present session. The Senatus has had under consideration the subject of a teacher's degree, and a programme relating to its institution has been laid before the Education Department of the Government.

AT a meeting of the Sedgwick Memorial Committee held at Cambridge on the 11th inst., Prof. Humphry in the chair, it was stated by the Treasurer that the subscriptions exceeded 10,000*l*., and that more than 7,000*l*. had been paid into the account of the fund at the several banks. The question of the site of the new Geological Museum, which is to constitute the memorial, was discussed, and the feeling of the committee was in favour of the space in front of the New Museum and Pembroke Street.

THE Italian Government has determined to send out four expeditions for the observation of the Transit of Venus, the main instrument of inquiry depended upon being the spectroscope. On the other hand, for reasons not far to seek, no spectroscopes are to be employed by the English parties. Truly "they manage these things better in France," and not only in France, but in America and Italy.

TO-NIGHT (Thursday) Mr. Dewar's lecture On Dissociation will be given before the Chemical Society.

AT the annual meeting of the trustees of the Museum of Comparative Zoology, Cambridge, U.S., held in January, a committee reported that to carry out the plan inaugurated by Prof. Agassiz, a considerably larger endowment will be necessary, and that the funds now on hand are not sufficient to conduct operations on the present scale later than April 1, after which, unless an additional income of 15,000 dols. can be secured, it will be necessary to greatly reduce the scale of work. 30,000 dols per annum is estimated as being the least sum on which the establishment can be maintained on a satisfactory scale. Efforts are now being made to secure an endowment of 300,000 dols., of which about 65,000 had been contributed at a recent date.

THE Duke and Duchess of Edinburgh, on their visit to the Zoological Gardens on Sunday last, desired to have their attention specially directed to a deer sent from Manilla by his Royal Highness as a present. When it arrived in this country, in May 1870, Mr. Sclater, F.R.S., the Secretary to the Society, immediately recognised that it belonged to a species hitherto quite unknown, and he accordingly named it *Cervus alfredi* or Prince Alfred's Deer. It is a very interesting fact that this specimen, and one of the opposite sex subsequently purchased by the Zoological Society, together with a young one born in the Gardens, are the only examples that have at any time been obtained of this particularly well-marked species of deer.

THERE will be an election at Worcester College, Oxford, in June, to three Scholarships, one of which will be in Natural Science. Particulars can be had on application to the senior tutor. At the same time there will be an election at Magdalen College to not less than four Demyships and one Exhibition. Of the Demyships, one at least will be Mathematical, and one at least in Natural Science; the Exhibition will be in Mathematics. The stipend of the Demyships is 95*l.* per annum, and of the Exhibition 75*l.*, tenable for five years. For particulars apply to the senior tutor.

THE Board of Trade have been informed by the Meteorological Committee that they are now prepared to re-introduce the use of Admiral Fitzroy's signals (cones and drum) with slightly modified significations, and that the change will take effect on and after March 15, 1874. The signals to be used will consist of:—1°. Cone, point downwards for southerly gales: S.E. round by S. to N.W. 2°. Cone, point upwards for northerly gales: N.W. round by N. to S.E. 3°. Drum *with cone*, to indicate the probable approach of a *very heavy gale* from the direction indicated by the cone. The drum will not be used without the cone. The signals are to be kept hoisted *during the daylight only*, until 48 hours have elapsed from the time *the telegram was despatched*, unless countermanded. At night lanterns may be used wherever the local authorities deem it advisable to do so, as pointed out in an explanatory pamphlet, copies of which are supplied for gratuitous distribution. It will be seen from the pamphlet in question, that the meaning of the signals is that an atmospherical disturbance exists (which will be explained in the telegram) and will probably, but not *necessarily*, cause a gale at the place warned *from the direction* indicated by the signal. The Meteorological Office will supply the canvas shapes and lanterns to such places as require them, on loan; but in all cases the local authorities must undertake the charges incidental to the hoisting of the signal, such as flagstaff and gear, oil, &c., and also to the keeping of the apparatus in repair, painting, &c.

M. G. TISSANDIER, the editor of *La Nature*, is completing a series of observations for calculating the amount of atmospheric dust falling each day. The mean found is said to be several pounds in twelve hours for a surface not larger than the Champ de Mars, rather less than a half a square mile.

THIRTY-SEVEN small planets have been discovered in the years 1872 and 1873, or 18½ for each year, making 1,850 per century. From the days of Hipparchus to the present time we may reckon 2,000 years; had astronomers worked with the same zeal and success during these 2,000 years, the number of small planets known would have amounted to 37,000, only three times the number given by Arago of stars up to the 7th magnitude, and a very small proportion of the stars of the 10th magnitude. Although very minute, the latter are generally much brighter than small planets as seen at the time of opposition.

PROF. O. C. MARSH has made out some interesting points in connection with the remains of equine forms in the North American tertiaries. Following up the genealogy of the horse, as traced by Prof. Huxley in the European remains, he has been able to show that the American deposits present even a more complete series of intermediate forms. Between *Orohippus agilis* of the Eocene, which was about the size of a fox, and had four toes on the fore foot, with three behind, and *Equus fraternus* of the Pleistocene, which is not osteologically distinguishable from the existing *Equus caballus*, the following genera form the connecting links in form, size, and antiquity, viz. *Miohippus* and *Anchitherium* of the Miocene, and *Anchippus*, *Hipparion*, *Protohippus* and *Pliohippus* of the Pliocene. "Considering the remarkable development of the group through the entire Tertiary period, and its existence even later, it seems very strange that none of the species should have survived, and that we are indebted for our present horse to the old world."

IN continuation of his exquisite researches on the phenomenon of flight (*Comptes Rendus*, January 12, 1874), M. Marey has made a series of observations which prove how important a part the onward movement of a bird plays in increasing the efficiency of each wing stroke. For supposing that in its descent the wing did not continually come in contact with a fresh volume of air, it would act at a disadvantage, because the downward impulse which, at the commencement of each stroke, it gives to the air below it, would make that air so much less efficient a resisting medium; whilst, by continually coming in contact with a fresh body of air, the wing is always acting on it to the best advantage. For this reason, when a bird commences its flight, it turns towards the wind if possible, to make up for its lack of motion on starting.

THE extension of the Cinchona cultivation in Darjeeling continues. Every year additional land is brought under Cinchona culture, and it is calculated that 2,000 acres more will be cleared and planted within the next four years. With regard to Ipecacuanha, upwards of 20,000 plants and cuttings are now in hand, all of which promise well. Another interesting fact relating to the introduction of useful plants into India, is that of the success in the Terai of the Cacao (*Theobroma cacao* L.). The plants that were planted out about a year ago, were sent from Kew at the suggestion of Dr. Hooker, and they are now in a most healthy and satisfactory condition.

THE French Academy has at last published the list of candidates for the seat rendered vacant by the death of the late Dr. Nelaton. The issue is quite uncertain. M. Broca, the celebrated anatomist, has obtained only a second rank, and M. Marey is placed in the third.

FATHER SECCHI is preparing, at Gauthier Villars, a second edition of his work on the Sun, on an enlarged scale. He has quoted so largely from Mr. Lockyer's "Solar Physics" that an intended translation of this work is abandoned for the present.

QUITE a sensation was produced in the last sitting of the Academie des Sciences, by the exhibition of photographs of Spitzbergen scenery, sent by Prof. Nordenskiöld. One of these represented a meteorite nearly 18 tons in weight.

A BELGIAN paper describes an immense petrified trunk of a conifer discovered in the province of Lineburg in perfect preservation. Its length is about 33 feet, and its diameter about 20 inches.

THE *Annuaire* of the Bureau des longitudes for 1874 has been recently published. Although sold at 1½ fr., it contains chromolithographs showing solar protuberances, and an essay by M. Faye on questions relating to the sun.

THE widow of General Poncelet, one of the most distinguished French military engineers, has written to the French Institute, announcing that the whole of the works of her late husband will soon be in the hands of the public. The last volume, the sixth of the series, containing the lectures delivered at the Metz School of Artillery and the École Polytechnique, will be issued shortly. It is edited by M. Kroetz, Chief Engineer of the National Manufactures, one of the General's pupils. General Poncelet was a member of the French Institute for more than twenty years. He died in 1864.

A MEMOIR upon the embryology of *Terebratulina*, by Prof. Morse, has just been published by the Boston (U.S.) Society of Natural History, this being the result of a thorough investigation in regard to the development of this genus of the brachiopod shells found so abundantly on the coast of Maine. Prof. Morse's labours were mainly prosecuted at Eastport, and extended through a period of several years. He found that the species spawns throughout the entire summer season (at least from April to August), but that investigations in the earlier part of the season were preferable, since, with the increasing warmth, the development is more rapid than is convenient to the observer.

THE "Fenland Meteorological Circular and Weather Report" is the name of a monthly periodical, two numbers of which, for February and March, have just reached us. It is edited by Mr. S. H. Miller, F.R.A.S. The circular is intended to be "a medium for local meteorologists, an abiding record of the climate of the Fenland, and a register of the changes of the weather, to which the agriculturist, horticulturist, and naturalist may easily refer." It is intended besides, we believe, that the "Circular" should fill to some extent an educational function, and induce agriculturists to take an interest in the sciences with which their art is so intimately connected. This purpose is to be served by the publication of readable articles on scientific subjects, the first of which is by Mr. S. B. J. Skertchly, F.G.S., on the "Practical Bearings of Meteorology." The two numbers sent us contain well-constructed tabular and graphic reports of the meteorology of Wisbech for January and February, besides district reports, and a number of notes and short articles on subjects more or less intimately connected with the department to which the "Circular" belongs; the paper and typography are all that can be desired, and the price is only 2*d.* per month. The enterprise is highly creditable to its originators, and we sincerely hope it will be a great success and attain a wide circulation in the important district for whose benefit it has been started; and we hope the example thus set will be followed by other districts in the kingdom. The publishers of the "Circular" are Leach and Son, Wisbech.

A BILL has been introduced into the U.S. Congress proposing an appropriation of 7,000 dols. to enable the Department of Agriculture to make a collection of all the species of trees growing throughout the United States, and for their exhibition in suitable cases. The collection itself, when completed, is to be exhibited at the Philadelphia Centennial Exposition, but to belong to the Agricultural Department, and to be returned to it.

AT the meeting of the French Academy of March 2, M. Charles Sainte-Claire Deville, gave an account of his meteorological mission to Biskra and Tuggurt. At each of these stations an observatory has been established similar to that at Montsouris. Captain Roget superintends the observatory of Biskra, and Dr. Audet that of Tuggurt; the latter, according to M. Deville, is in an almost perfect situation, surrounded by a sufficient quantity of vegetation, and preserved from the effects of radiation. The results comprehend already observations continued during the month of January, and M. Deville sees in them the promise of important discoveries.

WE have received a useful work lately published by the Smithsonian Institution, prepared by Prof. F. W. Clark, of Howard University, Washington. This is the first part of a series entitled the "Constants of Nature," and gives in tabular form the specific gravities, the boiling and melting points, and the chemical formulæ of a large number of substances, with indications of the authorities whence the facts are derived. The volume contains about 250 pages, and is well provided with the necessary indices.

LIEUTENANT G. M. WHEELER (United States Engineers) and party are in Washington, and are engaged in elaborating the results of their explorations in 1873. Already about 76,000 square miles of territory have been carefully gone over and topographed with a view to their representation in the new series of index maps now nearly ready for publication. Much valuable information in regard to the geology of the country, its mining facilities, and the probabilities of successful irrigation, has been obtained, and will be duly published. In the line of natural history the collections have been very large, embracing at least 1,200 skins of birds, besides many hundreds of reptiles, fish, insects, &c. The botanical collection is said to be the finest and largest ever procured by a Government expedition. The results of the work of the expedition for the years 1871, 1872, and 1873 are, we understand, shortly to be published, comprising seven large quarto volumes, which will prove a valuable addition to the scientific history of the great West.

WE are glad to see, from Part IV. of the Transactions of the Clifton College Scientific Society, that that Society is exceedingly prosperous, so far as number and attendance of members is concerned, though we very much regret to learn that, like not a few other similar Societies, it contains but a small number of real workers. We hope that the patrons and office-bearers of the Society will do their best to devise some means to remedy this very serious defect, for serious the defect is, seeing that one of the main objects of such a Society is to train its members to methodical work, careful observation, and independent thinking. The only paper in this part, by a student member of the Society, is a long and elaborate one on Potteries, by C. C. Stevenson.

A THICK supplementary number of Petermann's *Mittheilungen* has been published containing a vast number of statistics relating to the population of the globe, with two excellent illustrative maps.

AT the meeting of the Adelaide Philosophical Society held on Nov. 25, 1873, Dr. Schomburgk read the second part of a long paper on poisonous plants. The plants noticed were those of the genus *Strychnos nux vomica*, *Datura stramonium* (the thorn-apple), *Solanum nigrum*, and *Taxus baccata* (the yew tree).

WE learn, from a Reuter's telegram, that the bank, sixty miles in length, formed for a long time past in the White Nile south of the province of Secionda, and mentioned by Sir Samuel Baker as an obstacle to navigation, has been partially removed by the works ordered by the Soudan Government. The river is now navigable up to Kondokaro over a distance of ten and a half degrees. The works for the complete removal of the bank continue.

THE additions to the Zoological Society's Gardens during the past week include a Finsch's Amazon (*Chrysotis finschi*) from W. Mexico, presented by Mr. C. Chivers; a Virginian Deer (*Cervus virginianus*) from N. America, presented by Mr. N. M. Bateson; a Cornish Chough (*Fregilus graculus*), British, presented by Mr. J. T. Hewes; a Common Otter (*Lutra vulgaris*), British, presented by Dr. Stafford; a Blue and Yellow Macaw (*Ara ararauna*) from S. America, presented by Miss J. Staines.

THE GASEOUS, LIQUID, AND SOLID STATES OF WATER-SUBSTANCE *

IN two communications made by me to the British Association at its meetings at Edinburgh in 1871, and at Brighton in 1872, and printed as abstracts in the Transactions of the Sections for those years, considerations were adduced on relations between the gaseous, the liquid, and the solid states of matter. The new subject of the present paper constitutes a further development of some of those previous considerations, and a brief sketch of these is necessary here as an introduction for rendering intelligible what is to follow.

Taking into consideration any substance which we can have in the three states, gaseous, liquid, and solid, we may observe that when any two of these states are present in contact together, the pressure and temperature are dependent each on the other, so that when one is given the other is fixed. Then if we denote geometrically all possible points of temperature and pressure jointly by points spread continuously in a plane surface, each point in the plane being referred to two axes of rectangular co-ordinates, so that one of its ordinates shall represent the temperature and the other the pressure denoted by that point, we may notice that there will be three curves, one expressing the relation between temperature and pressure for gas with liquid, another expressing that for gas with solid, and another expressing that for liquid with solid. These three curves, it appears, must all meet or cross each other in one point of pressure and temperature jointly, which may be called the triple-point. The triple-point, considered in respect to its temperature, is in fact what would often be called the freezing point *in vacuo*; that is, the freezing temperature of water in contact with no gas except its own aqueous vapour or steam; and the pressure at the triple point is just the pressure of that aqueous gas or steam.

The curve between gas and liquid, which may be called the *boiling-line*, will be a separating boundary between the regions of the plane corresponding to the ordinary liquid and those corresponding to the ordinary gaseous state. But by consideration of Dr. Andrews's experimental results ("Phil. Trans.," 1869) we may see that this separating boundary comes to an end at a point of temperature and pressure which, in conformity with his language, may be called the *critical point* of pressure and temperature jointly; and we may see that, from any liquid state to any gaseous state, the transition may be gradually effected by an infinite variety of courses passing round the extreme end of the boiling-line.

The accompanying figure serves to illustrate these considerations in reference to transitions between the three states, the gaseous, the liquid, and the solid. The figure is intended only as a sketch to illustrate principles, and is not drawn according to measurements for any particular substance, though the main features of the curves shown in it are meant to relate in a general way to the substance of water, steam, and ice. A X and A Y are the axes of coordinates for the temperatures and pressures respectively; A, the origin, being taken as the zero for pressures and as the zero for temperatures on the Centigrade scale. The curve L represents the *boiling-line* terminating in the critical point E. The line T M represents the line between liquid and solid. It is drawn showing in an exaggerated degree the lowering of the freezing temperature of water by pressure; the exaggeration being necessary to allow small changes of temperature to be perceptible in the diagram. The line T N represents the line between the gaseous and the solid states of water-substance. The line L T N appears to have been generally (in the discussion of experimental results on the pressure of aqueous vapour above and below the freezing-point) regarded as one continuous curve; but it was part of my object in the two British-Association papers referred to, to show that it ought to be considered as two distinct curves (L T P and N T Q) crossing each other in the triple-point T.

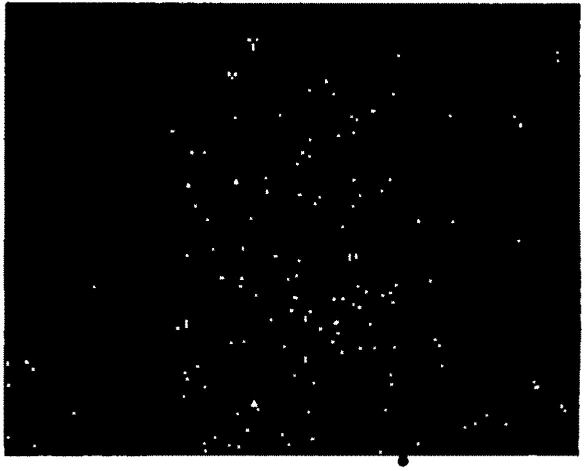
In the second of the two British-Association papers already referred to (the one read at the Brighton meeting, 1872), I gave demonstrations showing that these two curves L T and N T should meet, as shown in the accompanying figure, with a re-entrant angle at T, not with a salient angle such as is exemplified in the vertex of a pointed arch; and offered in conclusion the suggestion that the reasoning which had been adduced might be followed up by a quantitative calculation founded on experimental data, by which calculation the difference of the pressures of steam with water, and steam with ice for any given temperature very near the triple point, may be found with a very close approximation to the truth.

In the month of October 1872 I explained to my brother, Sir William Thomson, the nature of that contemplated quantitative calculation: I mentioned to him the method which I had prepared for carrying out the intended investigation, and inquired of him for some of the experimental data, or data already deduced by theory from experiments, which I was seeking to obtain. On his attention being thus turned to the matter, he noticed that the desired quantitative relation could be obtained very directly and easily from a simple formula which he had given in his paper on the Dynamical Theory of Heat, "Transactions of the Royal Society of Edinburgh," March 17, 1851, § 21 (3), to express the second law of thermodynamics for a body of uniform temperature throughout, exposed to pressure equal in all directions.

That formula is

$$\frac{dp}{dt} = \text{C M}$$

in which p denotes the amount of the pressure, and $\frac{dp}{dt}$ its rate of increase per unit increase of temperature, the volume being kept constant; C denotes Carnot's function; and M denotes the rate of absorption at which heat must be supplied to the substance per unit augmentation of volume, to let it expand without varying in temperature. The body may be either homogeneous throughout, as a continuous solid, or liquid, or gas; or it may be heterogeneous, as a mass of water and aqueous vapour (*i.e.* steam), or ice and water, or ice and aqueous vapour (*i.e.* steam).



Now apply that formula, 1st, to steam with water, and 2nd, to steam with ice, the temperature of the heterogeneous body in each case being that of the triple-point, or we may for the present purpose say 0° C., which is almost exactly the same. It is to be observed that while in the general application of the formula the rate of increase of the pressure with increase of temperature, *when the volume is kept constant*, has been denoted by $\frac{dp}{dt}$, yet in each of the two particular cases now brought under consideration it is a matter of indifference whether the volume be kept constant or not; because the pressure of steam in contact either with water or with ice, for any given temperature, is independent of the volume of the whole heterogeneous body; so that the change of pressure for change of temperature is independent of whether there be change of volume or not. As C is a function of the temperature which has the same value for all substances at the same temperature, it has the same value for the two cases now under consideration. Hence, retaining for the first case (that, namely, of steam with water) the same notation as before, but modifying it by the use of an accent where distinction is necessary in the second case (that of steam with ice), and thus using $\frac{dp}{dt}$ to denote the rate of increase of the pressure per unit increase of temperature for steam with wate at the triple-point (0° C. nearly), and M to denote the rate of absorption at which heat must be supplied to a body consisting of steam and water at the triple point, per unit augmentation of volume of that whole heterogeneous body, to let it expand without varying in temperature, and using $\frac{dp'}{dt}$ and M′ to denote the corresponding rates for steam with ice at the triple point, we have

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* "A Quantitative Investigation of certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance." By Prof. James Thomson, LL.D., lately of Queen's College, Belfast, now of the University of Glasgow. Communicated to the Royal Society by Sir William Thomson, LL.D., F.R.S. Abridged for NATURE by the Author.

$$\frac{\frac{dp}{dt}}{\frac{dp'}{dt}} = \frac{M}{M'}$$

The latent heat of evaporation of one pound of water at the freezing-point (or at the triple point) into steam at the same temperature, as determined by Regnault, is 606·5 thermic units, the thermic unit being here taken as the heat which would raise the temperature of 1 lb. of water 1° C., and the latent heat of fusion of ice is about 78 or 79 of the same thermic units. Hence, though M and M′ belong each to a cubic foot of steam at the triple point, not to a pound mass of it, still the ratio $\frac{M}{M'}$ is = $\frac{606}{79 + 606}$ Hence

$$\frac{\frac{dp}{dt}}{\frac{dp'}{dt}} = \frac{606}{79 + 606} = \frac{1}{1{\cdot}13}$$

This shows that for any small descent in temperature from the triple point (where the pressure of steam with ice is the same as that of steam with water), the pressure of steam with ice falls off 1·13 times as much as does the pressure of steam with water.

In submitting the quantitative calculation now given, I have preferred to adopt the method proposed and developed by my brother rather than that which I had myself previously devised, because his method is simpler, and brings out the results more briefly by established principles from existing experimental data. I may say, however, that the method devised by myself was also a true method, and that I have since worked it out to its numerical results, and have found that these are quite in accordance with those brought out by my brother. The two indeed may be regarded as being essentially of the same nature; and I think it unnecessary to occupy space by giving any details of the method I planned and have carried out. Its general character may be sufficiently gathered from the concluding passages of the British Association 1872 paper, as printed in the Transactions of the Sections, Brighton Meeting.

In order to discover whether the feature now developed by theoretical considerations is to be found showing itself in any degree in the experimental results of Regnault on the pressures of steam at different temperatures [*], I have made careful examinations of his engraved curve (Plate viii. of his Memoir), and of his empirical formulæ adapted to fit very closely to the results exhibited in that curve, and of his final tables of results at the close of his Memoir; and by every mode of scrutiny which I have brought to bear on the subject—in fact by each of some seven or eight varied modes—I have met with clear indication of the existence of the expected feature; and by some of them I have found that it can readily be brought prominently into notice. The engraved curve drawn on the copperplate by Regnault himself is offered by him as the definitive expression of his experiments, as being an expression which satisfies as well as possible the aggregate of his observations; subject, however, to a very slight alteration, which he has pointed out as a requisite amendment in the part of the curve immediately below the freezing-point, a part with which the investigations in the present paper are specially concerned.

After telling (p. 581 of his Memoir) of the great care with which he had marked the curve on the copperplate and got it engraved, he says:—"Je n'ai pas pu éviter cependant quelques petites irrégularités dans les courbes; mais une seule de ces irrégularités me paraît assez importante pour devoir être signalée. Elle se présente pour les basses températures comprises entre 0° et −16°, la courbe creuse trop vers l'axe des températures, elle laisse, notablement au-dessus d'elle, toutes les déterminations expérimentales qui ont été faites entre 0° et −10°. Ainsi les valeurs, que cette petite portion de la courbe donne pour les forces élastiques, sont un peu trop faibles, et j'ai eu soin de les augmenter, de la quantité convenable, dans les nombres que je donnerai plus loin." Whether we are now to think that this bend downwards[*] of the curve towards the axis of temperatures involving what Regnault regarded as a small faulty departure of his drawn curve from his actual experiments, was introduced merely by a casual want of accuracy in drawing, or whether we may suppose that possibly there may have been some experimental observations which attracted the curve downwards, but were afterwards rejected on a supposition of their being untrustworthy, it appears that such a bend is a feature which the curve really ought to possess, and is one which, even after being partially smoothed off by way of correction, is not obliterated, but still remains clearly discoverable in the final numerical tables of results.

This is best brought to light by means of the empirical formulæ devised and employed by Regnault for the collating of his results. He proceeded evidently under the idea of the curve being continuous in its nature, so that a single formula might represent the pressures of aqueous vapour throughout the whole of his experiments; but before seeking for such a formula he proceeded to calculate several local formulæ of which each should represent very exactly his experiments between limits of temperature not wide apart; and afterwards he worked out several general formulæ, each adapted singly for the whole range of his experiments.

Now in the paper communicated to the Royal Society, and printed in the Proceedings for December 11, 1873, from which the present paper is an abridgment, the details of a scrutiny of the chief of these formulæ are given (the formulæ, especially, which were adopted by Regnault for deducing his final general table extending from − 32° C. to + 230° C.), from which it appears that they present clear indications that at and very close to the freezing-point (or rather the triple-point) the rate of increase of pressure for increase of temperature is decidedly less in the case of steam with water than in that of steam with ice; or, in other words, that at and very close to the triple-point the steepness of the curve for steam with water is decidedly less than that of the curve for steam with ice; or, to state the same a little more fully, that while the steepness is increasing as we ascend from temperatures below the triple-point up to the triple-point, with ice in contact with steam, there is a sudden abatement of the steepness in passing the triple-point, where the change occurs from steam with ice to steam with water, after which, with continued rise of temperature, the steepness goes on again increasing. In fact the result comes out that these formulæ, expressing an aggregate of experimental results of Regnault, would indicate for $\frac{\frac{dp}{dt}}{\frac{dp'}{dt}}$ at the freezing-point, or the triple-point, not the value 1 (as would be the case if a curve continuous past the triple-point would express the pressure of steam or aqueous vapour, for different temperatures, in contact with ice below the triple-point and with water above it), but 1·09 or 1·10, which makes a near approach to the result 1·13 found by my brother's quantitative calculation already here cited. The decimal fractions in excess of unity, here represent the quantitative relation between the greatness of the feature under consideration as brought out by the theoretical investigation on the one hand, and as deduced from Regnault's results on the other hand: and thus we may say that this feature can be brought to view as existing in Regnault's principal results in a degree about $\frac{9}{13}$ or $\frac{10}{13}$ of that in which the theoretical investigation shows that it must really exist, and ought to be found experimentally, if the experiments had a sufficiently minute exactitude to detect it and to measure it.

Regnault also gives, in the same Memoir, another statement of results deduced from his experiments, and put in the form of a table intended chiefly for meteorological purposes; which table shows the pressures of aqueous vapour for temperatures ranging below and above the freezing-point at very small intervals of temperature, $\frac{1}{10}$th of a degree centigrade each. In this table, the numbers inserted as representing the pressures below the freezing-point are slightly different from the corresponding ones in his general table, which, with the formulæ used in making it, has just now been referred to; and he mentions that this slight discrepance has resulted from the fact that the two tables were formed at different periods, and were not calculated by the same formula; but he remarks that the differences are insignificant, as they scarcely amount to ·02 millimetre of mercury in the pressures which the two tables respectively show. Here, too, as in the general table, the feature expected shows itself, though in a diminished degree. By careful examination of the minute changes of pressure for small changes of temperature, close to the freezing-point, both above and below it, as they are shown in this table, I find that the experimental results as here offered would indicate the existence of the feature in a degree about $\frac{1}{15}$ or $\frac{1}{15}$ of that in which the theoretical investigation now shows that it ought to be met with, if experiments could be made on pressures of aqueous vapour, above and below the freezing-point, with sufficiently minute exactitude.

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[*] Regnault, "Des forces élastiques de la vapeur d'eau aux différentes températures," Mémoires de l'Académie des Sciences, 1847.

[*] In M. Regnault's curve the temperatures are measured horizontally across the sheet and pressures are measured upwards.

It is indeed a great credit to the accuracy of Regnault's experiments, and to the exactitude of his results, that the results contain the clear indications they do of this feature, which only comes to view through comparison of differences of pressure represented by very minute fractions of a millimetre of mercury; and which, unless a very high order of accuracy were maintained, might have given no perceptible indication of its existence, or might readily have been made to disappear totally from the final results, through the application of the ordinary methods for clearing off small errors of observation.

SCIENTIFIC SERIALS

American Journal of Science and Arts, February 1874.—This number commences with a paper by Mr. Langley (accompanied with plate), describing studies on the minute structure of the solar photosphere, made at the Alleghany Observatory. The equatorial used had an aperture of 13 inches. The author finds that the ultimate visible constituents of the photosphere are not the "rice grains," but small *granules* composing them, and not more than $0''\cdot3$ in size. Comparing the total area covered by them with that of the whole sun, he estimates that the greater part of the solar light comes from an area of not over one-fifth of its visible surface, and which may be indefinitely less. Hence the received estimates of the intensity of the action to which solar light is due must, he thinks, be greatly increased. In the penumbra there are not only numerous small cyclones, and even right- and left-handed whirls in the same spot, but probably currents ascending nearly vertically. The action of superposed approximately horizontal currents is a prominent feature. The outer penumbral edge seems to be formed by rupture. Mr. Langley accepts M. Faye's theory as the most probable.—Prof. Pickering communicates some measurements of the polarisation of light reflected by the sky, and by one or more plates of glass. One remarkable result arrived at was, that the polarisation (from the sky) is the same, for a given solar distance, for any meridian distance; in other words, that the polarisation is the same for all points equally distant from the sun.—In a translated paper on the dissipation of electricity in gases, by a Russian physicist, M. Bobouleff, the author concludes from his experiments, that the dissipation in air (and other gases) diminishes with diminution of the pressure; and that the dissipation in hydrogen is less than in air (at the same pressure).—Mr. Verrill continues his notes on results of recent dredging expeditions on the coast of New England.—In the "Scientific Intelligence" we find a summary of a recent important memoir by Prof. Morse on the systematic position of the Brachiopoda. His avowed object is to show that in every point of their structure the Brachiopoda are true worms, with possibly some affinities to the Crustacea, and that they have no relations to the Mollusca, save what many other worms may possess in common with them.—The organisation of an American Metrological Society is announced; the design being to originate and promote measures for improving the system of weights, measures, and moneys, and bring these into relations of simple commensurability with each other.

IN the number of the *Bulletin Mensuel de la Société d'Acclimatation de Paris* issued in January one of the principal papers is an article by M. Mérice on Agriculture in Brazil. This country is by nature fitted to be one of the most prolific agricultural tracts of earth in the world, so varied and abundant are its productions, and so fruitful its soil. Improved implements are necessary, and increased skill would give greatly increased results. Great strides are being taken in the "education" of various kinds of silkworms, and under the directions given in the *Bulletin* the different varieties may be properly reared on the most approved principles.—M. Carbonnier, to whom the honour is due of introducing the "Paradise fish" from China, has been successful in importing some live specimens of another species of *Macropodus*, the "rainbow-fish" of India: specimens of the *Pescare*, or "king-fish" of South America have also been brought to Paris from Buenos Ayres. M. E. Perris' paper on birds and insects is continued.—The recent transmission of salmon ova to New Zealand, the cultivation of lobsters in the United States, the introduction of the *Eucalyptus* in various parts of Europe, are all, amid a mass of other matter, referred to at length. The number for January, just received, gives an interesting account of the year's work at the *Jardin d'Acclimatation*. In the Bois de Boulogne experiments have been made in the cultivation of various vegetables and other useful plants, on the results of which the future utilisation of new importations greatly depends.—The notes on the cultivation of the vine and on the use of mineral manures will be found very valuable.—The question of fish culture, which has assumed such large proportions in England and America, is being taken up by the Society, and an interesting paper on the subject is contributed by M. de la Blanchere.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti: Dec. 18, 173. In this number Prof. Lombroso furnishes exact measurements of the crania of 66 Italian criminals (from various museum collections) along with an interesting analysis of those data. With reference to capacity as measured by apparent circumference, while there were a few of pretty large circumference (1 of 580, 1 of 550, 2 of 560, 2 of 540, out of 65), and a moderate number of ordinary size (8 of 530, 13 of 520), a large proportion were quasi-microcephalic (39 out of 65); and precisely 19 of 510, 8 of 490, and 12 of 500. Of 40 crania examined the mean capacity, in cubic centimetres, was 1,389; two were of more than ordinary capacity (1,610 and 1,633), 3 were of ordinary (1,500 and more), 12 had a capacity of 1,400 and more, 19 were of much inferior capacity (1,300 and more), while 4 were truly microcephalic, with a capacity of 1,100 to 1,200. Of the last, 2 were crania of assassins, 1 that of a thief, and 1 that of an incendiary (with intent to rob). Prof. Cantoni gives the concluding portion of a valuable paper in experimental physics, on the polarisation of non-conductors. He here takes up several objections urged by Prof. Righi to his opinion as to the possibility of polarising a non-conductor durably.—M. Corradi continues an historical sketch of the study and teaching of anatomy in Italy in the Middle Ages.—We also note short papers on primitive tumours of the dura mater (by M. Buzzorero), and on a remarkable appearance of the zodiacal light, and a shower of falling stars observed in some parts of Italy on December 12 last.

Bulletin de l'Academie Royale des Sciences (de Belgique). No. 12. In this number are given a series of papers or lectures of a somewhat popular nature, read at the public seance in December. The first is by M. Gluge, who advocates the teaching of biology in the Belgian schools.—M. d'Omalius d'Halloy follows with an argument for the hypothesis of transformation by generations of forms from a first creation, as against that of successive creations on the one hand, and that of evolution from matter on the other.—The next lecture, by M. van Beneden, is entitled "A Word on the Social Life of the Inferior Animals," and gives some curious facts in natural history, relating especially to parasites.—And, lastly, we have an able lecture by M. Schwann, on the commencement and the end of the world, according to the mechanical theory of heat. The author gives a lucid exposition of the two fundamental principles of conservation and dissipation of energy, explaining, with special fulness, the doctrine enunciated by Clausius and the considerations leading to it.—The *Bulletin* further contains reports by members of the Academy on various prize-competitions. One subject proposed was the relation of heat to development of phanerogamic plants, with special reference to the periodic phenomena of vegetation. The Committee give lengthy analyses of the memoir received, to which, while of some scientific merit, the prize is not awarded. In a lecture by M. Morren on the subject, the law is elucidated, that, other things being equal, the quantity of carbon fixed by a vegetation is greater in proportion as its height is less, inasmuch as this supposes a less expenditure of movement.—Another question called for a description of the coal system of the Liége Valley. From a memoir on which the committee report favourably, it appears that instead of having twenty-five beds of coal, as had been thought since the labours of Dumont, the district referred to contains scarcely half that number.—Several mathematical notes are given, and there is a description of three additions to the synopsis of Calopterygines, by M. Longchamps.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 12.—"Contributions to the Developmental History of the Mollusca, Sections I., II., III., IV." By E. Ray Lankester, M.A., Fellow of Exeter College, Oxford. Communicated by G. Rolleston, F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford.

The points of greatest interest to which the author draws attention in the present memoir are:—

1. The explanation of the basket-work structure of the surface of the ovarian egg, by the plication of the inner egg-capsule.
2. The increase of the yelk by the inception of cells proliferated from the inner egg-capsule.
3. The homogeneous condition of the egg at fertilisation.
4. The limitation of yelk-cleavage to the cleavage-patch.
5. The occurrence of independently-formed corpuscles (the autoplasts) which take part in the formation of the blastoderm.
6. The primitive eye-chamber formed by the rising up of an oval wall, and its growing together so as to form a roof to the chamber.
7. The origin of the otocysts by invagination.
8. The rythmic contractility of a part of the wall of the yelk-sac.
9. The disappearance of the primitive mouth, and the development of a secondary mouth.
10. The development of a pair of large nerve-ganglia by invagination of the epiblast immediately below the primitive eye-chambers.

"The Localisation of Function in the Brain," by David Ferrier, M.A., M.R.C.P., Professor of Forensic Medicine, King's College, London. Communicated by J. Burdon Sanderson, F.R.S., Professor of Practical Physiology in University College.

The chief contents of this paper are the results of an experimental investigation tending to prove that there is a localisation of function in special regions of the cerebral hemispheres.

Anthropological Institute, March 10.—Prof. George Busk, F.R.S., president, in the chair. A paper, by Dr. A. P. Reid, was read, "On the mixed or half-breed Races of North-Western Canada." The mixed races were nine in number, viz. the progeny of (1) the Anglo-Saxon father and Indian mother, (2) the French and French-Canadian father and Indian mother, (3) the Anglo-Saxon father and mixed Anglo-Saxon and Indian mother, (4) the French father and mixed French and Indian mother, (5) the "half-breed" Anglo-Saxon and Indian as father and mother, (6) the "half-breed" French and Indian as father and mother, (7) the descendants proceeding from intermarriage of 5th class, (8) the descendants proceeding from intermarriage of 6th class, (9) the mixed or "half-breed" father and Indian mother. Those nine divisions included the principal mass of the mixed peoples of Manitoba. The French and Anglo-Saxons and their descendants rarely intermarried. The author pointed out the marked change in physique, which was common to all the classes he had enumerated, that quickly followed the removal of Europeans to American soil. The complexion becomes swarthier and more nearly resembling the type of native Americans than one would suppose. That change was due to climatic influences, to different food, and to altered customs. On the whole, there was a tendency, in all the mixed races, to the Indian rather than to the European type. They could not be said to possess any objectionable peculiarities; they were not more inclined to the abuse of alcohol or to other irregularities than the pure whites; and it would be difficult to find a people who have fewer faults. Some of the families of the pure white and pure Indian were often very numerous, sometimes reaching the number of fifteen; but four to six was the average.—A paper, by the Rev. George Taplin was read, "On the mixed races of Australia and their migrations." The author's deductions were made chiefly from linguistic data. He however recorded the fact of having met with some individuals of the Narrinyeri tribe who had light complexions and straight hair. He found also that among the Narrinyeri there were superstitions and customs identical, even in name, with those obtaining among the Samoans.—Commander Telfer, R.N., communicated notes on the discovery of burial grounds near Tiflis in Georgia. In one of the graves were found parts of a body that had undoubtedly been interred in a sitting posture. The skull of an adult was remarkably distorted, and bore a striking resemblance to the longest form of the Titicacan skulls of South America.—A paper by Miss A. W. Buckland "On the Serpent, in connection with primitive metallurgy," was also read.

Royal Horticultural Society, March 4.—General Meeting.—Lieut.-Gen. Hon. Sir A. H. Gordon, K.C.B., in the chair.—The Rev. M. J. Berkeley commented on some of the plants shown. They included *Encholirion corallinum*, a curious Bromeliaceous plant from Brazil, a fine species of Medinilla probably new, and the beautiful *Iris reticulata*, which, though a native of Persia, proved quite hardy in this country.

Scientific Committee.—Dr. Hooker, C.B., P.R.S., in the chair.—The Rev. M. J. Berkeley called attention to the following communication made by Prof. Panceri to the Institut Egyptien at its meeting on December 13, on Cryptogamic vegetation found within the egg of an ostrich, which was interesting in connection with what he had himself brought before the committee on March 5 and 19, 1873. The egg had been given Prof. Panceri at Cairo, and was still fresh, the air space having not even been formed. He soon, however, noticed the appearance of dark blotches within the shell, and having broken it open to ascertain the cause, he found that they were produced by the growth of minute fungi. Instances of a similar kind had already been studied by him, and he had communicated the results to the Botanical Congress held at Lugano in 1859. The Rev. M. J. Berkeley had found *Cladosporium herbarum* in the interior of a fowl's egg.—Dr. Masters brought shoots of *Picea nobilis*, in which the primary shoot was dead and swollen beneath the apex. In many instances he had found similar excrescences to contain the larva of an insect. In other cases the primary cause of injury appeared to be frost or cutting east winds.—Dr. Masters exhibited some peas which had been attacked by a beetle (*Bruchus pisi*) which fed on the cotyledonary portion, but left the plumule, so that the seeds still germinated.—Dr. Masters reported on the monstrous Cyclamen which had been referred to him at the last meeting. The apparent corolline whorl in the Primulaceæ is now regarded as an outgrowth from the androecium. In the present case there appeared to be a supplementary staminal whorl alternating with the normal one, and therefore with its members opposite to the sepals. These members, however, had become partially petaloid, and were rolled up, so that the whole flower had a superficial resemblance to a case of lateral prolification.—Mr. Grote stated that Mr. F. Moore, of the Indian Museum, agrees with Prof. Westwood, and refers the Assam Tea-bug to the genus *Helopeltis* of the Capsidæ. A Ceylon species of this bug is figured by Signoret in the *Ann. Soc. Entom. de France*, 3rd series, pl. 12, fig. 2. Two other species are known from the Indian Archipelago. The Indian species described by Mr. Peal, differs from the Ceylon species in its habit of feeding on the juice of the tea-plant; and Mr. Moore proposes to call it *H. theivora*.—Prof. Thiselton Dyer read the following extract from a letter from Mr. James Caldwell, Port Louis, Mauritius:—"I would especially call your attention to a case in which the ribbon cane has sported into a green cane and a red cane from the same head. I saw at least 200 instances of it in the same plantation, and the fact has completely upset all our preconceived ideas of the difference of colour being permanent. The conversion of a striped cane into a green cane was not uncommon, but the change into a red cane universally disbelieved, and that both events should occur in the same plant incredible. I find, however, in Fleischman's 'Report on Sugar Cultivation in Louisiana for 1848,' published by the American Patent Office, the circumstance mentioned, but he says he never saw it himself."—In the report of the meeting for Feb. 11 (NATURE, vol. ix. p. 354) the word "gabey" in line 12 should be omitted.

Mathematical Society, March 12.—Dr. Hirst, F.R.S., president, in the chair.—The following papers were read:—On certain constructions for bicircular quantics, and On a geometrical interpretation of the equations obtained by equating to zero the resultant and the discriminant of two binary quantics, by Prof. Cayley.—On the Cartesian equation of the circle which cuts three given circles at given angles, by J. Griffiths.—On another system of poristic equations, by Prof. Wolstenholme.

EDINBURGH

Royal Physical Society, Feb. 25.—Dr. John Alex. Smith, president, in the chair.—On a New Mode of Estie Amount of Colour in Water, by Mr. J. Falconer Mr. King's process consists in adding to chemically pure standard coloured solution contained in an accurately ated instrument until the pure water equals in colour the men of water under examination. The result of an estiion of colour in a water can thus be accurately recorded and preserved for future reference.—Occurrence of the Deal Fish (*Trachypterus arcticus*), near Montrose, by James C. Howden, M.D., Sunnyside.—Notes of the American Bittern (*Botaurus lentiginosus*), and some other of our rarer birds recently shot in the South of Scotland, by John Alex. Smith, M.D.—Note on a New Fossil from the Silurian Rocks in the Pentland Hills, by Mr. D. J. Brown. Mr. Brown described a section through the rocks in which the fossil occurred. The fossil he believed to be a seaweed, of which it appeared to be a frond. This is the third specimen of seaweed that has been found in these rocks.—Note on Bryozoa, from the carboniferous limestone at Longniddry Station, by Mr. D. J. Brown. Mr. Brown exhibited a fine series of Bryozoa from the lower carboniferous limestone group of Longniddry quarry, Haddingtonshire. This series of Bryozoa consisted principally of fragments of the genera *Fenestrella* and *Polypora*, the whole facies being eminently that of the carboniferous limestone, although amongst them was one fragment apparently referable to the Permian genus *Thamniscus*.—On some Peculiarities in the Geographical Distribution of the Mammalia of Greenland, as explanatory of the origin of the flora and fauna of that country, by Dr. Robert Brown, secretary. Dr. Brown considered that a great portion of the Greenland fauna and the bulk of its flora had been derived from Europe when Greenland was united, probably during or shortly after the time the Miocene beds were laid down to the European continent, by continuous land or a chain of islands, of which it is possible that Iceland, Bear Island, and perhaps even the Orkneys and Shetlands, are only fragments.

MANCHESTER

Literary and Philosophical Society, Feb. 24.—Rev. William Gaskell, vice-president, in the chair.—On the Effect of Acid on the Interior of Iron Wire, by Prof. Osborne Reynolds. It will be remembered that at a previous meeting of this Society Mr. Johnson exhibited some iron and steel wire in which he had observed some very singular effects produced by the action of sulphuric acid. In the first place the nature of the wire was changed in a marked manner, for although it was soft charcoal wire it had become short and brittle; the weight of the wire was increased; and what was the most remarkable effect of all was that when the wire was broken and the face of the fracture wetted with the mouth it frothed up as if the water had acted as a powerful acid. These effects, however, all passed off if the wire were allowed to remain exposed to the air for some days, and if it were warmed before the fire they passed off in a few hours. Prof. Reynolds subjected one of the pieces of wire to a farther examination, and from the result of that examination was led to what appears to be a complete explanation of the phenomena. He was led, from certain observations, to conclude that the effect was due to hydrogen, and not to acid, as Mr. Johnson appeared to think, having entered into combination with the iron during its immersion in the acid, which hydrogen gradually passed off when the iron was exposed. This conclusion he tested and proved to be correct by further experiments. The question appears to the author one of very considerable importance, both philosophically and in connection with the use of iron in the construction of ships and boilers. If, as is probable, the saturation of iron with hydrogen takes place whenever oxidation goes on in water, then the iron of boilers and ships may at times be changed in character and rendered brittle in the same manner as Mr. Johnson's wire, and this, whether it can be prevented or not, is at least an important point to know, and would repay a further investigation of the subject.—Dr. Ransome demonstrated the movements of the chest in respiration, showing the remarkable mobility of its several parts, and the consequent facility with which its cavity can be inflated.

BALTIMORE

Maryland Academy of Sciences, Feb. 2.—A paper was read from Prof. D. S. Martin upon the economical resources of Cornwall, embracing one of the most remarkble deposits of iron ore in America, and forming three large banks. Reference was made in the paper to the immense development of the magnetic oxide of iron in these mines, and to the fact that some of the ore exhibits marked magnetic polarity (native lode-stone). The yield is 175,000 tons annually, but the capacity is double. The minerals of unusual interest found here, with the magnetite, are lodes and coatings of cuprite, chrysocolla, azurite, malachite, and brochantite, the last named being found in no other locality in Eastern North America.

PARIS

Academy of Sciences, March 9.—M. Bertrand in the chair.—M. H. Resal communicated a paper on the theory of the "ground swell" (*houle*).—On a new spiral regulating chronometers and watches, by M. Phillips. The results of experiments made to test the isochronism of chronometers provided with the new spiral.—Researches on crystalline dissociation; estimation and division of the work done in saline solutions, by MM. P. A. Favre and C. A. Valson. A continuation of former communications on this subject.—On a particular arrangement of micrometer with movable wires proposed for the telescopes to be used for the observation of the Transit of Venus, by M. Ph. Hatt. Communicated by M. Fizeau.—New note on waves of variable height and velocity, by M. L. E. Bertin.—On the dispersion of gases, by M. Mascart. The author has determined the dispersion of air, nitrogen, hydrogen, N_2O, CO, CO_2, and CN. The dispersion of these gases bears no direct relation to their refractive power nor to their density.—On the wave-lengths and characters of the violet and ultra-violet rays of the solar spectrum, given by a photograph taken by means of a diffraction grating, by M. H. Draper. An abstract of this paper and the accompanying Alberttype print have already appeared in this journal.—Note on hydrogenised palladium, by MM. L. Troost and P. Hautefeuille. By studying the tension of the gas in the metal at various temperatures the authors come to the conclusion that hydrogenised palladium is a definite compound of hydrogen and palladium with an excess of hydrogen dissolved in it. The tension of the hydrogen is constant from the time the amount of contained gas is equal to 600 volumes; this amount corresponding to the formula Pd_2H. The authors announce a future communication on hydrides of potassium and sodium (K_2H and Na_2H). No allusion is made to the results recently obtained by Wright and Roberts by the determination of the specific heat of hydrogenised palladium.—New apparatus for determining the tannin contained in the different astringent materials employed in tanning, by M. A. Terreil. The process used depends upon the absorption of oxygen by tannin in presence of alkaline liquids.—Organogenesis compared with androgenesis in its relations with natural affinities (Classes *Selaginaceae* and *Verbenaceae*), by M. A. Chatin.—Reply to a reclammation of priority of M. Béchamp, by M. P. Schutzenberger.—Probable character of the first fortnight of March, by M. de Tastes. A weather prognostication.—Researches on the origin of the lithological elements of the tertiary and quaternary soils of the neighbourhood of Oran, by M. Daubrée. The author concludes from his researches that the Middle Tertiary epoch was especially an age of eruptions of a trachytic nature. M. A. Nordenskiold presented to the Academy some photographs taken in Spitzbergen in 1872–73. Among the photographs was one of the largest known mass of meteoric iron. It was discovered in Greenland (Ovifak) in 1870. This mass weighs 21,000 kilogrs. and is about to be brought to Stockholm for the Royal Museum.

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THURSDAY, MARCH 26, 1874

THE SCIENCE COMMISSION'S MUSEUM REPORT

THE Royal Commission on Scientific Instruction and the Advancement of Science have just issued their fourth Report, which is mainly concerned with the principal public Scientific Museums and Collections of the Metropolis, touching also briefly on the Scientific Museums and Botanic Gardens of Edinburgh and Dublin, and at some length upon provincial Local Museums generally, and upon the means by which these last might be made widely beneficial for scientific instruction. The Report also deals with the subject of Public Lectures in connection with Museums.

The Metropolitan Museums dealt with in the Report are the British Museum, the Museum of the Royal College of Surgeons, the National Botanical Collections and Gardens, the Museum of Practical Geology, and the South Kensington Museum, with its branch at Bethnal Green. The Report of the Commission is founded upon a thorough investigation into the growth and present condition of these institutions, and the opinions of a large number of men competent to speak on the subject as to the best means of systematising the various institutions, and of enabling them to discharge efficiently the objects for which they exist. The following are the principal recommendations of the Commissioners, which we prefer to give in the words of the Report.

With regard to the Natural History Collections of the British Museum, it is recommended :—

> "That a Director be appointed by the Crown, and should have the entire administration of the establishment, under the control of a Minister of State, to whom he should be immediately responsible. That the appointments of keepers and other scientific officers should be made by the Minister, after communication with the Director and with the Board of Visitors.
>
> "That a Board of Visitors be constituted, to be nominated in part by the Crown, in part by the Royal and certain other Scientific Societies of the metropolis, and, in the first instance, in part also by the Board of Trustees; the members to be appointed for a limited period, but to be re-eligible; and that the Board of Visitors should make annual reports to the Minister, to be laid before Parliament, on the condition, management, and requirements of the Museum, and should be empowered to give him advice on any points affecting its administration."

With regard to the National Botanical Collections and Gardens, the Commission recommend :—

> "That the collections at the British Museum be maintained and arranged with special reference to the geographical distribution of plants and to palæontology; and that the collections at Kew be maintained and arranged with special reference to systematic botany.
>
> "That all collections of recent plants made by Government expeditions be, in the first instance, sent to Kew, to be there worked out and distributed, a set being reserved for the British Museum; and that all collections of fossil plants made by Government expeditions be sent to the British Museum.
>
> "That opportunities for the pursuit of investigations in physiological botany should be afforded in the Royal Gardens at Kew."

With regard to the Museum of the Royal College of Surgeons, it is recommended :—

> "That, should the fund at the disposal of the College prove inadequate for the efficient maintenance and continued extension of the Museum, it should receive support from the State, as an institution intimately connected with the progress of biological science in this country."

With regard to the Scientific Collections of the South Kensington Museum, the Commissioners recommend :—

> "The formation of a collection of physical and mechanical instruments; and they submit for consideration whether it may not be expedient that this collection, the collection of the Patent Museum, and that of the scientific and educational department of the South Kensington Museum, should be united and placed under the authority of a Minister of State."

With regard to Provincial Museums, the Commissioners recommend :—

> "That, in connection with the Science and Art Section of the Education Department, qualified naturalists be appointed to direct the collection of specimens in order to supply whatever deficiencies exist in the more important provincial museums; and also in order to organise typical Museums, to be sent by the Department of Science and Art into the provinces to such Science Schools as may be reported to be likely to make them efficient instruments of scientific instruction.
>
> "That a system of inspection of provincial museums be organised with a view of reporting on their condition, and on the extent to which they are usefully employed, and whether the conditions of the loan or grant from the Department of Science and Art have been fulfilled."

The final recommendations are on the subject of Lectures, and are :—

> "That courses of lectures be given in connection with the collection of physical and mechanical instruments, the object of these lectures being to illustrate the progress of scientific and mechanical discovery and invention.
>
> "That the establishment of lectures on Science, accessible to all classes on the payment of a small fee, should be promoted by the Government in the great centres of population.
>
> "That, in the first instance with the view of carrying out the preceding recommendation, the system of instruction of this kind, which has already been established by the Government in the metropolis, should be developed by the institution of courses of lectures on the principal branches of Experimental and Natural Science.
>
> "That the proposed lectures be of two kinds (1) lectures of an elementary character on the general principles and most important facts of Science; (2) lectures specially intended for the working classes on the application of Science to the arts and industries of the country."

Until this Report was issued no general survey had ever been taken of our Museum system, if that can be called a system the growth of which has been almost entirely the result of accident. Both in the metropolis and in the provinces there exists a large number of museums and of collections of various kinds—to a large extent, however, connected with Natural History, and in local museums with Antiquities—but in almost every case, when the history of any of these institutions is traced, it will be found that it had its origin in quite an accidental way, and that no well-defined and intelligent system has been followed in the establishment of those institutions meant for public instruction. Some of the consequences of this capricious birth and untrained growth of the institutions referred to are that, especially in the metropolis, we have a heterogeneous collection of museums that have no relation whatever to each other, each pursuing the even tenor of its way without any regard to its neighbours; the collections in these museums often overlap each other, thus wasting means that might be expended to better purpose in developing some well-organised common system of aid to scientific research and instruction, and consequently some departments of Science are represented and endowed almost to excess, while others of at least equal importance are not represented at all; and although all of them have ostensibly the same objects in view, viz. to afford facilities for scientific research and for the scientific instruction of the public, some are directly under the control of a Minister of State, others are not.

If the recommendation of the Commission, that the government of the British and Patent Museums be transferred to a department of State with a responsible Minister at its head, is adopted by the Government, no doubt some of these anomalies will be abolished; the institutions will be made to fit into each other, and their government will be reorganised on some common and intelligent system, such as that recommended by the Commission.

One of the most glaring of these anomalies is the almost exclusive representation and endowment in our public museums of the Natural Sciences; Botany, *e.g.*, being twice endowed, in the British Museum and at Kew—while the Physical Sciences, as if they were the illegitimate offspring of man and nature, are left to pick up a living as best they may, so that had it not been for their inherent vitality they would long ago have been starved out of existence.

In our Museums and Gardens, and elsewhere, aid to research in connection with the Biological Sciences is well provided for, while students of Botany, of Zoology, and of Geology in its various departments, have abundant facilities afforded them for the practical study of these sciences. The result is that there is nothing to check the career of these sciences; they have been rapidly extending their domain, and may go on extending it still further without much anxiety as to where the sinews of war are to come from; all this with the very best results to our country. Our readers need no reminding of the immense strides recently made by Physical Science in its various departments, departments increasing in number and complication, and of the vistas of possible discovery of the most stupendous character which have been opened up, but for which private enterprise is utterly inadequate, and which must remain shrouded in mystery unless assistance similar to that which has been so amply accorded to the favourite Natural History Sciences be also given to the hitherto neglected Physical Sciences. Physical Science, though she sees many a glorious world that she longs to conquer, and whose conquest would be fruitful of the best results, can only in sadness let her hands dangle idly by her side, because unaided she cannot reach these fields of battle. No one competent to pronounce an opinion would venture to say that Physical Science has done less for the material prosperity of this country than Natural Science; indeed within the last few years our rapid advance has been almost entirely owing to the practical application of physical discoveries. Yet what encouragement is held out to those who are able and willing to devote themselves to research which brings no profit to the researcher, but which is fraught with ultimate benefit to the race?

The public, as we have shown, is made familiar in our museums with the results which have been reached in the Natural History Sciences, as well as with their *matériel*, but looks in vain for any exhibition of the instruments, the methods, and equally valuable results which belong to Physical Science; hence, no doubt, partly the reason why the latter has been hitherto almost entirely left out in the cold; it is not known, and has but little opportunity of letting the public know its history and achievements, though it has something to show of at least equal value with the umbrella or the boomerang of a conquered savage. In chemistry, heat, light, sound, electricity, astronomy in its various branches—if a student wishes to have something more than a mere book knowledge of the methods of work and of the results obtained (and there are many such students), where can he obtain it in the same way as students of Zoology, Botany, Geology, Comparative Anatomy, and Physiology can carry on the practical study of these sciences? And yet no one, we dare say, would venture to give any better reason for this state of things than that Chance, which has hitherto governed the growth of our museums, has ordered it so.

There can be now, however, no possible excuse for the continuance of this anomalous system, seeing that the Report of the Commission has thoroughly exposed it, and suggested methods whereby to some extent its glaring defects and anomalies may be remedied. If Government wish to find a model for their guidance in reorganising and supplying the deficiencies of our public museums and institutions intended for the researches of students and for popular instruction, let them turn to Appendix III. of the Report of the Commission, containing the Report of the Secretary on the Aid given by the State to Science in France. In a previous article (vol. ix. p. 217) we referred to the disgraceful condition of our Patent Museum, and contrasted it with the magnificent Conservatoire des Arts et Métiers of Paris, extracts from the long and complete catalogue of which, as well as syllabus of its well-organised courses of lectures, will be found in the Appendix referred to.

> "Evil is wrought by want of thought,
> As well as by want of heart."

Government can no longer plead the excuse implied in Hood's lines for neglecting to remedy the evils so forcibly brought under their notice by the Report of the Commission. If means are not forthwith taken to organise our public museums and institutions for scientific research and instruction on some intelligent system, to supplement their lamentable deficiencies, and make them as widely beneficial to the advancement of Science in all its departments and conducive to the highest instruction of the public as they are calculated to be, it can no longer be set down to ignorance, but to an utter disregard to the highest welfare of the country. In this direction the new Government have a chance of distinguishing themselves and winning for themselves an enduring and worthy popularity; let them lose no time in showing their wisdom by appointing a responsible Minister of Education whose duty it will be to keep all our public scientific and educational institutions up to the highest pitch of efficiency, to re-organise them upon some common basis, and to see that the progress of research in all branches of Science is not hampered by the want of adequate means for its pursuit. By such means will our rulers show themselves to be the real well-wishers and benefactors of their country.

TODHUNTER'S "MATHEMATICAL THEORIES OF ATTRACTION"[*]

A History of the Mathematical Theories of Attraction and the Figure of the Earth from the time of Newton to that of Laplace. By I. Todhunter, M.A., F.R.S. Two vols. (London: Macmillan, 1874.)

II.

OF the great Scotch mathematician, Maclaurin, we read—"The importance of his investigations may be seen by observing how great has been his influence on succeeding writers. Clairaut, D'Alembert, Lagrange, Legendre, Gauss, Ivory, and Chasles show by reference, explicit or implicit, their obligations to the creator of the theory of the attraction of ellipsoids.

Maclaurin well deserves the memorable association of his name with that of the great master in the inscription which records that he was appointed Professor of Mathematics at Edinburgh, "*ipso Newtono suadente.*" His main contribution to the theory of the figure of the earth was an exact demonstration of Newton's postulate, of which only approximate solutions had previously been given. We may note on § 260 that we have seen the French version of his "Treatise on Fluxions," "traduit de l'Anglois, par le R. P. Pezenas, Paris, 1749; 2 vols." The first volume has li. pp. of Introduction; v.-viii. *Avertissement par le traducteur* ix.-li., Translation of Author's Preface and Introduction with *Table des Matières;* then 344 pp. of text, plates, and 4 pp. of errata. The second volume has viii. pp. of contents, 322 pp. of text, plates, with 6 pp. at end (4 pp. of errata), for errata, *approbation*, and *privilège*.

The next noteworthy name is again that of an English writer, Thomas Simpson. His contributions are of eminent importance. The analysis he employed, Mr. Todhunter observes, "would not have been unworthy of Laplace himself." There is here an interesting biographical note of a kind which the writer so well knows how to introduce, and which adds a charm to the more general details. In writing our notice we have especially dwelt upon the English contributors to our subject; on the whole it can hardly be denied "that Newton's countrymen have left to foreigners the glory of continuing and extending his empire." Singularly enough Mr. Todhunter gives no account of Simpson's work, "A New Treatise of Fluxions . . . with a variety of new and curious problems." London, 1737. 8vo. This is six years earlier than the "Mathematical Dissertations." Problems XXI. to XXIII. (§§ 201-206) deal with attractions of a circular plate on a point on the axis; of a cylinder on a point on its axis; of a sphere on a particle on its surface, or any distance above it, for law varying as inverse of (distance)^2 and for (distance)^n. They correspond to Problems II., IV., V., VI. of 1823 edition of the "Doctrine and Application of Fluxions."

The great work of Clairaut, "Théorie de la figure de la terre," &c., appeared in 1743. In this branch "no other person has accomplished so much as Clairaut; and the subject remains at present substantially as he left it, though the form is different. The splendid analysis which Laplace supplied adorned but did not really alter the theory which started from the creative hands of Clairaut." Laplace, too, places it "au rang des plus belles productions mathématiques."

The expedition to Peru gave rise to much paper warfare, and Mr. Todhunter has collected together, in a useful form, the titles of the original pamphlets. We think he has overlooked the following, "Nouveau projet d'une mesure invariable propre à devenir universelle, extrait d'un memoire lû . . . le 24 avril, 1748, par M. de la Condamine," viii. pp. A copy we have consulted of No. xx. (p. 236) is dated "Plombières, juin 30, 1754." (Consult Lalande, p. 455.)

D'Alembert need not long detain us. Laplace points out that his writings want "clarté." Mr. Todhunter says of him, "The errors of D'Alembert are certainly surprising; they seem to me to indicate that he was little in the habit of enlarging his own views by comparing them with those of others. His criticisms of Clairaut prove that he had not really mastered the greatest work which had been written on the subject he was constantly studying. His readiness to publish unsound demonstrations and absolute errors is abundantly shown in the course of our criticism. On the whole the blunders revealed in the history of the 'Mathematical Theory of Probability,' and in the present history, constitute an extraordinary shade on a fame so bright as that of D'Alembert."

Here we must give an account of a work not mentioned in the History. The "Considerazioni sopra la Figura della Terra"[*] of Tommaso Narducci appeared about the year 1747. It comprises two Lemmas (in modern geometrical conics they would be for the ellipse (1), GN equal $\frac{CB^2\,CN}{CA^2}$, (2) radius of curvature equal $\frac{PG^3\,CA^2}{CB^4}$), and nine problems. The first problem is "Dati due gradi di meridiano e loro latidudine, trovare la ragione degl' assi, e gl' assi stessi;" the last is "Data la ragione de' due assi, che sia di 1 ad m, trovare nel meridiano un grado, che sia eguale al grado dell' equatore." It is an interesting piece of geometrical work.

In his § 490 Mr. Todhunter considers it curious that the (Cambridge) University library does not possess a complete copy of the famous work of Stay and Boscovich. His surprise will probably be increased when we state that, if we are not mistaken, neither do the libraries of the British Museum or that of the late Mr. Graves; at any rate, we do not remember to have met with Boscovich's commentary on the poem. "These writings furnish elementary accounts of the most important results which had been obtained up to their date, and reveal apparently great knowledge and judgment in Natural Philosophy." A copy of Boscovich's "Dissertatio de telluris figurâ, habita in seminario romano Soc. Jesu nunc primum aucta et illustrata ab ipsomet auctore, P. R. J. Boscovich," forms pp. 161-218 of vol. ii. of Giuliani's memoirs (cited at the foot, date 1744). In p. 184 he speaks of Maclaurin's "Fluxions" as "Newtono ipso dignissima;" there is a noteworthy passage, pp. 217, 218, and also a notice on p. xii.

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[*] Continued from p. 380.

[*] It occupies pp. 225–266 of vol. iii. of the "Memorie sopra la Fisica e Istoria Naturale di diversi valentuomini," edited by Carl Antonio Giuliani, in 4 vols. (1743–1747)

of the introduction of the volume.* The date on the copy of Frisi's dissertation (not seen by Mr. Todhunter) we consulted was 1751; it contains 8 pp. of *antecessio;* 86 pp. of text, 3 plates of figures, and 2 pp. of dedication. It consists of ten chapters, and may possibly, as Mr. Todhunter suggests, have been incorporated by Frisi in his "Cosmographia" (§ 532), though it certainly was not transferred bodily; on p. 65 he gets "axis terræ ad diametrum sibi normalem erit ut 229 : 230."

We have seen one of the works alluded to in § 551, viz., that of Bouguer; it is entitled, "Opérations faites par ordre de l'Académie . . . pour la vérification du dégré du méridien compris entre Paris et Amiens." Paris, 1757. 8vo. It consists of 28 pp., including two for title, and was read March 23, 1757.

In § 717 reference is made to the account of a measurement of an arc of the meridian in Lombardy by Beccaria, published at Turin "in 1744." We know of no work by Beccaria on this subject, except the "Gradus Taurinensis." The two copies of this which we have seen are dated 1774 and 1775; possibly from its position in the book the former of these dates is intended by Mr. Todhunter. It is a scarce work.

Mr. Todhunter has in § 725 devoted more attention to a supposed work by Newton than perhaps it really deserves. At any rate, De Morgan ("Budget of Paradoxes," p. 83) greatly doubts that Newton wrote it. He remarks that it has been treated with singular silence, and the name of the editor has never been given.

Lagrange contributes a memoir, in which he arrives by analysis at the point Maclaurin had reached by geometrical methods.

The operations carried on at Schehallien for ascertaining the density of the earth are next noticed, and with the conclusion of vol. i. the history of the two subjects during the century which followed the appearance of the "Principia" is nearly completed.

Some works mentioned as not having been seen (in § 738) may here be described.

The title of D'Anville's book is incorrectly given by Lalande.† For "*la* circonférence," read *sa;* for "*de l'équateur,*" read *sur les parallèles.* There are 8 pp. of dedication (to Duc de Chartres), 20 pp. of *avertissement,* 3 pp. of *privilège,* &c, 11 pp. of observations, and 147 pp. of text, with a plate.

Lalande describes very accurately the contents of the "Anecdotes physiques et morales."

Mayer's "Basis Palatina" has 6 pp. of dedication, 14 pp. *lectori astronomo;* 23 pp. are taken up with "Series et ordo triangulorum quæ ex propriis suis observationibus, anno 1763 habitis, deduxit et correxit C. M.; 2 pp. of *conspectus totius operis.*

Hennert's work (Utrecht, 1778) contains five dissertations, of which the fourth is—"Sur le mouvement que prend un corps, quand il est parvenu au centre d'attraction, et sur l'attraction considerée comme principe universel." It takes up pp. 125-166. On p. 166 he says—

> "Concluons de nos recherches qu'on n'a pas assès de preuves pour admettre l'attraction comme principe universel de tous les changemens qui arrivent dans le monde matériel. Nous avons vu que la cohésion des corps ne se déduit pas sans difficulté de l'attraction. . . . . Je sens bien, qu'il reste d'autres recherches à faire sur cette matière. J'en ai ebauché quelques unes que je pourrai publier si elles me paroissoient pouvoir contribuer au progrés des sciences physico-mathèmatiques."

His fifth dissertation is "Sur la figure de la terre relativement à la parallaxe de la lune et à la navigation." It occupies pp. 167-214. In § 18 we read, "Après avoir cherché inutilement de concilier les observations avec les hypothèses, dont les astronomes ont fait usage jusqu'aujourdhui, tachons de tirer un meilleur parti de notre formule générale," &c. And in § 25, "Il résulte de nos calcules que l'hypothèse de M. Bouguer et la notre donnent plus exactement le degré de longitude que l'hypothèse elliptique qui s'écarte considérablement de l'expérience." There are 4 pp. of dedication, 11 pp. of preface (interesting and amusing), 3 pp. of contents, 214 pp. of text, 2 pp. of errata, and 3 plates.

We may here describe a volume containing "Dissertationes de uniformitate motus diurni," by Hennert and Frisius, "præmio coronatæ," Petropoli, October 10, 1783. That by Hennert contains 42 pp., and *additamenta* making up 70 pp. in all; that by Frisius goes on to p. 112, and then 40 pp. more. They treat of attraction, but are not, apparently, of much importance.

Frisius it was who first introduced the ellipsoid as distinguished from the oblatum and the oblongum; from § 669, we learn, also, that he has no hesitation in adopting the truth as to the earth's motion.

Hube's work is in 87 pp. 8vo., with 1 page of plates. Its value may be seen from the following extract § 43:—

> "Demonstravimus itaque tellurem nostram omnino esse homogeneam, vel potius densitatem variam illius partium, ratione universæ massæ, nullius fere momenti esse . . . probavimus porro, pendulorum experimentis, differentiam gravitatis sub polo et æquatore, tantam esse, ut terræ forma elliptica omnino esse nequeat . . . ad finem itaque meum pervenisse mihi videor, qui tantum fuit, ut experimentorum hucusque factorum ope, summâ qua fieri potuit evidentiâ, telluris formam certo definirem ac omnia dubia, quæ in hac re jure moveri possent, tollerem; neque adeo immorabor in consectariis variis nostræ theoriæ explicandis, quoniam vera telluris forma semel certo stabilita, haud difficile est, eo colligere atque perspicere, quæ inde necessario efficiuntur."

Further particulars concerning Thomas Williams (§ 988) are given in the "Budget of Paradoxes," p. 102.

The work referred to in § 1,000, is a 4to. volume, and contains 1 page of *avertissement,* 13 pp. of introduction, 1 page of table, 94 pp. of text, and 1 page of errata (in this Houslowheat is corrected to Hounslowheat). We think we have somewhere seen the preface ascribed to Legendre; it is clear, however, from the introduction (from the words "deux de mes confrères de l'Académie M.M. Mechain et Legendre, p. xiv.) that it is due to Cassini, as Lalande correctly states. With the copy we examined was bound up "Descriptions des moyens employés pour mesurer la base de Hounslow Heath dans la province de Middlesex, publiée dans le vol. lxxv. des 'Trans. Phil.' par le Major-Général William Roy, traduite de l'Anglais par M. de Prony," Paris, 1787. There is a *discours préliminaire du traducteur* 18 pp. There is a notice of 1 page, a page of errata: the translation occupies 80 pp., there are 3 tables, and 5 planches. Mr

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* We have also seen another work, the "De centro Gravitatis, editio altera, accedit disquisitio in centrum magnitudinis" (Rome, 1751), but it does not, if we remember, bear upon our subject.

† There is a very valuable copy of this work ("Bibliographie Astronomique") in the Graves' Library, containing Lalande's autograph notes, and many other interesting features.

Todhunter gives no account of this work: it is apparently a translation of the work described in § 984.

The following work by Delambre and Legendre (will this throw light on § 1,146?) does not seem to be described: it is "Methodes analytiques pour la détermination d'un arc du méridien précédées d'un memoire sur le même sujet," par A. M. Legendre. Paris, 1799. 4to.) In note ii. to "Methode pour determiner la longueur exacte du quart du meridien," par A. M. L. occurs *Legendre's Theorem*. There are 176 pp., 16 pp. of tables at the end, 5 pp. of "Observations sur quelques endroits du mémoire du cit. Delambre," par A. M. Legendre; 2 plates, 7 pp. of *avertissement*, and 4 pp. of contents.

We now close our notes with a few remarks on §§ 531, 682, and 1,584. The memoir, which neither Playfair nor Mr. Todhunter have been able to procure a sight of, is to be met with in vol. i. of the Giuliani memoirs cited above, consequently its date is antecedent to 1743. It occupies pp. 65-88, and is entitled "Problema mecanicum de solido maximæ attractionis solutum, a P. Rogerio Josepho Boscovich." The problem is, "Data quantitate materiæ punctum attrahentis, in quacunque lege distantiarum invenire solidum ipsum continens, quod maximè omnium attrahat ipsum punctum positum in axe solidi producto ad datam distantiam ob ipsius solido vertice propiore." The author gives a geometrical and an analytical solution of the problem, and concludes, "Solutio geometrica in eo huic posteriori præstat quod ibi determinatur solidum maximæ attractionis etiam inter solida omnia irregularia, hic tantum inter solida genita rotatione curvæ circa axem."

The greater part of this second volume is taken up with the important writings of Laplace, Poisson, Gauss, Ivory, and Plana.

In the case of Laplace, who was, like some other writers, not in the habit of acknowledging his indebtedness to preceding authors, the result of the investigations is to restore his reputation to its proper eminence. "In the present history, and in that of Probability, I have gone over a third part of the collected mathematical works of Laplace; and to that extent the evidence of his great powers and achievements is, I hope, fully and fairly manifested."

Our work is now nearly done; were we to make use of all our notes, we might easily double and treble what we have written.[*] We have noted upwards of fifty articles which are interesting as sidelights: thus, § 227, Bouguer's remarks on what we now call the "personal equation;" § 710, transformation of variables in a triple integral, and many others. The value of the index would have been enhanced had reference been made to these; as it is, the index is much fuller than in the earlier volumes, and we have detected but few errors. This is no new feature, for all Mr. Todhunter's books are most carefully got up in this respect, and we have not met with a single important error in the mathematical work; the few mistakes we have come across are easily corrected.

There are indications here and there throughout the volumes that, should the writer be able to secure the requisite leisure, he will not want for subjects to exercise his special gifts upon. We sincerely hope that he will do so at an early date, and that it may be our lot to read the results. In the course of the preparation of the present work Mr. Todhunter has published, in the Royal and other Societies' proceedings, various papers which have grown out of his investigations in the history of our two subjects.[*]

R. TUCKER

TRAINING

Training in Theory and Practice. By Archibald Maclaren. Second Edition. (Macmillan & Co., 1874)

WHEREVER we go, "The Boat Race" is the topic of the hour. Opinions are freely expressed as to the relative merits of the rival crews; and the risks and dangers incurred during the process of training, sturdily insisted on by some, are as obstinately denied by others. The respective values of the slow and quick stroke—the American fashion of "sliding-seats," and a variety of kindred questions, are eagerly debated, occasionally by men who really understand what they are talking about, but more frequently in order to make conversation, or, as the phrase goes, "for the sake of something to say."

In such a state of the public mind, it has by a happy chance been so ordered that the second edition of Mr. Maclaren's well-known work on Training should appear, at a moment when all will take unusual interest in its contents.

From a perusal of this treatise everyone may gather information calculated to be of service to him, not only for the time being but for the future. The man who likes to know a little of everthing, may with advantage indulge his curiosity about a subject which has perhaps hitherto been out of his line; while the father with a son in "the eight," and whose mind has been disturbed by letters in the papers, hinting at dire mischief as the outcome of training, may find substantial comfort in its pages. All those who, either from necessity or for amusement, undergo much physical exertion, will do well to imbue themselves with the author's teaching; and the "man of ease" or of sedentary occupation will find hints for health or sanitary lessons of a description which, if honestly carried out, would cause the ruin of half the doctors in London.

Mr. Maclaren uses "rowing" as the peg on which to hang his theme, and the reason he gives for so doing is as follows:—

> "It is the exercise most susceptible of being influenced by a judicious system of bodily preparation, being at once an act of considerable intricacy, demanding long and assiduous practice, and an exercise of considerable difficulty, involving the possession, although not in an equal degree, of both muscular and respiratory power, to promote which is the object of all training."

The question, what is training? and what is it meant to do? he answers thus:—"It is, to put the body with extreme and exceptional care under the influence of all the agents which promote its health and strength, in order to enable it to meet the extreme and exceptional demands upon its energies." There can be no doubt that the essence of this paragraph is contained in the words "extreme and exceptional care;"—for without such care, instances are furnished to alarmists of men "fainting by the way," and a highly valuable art is in danger of being brought into disrepute. We have no fear for the men engaged in a great race like that which takes place on Saturday—they have been carefully instructed in the proper method of training—all that experience has taught has been lavished on them, and the result is, as Dr. Morgan's statistics show, that they live as long as other portions of the civil community.

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[*] In our remarks we have preferred to treat the subjects from an historical rather than from a mathematical point of view.

[*] Possibly some further memoirs of use might be found in the "Librorum in Bibliotheca Speculæ Pulcovensis, anno 1858, exeunte contentorum catalogus systematicus," by Otto Struve. Petropoli, 1860.

But on the other hand we do fear for the many persons who assert that they are in training for some race or other physical feat, but from whom on inquiry we learn that their notions and practice are so desultory, and so deficient in anything like scientific detail, that it is an abuse of the word training to apply it to their misdirected proceedings. They forget, or at least do not sufficiently consider, that the feat they are about to attempt will require of them, at some critical moment, a supreme effort; and that in the making of this effort, a lasting injury may be inflicted upon a frame that is only imperfectly prepared.

To them we say, "if a thing is worth doing at all it is worth doing well." Train thoroughly or not at all—you have no right to jeopardise your future by "extreme carelessness" when all bad results may be avoided by "extreme care."

In former times Diet was looked upon as of paramount importance among the agents of health in training; Mr. Maclaren, however, places exercises in the front rank, and justifies himself for doing so in the following logical and telling words:—"So long as men believe that the qualities which they covet are to be obtained from mere dietary regulations, they will neglect the real agencies which can alone bestow them; exercise, the one agent which gives, which can give, these qualities, both from its own nature and from the influence which it exerts upon all other agents of health, is, in a measure, neglected, nay, avoided, and to the imaginary virtues of diet men look for the longed-for acquisitions; they have yet to learn, they have yet to know, and to themselves realise, that power of muscle in trunk and limb, that freedom and capacity of heart and lung, that energy, stamina, strength and endurance, are not to be obtained from what they *eat*, but from what they *do*."

To each of these subjects, Exercise and Diet, a separate chapter is devoted; and each is treated in a masterly and exhaustive manner. Our space will not, however, permit of more than an allusion to them. Mr. Maclaren deprecates the error of confining the attention to any one form of exercise, on the ground that it must be insufficient to produce the desired result, that is to say, the increased action, and thereby the fuller development, of all the muscles of the body. He illustrates his meaning by the example of the man who, having one favourite author or favourite object of study, fails to cultivate or employ his *whole* mind. On the subject of Diet he is much less stringent and exclusive than is customary, and we think rightly so, recognising the fact that each one must be a law to himself; whence the truth of the trite saying, "What is one man's meat is another man's poison." His views on this point may be summed up in his own words: "I would only advocate the rational system of not suddenly breaking in upon a man's fixed habits, at the time you are asking for an effective display of his greatest bodily energies."

Another moot point, the question of the amount of sleep required by different persons under the same circumstances, is next discussed. No hard and fast rule is laid down, but the breadth of view that is one of the great charms of the book is again apparent. The opinion is expressed that the time to be devoted to the purpose should vary not only with the individual, but with the same individual at successive periods of life, and that the wants of the system, in this respect, are influenced by various causes, and by the action of the other agents of health, especially by exercise. With reference to this matter, we have frequently observed that tall persons require a longer period of recumbency than short, whose hearts are called upon for less powerful exertion, by reason of the smaller height of the column of blood that they have to sustain and propel.

The chapters next in order deal with the important but still subsidiary questions of air, bathing, and clothing. We cannot at present enter into detail with regard to any of these subjects, but we would add to Mr. Maclaren's observations on bathing, that one great secret of using a cold bath in the dressing-room without discomfort or injury, is to sit down in the water in the first instance, and to wash the upper part of the body, thus somewhat raising the temperature of the water before the feet are immersed.

The concluding portion of the book is devoted to very clear and practical directions for self-training for aquatic purposes, and in this, as in all other parts, it will be found a complete and trustworthy guide by those for whose use it is intended.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Herbert Spencer versus Thomson and Tait

A FRIEND has lent me a copy of a pamphlet recently published by Mr. Herbert Spencer, in which certain statements of mine are most unsparingly dealt with, especially in the way of attempted contrast with others made by Sir W. Thomson and myself. I am too busy at the present season to do more than request you to reprint one of the passages objected to (leaving it to your readers to divine to what possible objections it is open), and to illustrate by a brief record of my college days something closely akin to the mental attitude of the objector.

"Natural philosophy is an experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth" (Tait, *Thermodynamics*, § 1).

One of my most intimate friends in Cambridge, who had been an ardent disciple of the late Sir W. Hamilton, Bart., and had adopted the preposterous notions about mathematics inculcated by that master, was consequently in great danger of being plucked. His college tutor took much interest in him, and for a long time gave him private instruction in elementary algebra in addition to the college lectures. After hard labour on the part of each, some success seemed to have been obtained, as my friend had at last for once been enabled to follow the steps of the solution of a question involving a simple equation. A flush of joy mantled his cheek, he felt his degree assured, and he warmly thanked his devoted instructor. Alas, this happy phase had but a brief duration; my friend's early mental bias too soon recovered its sway, and he cried in an agony of doubt and despair, "But what if x should turn out, after all, *not* to be the unknown quantity?"

Compare this with the following extract from Mr. Spencer's pamphlet:—

" . . . if I examine the nature of this proposition that 'the properties of matter *might have been*' other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments!" P. G. TAIT

Animal Locomotion

MY former letter on this subject was merely to show that, mechanically, Dr. Pettigrew's view of the forward motion or inclination of a bird's wing during the down stroke was less absurd than had been supposed, and even seemed necessary to flight. I did not profess to have made accurate observation or experiment on the point. I accept, therefore, the observation of the Duke of Argyll as to the vertical motion of the heron's wing; but as he expressly refers to its great concavity, that would give a vertical down stroke the effect of a somewhat forward stroke of a flatter wing. The proper inference would therefore seem to be, that in birds with less concave wings the stroke is slightly directed forwards. As to the last two paragraphs of his Grace's letter, he will see, if he refers again to mine, that he has quoted words I never used. I impute to Dr. Pettigrew the "merit of showing" that the "*slight upward angle* of the mean position of the wing plane is essential to secure horizontal forward motion *as* a general resultant," &c., and this is exactly what the Duke denies.

Mr. James Ward's elaborate analysis of the down stroke of a bird's wing simply shows (if correct) that in the position he ascribes to it (moving downward and backward) it would send the bird horizontally forward. Of course it would. But then what becomes of the bird during the up stroke in an opposite direction? The bird is then falling, and by the downward reaction of all the solid surface of the anterior margin of the wing, and of all the feathers, however obliquely turned, it is driven farther downwards; and as this takes place between every two down strokes, and approximately during an equal space of time, how is a horizontal average motion to be produced unless the down stroke alone produces, not a horizontal, but a highly-inclined upward motion? Mr. Ward's whole argument appears to me to ignore the great downward reaction, added to gravitation, during every up stroke, which requires that the down stroke should not merely support the bird, but raise it up vertically just as much as during the up stroke it has fallen vertically. The matter, however, is not to be settled by discussing theoretically, but by observation and experiment. I simply maintain that the results of Dr. Pettigrew's observations and experiments are not, as supposed, inconsistent with mechanical principles; and nothing in your correspondent's letter induces me to alter that opinion.

ALFRED R. WALLACE

The Newfoundland Cuttle-Fish (*Megaloteuthis harveyi* S. Kent)

MY right being questioned, through an anonymous paragraph in the *Globe* of the 11th inst., to institute a new generic title for the gigantic Cephalopod encountered off Newfoundland, and of which I communicated an account to the Zoological Society's meeting of March 3, I would briefly reply to my criticiser in these columns.

In the first place, it is a somewhat anomalous proceeding to raise objections on such a question before details of the grounds upon which it has been deemed advisable to establish such a title have appeared, as in the ordinary course of events they will, in the "Proceedings" of the Society. In these it will be found that ample reasons are given for the course that has been taken, as also due notice of both Prof. Steenstrup's and Prof. Verrill's researches in a similar direction. Had my assailant placed himself more thoroughly *au courant* with the details of the case, he would possibly have held back his emphatic assertion that Prof. Verrill had "actually identified the species from Newfoundland with those described by Steenstrup as belonging to his genus *Architeuthus*." This identification in Prof. Verrill's own language is entirely problematical, and must unfortunately remain so, since a beak only, an organ of no value in generic discrimination, has been preserved of the typical species *A. dux* Respecting the second form, *A. monachus*, we have still less knowledge, the title being provisionally instituted by Prof. Steenstrup for the reception of two gigantic Cephalopods cast on the shores of Jutland in the years 1639 and 1790, and of which popular record alone remains.

In reference to the "imperfect evidence" asserted by my critic to be at my command, I may state that I received accounts of the examples, upon which I have proposed to base my new title of *Megaloteuthis*, direct from America as long ago as in the beginning of December last, supplemented by numerous fuller details since.

W. SAVILLE-KENT

Lord Lindsay's Expedition

THE expedition of Lord Lindsay for observation of the Transit of Venus at Mauritius (why will people still call it *the* Mauritius?) will afford a good opportunity for re-measuring the base line of Abbé de la Caille, made in 1753, and which, to the best of my belief, has never been since verified.

The small conical cairns which mark its extremities should still be found *in situ*. I saw one only of them in November 1864, when I had not time to search for the other. The base measured was 1,828 toises in length, and, I imagine, on the meridian. It was on a level plain at the south-west extremity of the island, close under the western slopes of the precipitous and noble "Morne du Brabant," which rises nearly 1,700 feet above the sea-level. By road the distance of this spot from Port Louis must be at least 30 miles, but it is much more easily reached direct by boat; or, as December is a bad time of year for boating outside the reefs, the best route would be from Black River by water inside the isle Bénitier. It is a glorious district, all that part of the island, and contains the finest scenery, including the Chamarel Falls.

S. P. OLIVER

Buncrana, near Londonderry, March 14

QUETELET

ON February 17 last Jacques-Adolphe-Lambert Quetelet died at Brussels, in the seventy-eighth year of his age, having been born on February 22, 1796, at Ghent. At the early age of 18 he was appointed Professor of Mathematics in the College of his native town. In July, 1819, the degree of Doctor of Science was conferred on him by the University of Ghent, then recently founded by King William. His dissertation on this occasion was so well received that he was shortly thereafter appointed to the Chair of Mathematics at the Royal Athenæum of Brussels; and in February following was elected a member of the Academy of Sciences and Belles-Lettres.

At this time he applied himself with ardour to the cultivation of literature and pure mathematics, thus laying a sure foundation for the world-wide fame he afterwards achieved as an exact investigator in many departments of physics, as an original thinker in applying methods of scientific treatment to the discussion of problems previously considered as belonging exclusively to moralists and divines, and as a clear and eloquent expounder of the truths he had demonstrated. The many-sidedness and fertility of his mind may be seen from his scientific memoirs enumerated in the Royal Society's Catalogue of Scientific papers, amounting at the close of 1863 to 220. He continued to write almost to the last, notwithstanding the mental malady, consisting in loss of memory, with which he was afflicted many years before his death, and it is noteworthy that even to the last his handwriting retained much of the rare grace and elegance for which it had been so remarkable.

The earliest of Quetelet's published memoirs, begun in 1820, were on geometrical subjects. The non-appreciation of these by the public determined him to devote himself to physical science and astronomy. On these subjects he lectured publicly with great success.

In 1823 he was sent on a mission to Paris with the view of preparing a report on the observatory of that city for the guidance of the Belgian Government in founding a similar observatory at Brussels. After some delay, the observatory was established, with Quetelet as director, and in 1833 began the long series of observations on astronomy, meteorology, and other physical inquiries, for which this observatory is so well known. The most important of his astronomical observations was the preparation of a catalogue, begun in 1857, of stars which seem to have appreciable motion. He also began, so early as 1836, systematically to observe and record the occurrence of meteors and shooting-stars. These observations came to be of great value thirty years later, when the true nature of these bodies was satisfactorily established.

The meteorological observations at this observatory have been particularly full and valuable, embracing hourly and bi-hourly observations, published annually *in extenso*, of atmospheric pressure, temperature, humidity, rain, cloud, &c. These have been exhaustively discussed by Quetelet in "La météorologie de la Belgique comparée à celle du globe," published in 1867. In this admirable treatise we have what must still be regarded as the fullest and best account of the meteorology of any single locality on the globe—the yearly, monthly, daily, and hourly march of the various meteorological elements being given. In the same volume are given *résumés* of the observations made at the other stations which began to be established at Alost, Ghent, Liége, &c., in 1835.

He was elected perpetual secretary of the Academy of Sciences and Belles-Lettres in November 1834, and was chiefly instrumental in adding a section on the Fine Arts in 1845. It is scarcely necessary to refer to the scientific contributions he made to the Fine Arts, by his extensive and minute investigations regarding the proportions of the human body, the results of which are given in his "Anthropométrie." In matters relating to the higher education, to the census, and other national questions, the Belgian Government wisely availed itself repeatedly of his wide knowledge and great experience.

His first paper on the subject of statistics was published in 1826; in 1835 appeared his "Physique sociale," and ten years later his "Lettres sur la théorie des probabilités appliquées aux sciences morales et politiques." In 1841 a Central Commission of Statistics was established by royal decree, of which Quetelet was made president, and of which he continued to be president to his death. He originated the idea of convening an International Congress of Statistics. The first was held in Brussels in 1853, and others have since been held at Paris, London, Berlin, Florence, the Hague, and St. Petersburg. It is in the field of statistics that Quetelet appears as a great discoverer, and his success in this department must be attributed to the clearness with which he saw that statistics occupy the ground in the development of the social and political sciences which observational data do in the development of such sciences as astronomy and meteorology, to the patient industry with which through long years he gathered together his facts, and to the mathematical skill he brought to bear on the discussion of the results. He was truly, as expressed by the Academy of Berlin in their congratulatory letter on the occasion of the centenary of the Belgian Academy, "the founder of a new science which proceeds from the firm basis of observation and calculation to discover and unfold those immutable laws which govern the phenomena, apparently the most accidental, of the life of man, down even to his most trivial actions."

SCIENTIFIC RESULTS OF THE "POLARIS" ARCTIC EXPEDITION

We have received advanced sheets of the Report of the Secretary of the United States Navy, of the examination of those of the crew of the *Polaris* who were in the ship when she broke loose from the floe to which she was anchored, on October 15, 1872, leaving the nineteen persons on the sheet of ice which was their floating home, until picked up about six months after off the coast of Labrador (NATURE, vol. viii., p. 217). This report confirms the opinion we have already expressed that no Arctic expedition can be adequately conducted unless carried out under naval discipline. It was only on account of the good intentions and good nature of the crew, especially after their noble and enthusiastic captain's death, that things went on as smoothly as they did. Captain Buddington seems to have had no heart in the object of the expedition, and we cannot help thinking that had he not been with it much more would have been gained. It was in deference to his opinion that Captain Hall refrained from trying to push beyond his furthest point (82° 16′ N.) with the ship; all the other officers, though they do not seem to have been very well assorted, being of opinion that an attempt should be made to get further north, or at least not to lose ground by wintering further south.

We have already (vol. viii., p. 435) given details as to the rescue of those who were left in the *Polaris*, and of their being landed in Scotland by the *Arctic* and *Eric* whalers. The present report affords some idea of the scientific results of the expedition, a detailed account of which will no doubt by and by be published, although we regret to see that many of the records of the scientific results were lost in the confusion incident to the parting of the ship from the floe. Still much that is valuable has been brought home, from which many additions to a scientific knowledge of that part of the Arctic region will be obtained. Notwithstanding the want of perfect harmony among the officers, the scientific work of the expedition seems to have been diligently carried on, and the evidence of Dr. Bessels especially contains a great deal of value to Science. Geographers will be able to correct and extend their maps of the regions visited, and we hope that very soon the complete material for enabling them to do so will be in their hands. Constant and careful tidal observations were carried on, with the very valuable result of ascertaining that the tide of Thank-God Harbour, 81° 38′ N., is not produced by the Atlantic but by the Pacific tidal wave. "It was found," Dr. Bessels says, "that the co-tidal hour is about $16^h 20^m$. Rensselaer Harbour, being the northermost station, has its co-tidal hour at $18^h 04^m$, consequently the tide comes from the north, the rise and fall at spring-tides amounting to about 5 ft.; at neap tides $2\frac{3}{10}$. Most likely the two tidal waves meet somewhere in Smith Sound, near Cape Frazier. Kane and Hayes have both found a ridge of hummocks near Cape Frazier, and in drifting down we experienced that during some time, being abreast of Cape Frazier; we hardly made any headway, but we drifted both north and south."*

The results of the expedition may be summed up briefly as follows:—(1) the *Polaris* reached 82° 16′ N., a higher latitude than has been attained by any other ship; (2) the navigability of Kennedy Channel has been proved beyond a doubt; (3) upwards of 700 miles of coast-line have been discovered and surveyed; (4) the insularity of Greenland has been proven; and (5) numerous observations have been made relating to astronomy, magnetism, force of gravity, ocean physics, meteorology, zoology, ethnology, botany, and geology, the records of which were kept in accordance with the instructions supplied by the National Academy, and some of the results of which we propose briefly to enumerate.

ASTRONOMY.—Great care was taken in determining a reliable meridian at Thank-God Harbour. Soon after entering winter-quarters an observatory was erected on the shore, thirty-four feet above mean sea-level, and the transit instrument stationed there. The longitude of this station was determined by the observation of 300 lunar distances; a number of moon culminations; a great number of star transits; a number of star occultations; a great number of altitudes of the sun on or near the prime vertical. Its latitude, by the observation of a great number of circummeridian altitudes of the sun, and a number of altitudes of stars. All of these observations were lost, but a number of the results have been preserved which are sufficient to establish the position of his station.

The instruments used in the above observations were a Würdemann transit and Gambey sextants divided to 10″. The expedition carried six box chronometers made by Negus, three of which indicated sidereal time, and four pocket chronometers by different English makers. These time-pieces were compared every day at precisely the same time, and the result entered in the chronometer-journal.

Besides the above-mentioned observations, twenty sets of pendulum experiments were made, which are saved, but the observations for time belonging to them are lost.

MAGNETISM.—The magnetic observations obtained were more complete than any others ever before made in the Arctic regions. The instruments supplied were:—one unifilar declinometer; one dip circle, with Lloyd's needles; one theodolite; and several prismatic compasses.

The observations on variation of declination were registered at Göttingen time, and were continued for five months: readings taken hourly. Besides that, three term days were observed every month, according to the Gottingen regulations, one of these term days corresponding with the day accepted by all the magnetic stations. Further, a number of observations were taken either with the theodolite or the prismatic compass. Whenever possible, the dip was observed, and several sets of observations on relative and absolute intensity and of the moment of inertia were obtained.

OCEAN PHYSICS.—Unfortunately there was not much opportunity for taking soundings. About twelve were obtained along the coast of Grinnell Land, which prove that the hundred-fathom line follows the coast at a distance of about 15 miles in Smith's Sound. One of these soundings (90 fathoms) proved highly interesting, containing an organism of lower type than the *Bathybius* discovered by the English dredging expedition. It was named *Protobathybius robesonii*.

A number of deep-sea temperatures were taken with corresponding observations on the density of the water. Following the coast of West Greenland the limits of the Gulf Stream were ascertained. Specimens of water from different depths were preserved in bottles, but were, unfortunately, lost.

As soon as the vessel was fairly frozen in, a tide-gauge was erected over a square hole cut in the ice-floe, and kept open continually; the pulley and rope were supported by a tripod of oars. A rope, to which a wooden scale, divided into feet and inches, was fastened, was carried through a block attached to the tripod. One end of the rope was anchored at the bottom by means of two thirty-two pound shot, and a counterpoise was attached to the other end to keep the rope properly stretched. This apparatus was tested by a series of scale readings with corresponding soundings, and proved to work very satisfactorily. The observations comprise eight lunations, the readings being taken hourly, half-hourly, and in some instances every ten minutes, in order to determine the precise moment of the turn of the tide.

METEOROLOGY.—After having entered winter-quarters meteorological observations, which up to this time had been made three-hourly, were made every hour, Washington time. The register contained observations on the temperature of the air, atmospheric pressure, psychrometrical observation, direction and force of wind, appearance of the sky, state of weather, and both solar and terrestrial radiation. Besides, all extraordinary meteorological phenomena were carefully noted.

For the registration of the temperature of the air mercurial thermometers were used for temperatures down to $-35°$ F.; for lower ranges spirit instruments being compared at intervals of 10°. As circumstances would permit, mercurial or aneroid barometers were used. As it was not supposed that psychrometrical observations could be favourably conducted at very low temperatures, the expedition was not supplied with the suitable instruments. For that reason two uncoloured spirit thermometers were selected and used, the readings of which agreed. As check observations the dew-point was determined by means of Regnault's apparatus. To measure the velocity of the wind, Robinson's anemometer usually served. The distance travelled by the wind was noted hourly, at the same intervals of time. The velocity of the wind was determined either by the same instrument or by means of Casella's current-meter. These observations on the winds, combined with those on moisture of the atmosphere, will form a valuable contribution to physical geography.

It was not thought essential to procure photographs of the clouds, as they do not differ in their general character from those in more southerly latitudes. The only remarkable fact to be noticed is that sometimes cirri could be observed at very low altitudes among stratus clouds, which, however, is not surprising if their mode of formation is taken into account.

Special attention was devoted to the aurora borealis, which occurred frequently, but rarely showed brilliant colours, never bright enough to produce a spectrum. Whenever necessary one observer was stationed at the magnetometer and the other out-doors, the former observing the motions of the magnets, while the other was watching the changes in the phenomenon and taking sketches. Although an electroscope and electrometer were set up, and the electrical condition of the atmosphere frequently tested, in no instance could the least amount of electricity be detected. The amount of precipitation was measured as carefully as the violent gales would permit, by means of a rain-gauge supplied with a funnel. In February, as soon as the sun re-appeared, observations on solar radiation were commenced, and continued throughout the entire summer. The instruments employed were a common black-bulb thermometer, and one *in vacuo*; both exposed on white cotton.

ZOOLOGY AND BOTANY.—The collections of natural history are almost entirely lost. With the exception of two small cases containing animals, minerals, and one package of plants, nothing could be rescued. The character of the fauna is North American, as indicated by the occurrence of the lemming and the musk ox. Nine species of mammals were found, four of which are seals. The birds are represented by twenty-one species. The number of species of insects is about fifteen, viz.: one beetle, four butterflies, six diptera, one bumble-bee, and several ichneumons, parasites in caterpillars. Further, two species of spiders and several mites were found. The animals of lower grade are not ready yet for examination.

The flora is richer than could be expected, as not less than seventeen phanerogamic plants were collected, besides three mosses, three lichens, and five fresh-water algæ.

GEOLOGY.—Although the formation of the Upper Silurian limestone, which seems to constitute the whole west coast north of Humboldt Glacier, is very uniform, some highly interesting and important observations have been made. It was found that the land is rising, as indicated, for instance, by the occurrence of marine animals in a fresh-water lake more than 30 feet above the sea-level and far out of reach of the spring-tides. Wherever the locality was favourable the land is covered by drift, sometimes containing very characteristic lithological specimens, the identification of which with rocks in South Greenland was a very easily accomplished task. For instance, garnets of unusually large size were found in latitude 81° 30′, having marked mineralogical characteristics by which the identity with some garnets from Fiskernaes was established. Drawing a conclusion from such observations it became evident that the main line of the drift, indicating the direction of its motion, runs from south to north.

*THE COMMON FROG**

XII.

SO much for the circulation of the frog in its adult condition. Its larval, or tadpole stage, presents us with a series of changes which, though more familiar, are not less wonderful.

In the first place, however, it may be well to describe shortly the condition of the circulation in fishes, where the purification of the blood is effected, not by means of the exposure of the blood to the action of air taken into respiratory cavities of the body, but by its subjection in little plates of membrane, the gills, to the influence of air mechanically mixed up with and dissolved in the water in which those gills are bathed.

In fishes, moreover, unlike all air-breathing animals, none of the oxygenated blood is returned to the heart for propulsion, but is collected directly into the great dorsal aorta, whence it is distributed to the whole body, only being returned to the heart after such distribution, so that venous blood alone enters that organ.

This venous blood is sent out from the heart through a bulbous aorta, whence arise on each side a series of arteries which ascend the branchial arches, one on the outer side of each such arch, decreasing in size as it ascends.

Each branchial artery gives off small gill arteries, which run along one edge of each little membranous leaflet or gill, and supply it with minute branches ending in capillaries, in which the blood is purified. There the purified blood is taken up by minute veins which open into gill veins, one of which runs along the opposite edge of each gill to that occupied by the gill artery.

The gill veins pour their contents into branchial veins, one of which ascends the outer side of each branchial arch, increasing in size as it ascends. The branchial veins open into the great dorsal aorta, whence the blood is distributed over the body. Generally the branchial arteries are only connected with the branchial veins by the intervention of the capillary vessels of the gills. Sometimes, however (as *e.g.* in the mud-fish, *Lepidosiren*), the branchial veins are directly continuous with the branchial arteries.

In the tadpole, while the gills remain fully developed, a condition exists quite similar to that of fishes. Minute vessels, however, directly connect together, at the root of each gill, the branchial artery and branchial vein of each gill. Such a connecting vessel is termed a *ductus botalli*.

A minute vessel given off from the third branchial artery, is the incipient pulmonary artery.

As development proceeds, as the gills diminish by absorption, and as their respective arteries and veins decrease in size and importance, each *ductus botalli* increases until at last we have established the six great continuous vessels of the adult frog.

We have, then, in the life-history of the frog, a complete transition from the condition of the fish to that of a true air-breathing vertebrate, as regards its circulation. The various conditions herein referred to have, however, an important bearing on the question of the first origin of such structure.

All higher animals, even the very highest, have the great arteries, when they first appear, arranged substantially as in fishes.

From the common aortic bulb five vessels ascend each side of the neck, and more or fewer of these arteries abound in different classes, the permanent adult condition being arrived at by this circuitous route.

This argument has commonly been adduced as an argument in favour of the descent of air-breathing animals from more ancient gill-bearing forms, and it is not without weight.

Nevertheless it must be borne in mind that the primitive condition in Fishes is that of direct continuity between the branchial arteries and veins such as we have seen exists permanently in *Lepidosiren*. It is only as development proceeds that each primitive continuous arch becomes broken up into an artery and a vein connected by a net-work of capillaries.

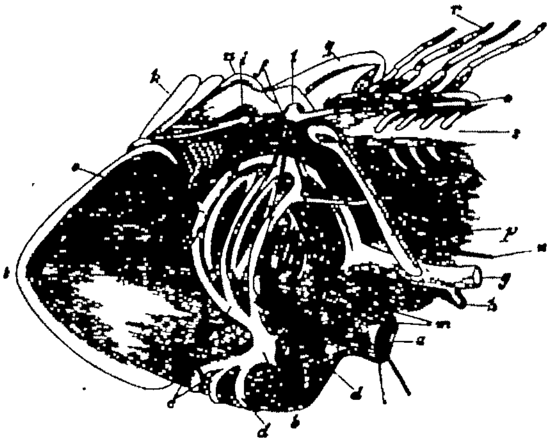

FIG. 78.—Infero-lateral view of Head and Aortic Arches of Lepidosiren (after Hyrtl). *a*, œsophagus; *b*, anterior end of bulbus aortæ; *c*, common roots of the first aortic arches; *d*, third aortic arch, *e*, first aortic arch; *f*, dorsal union of the first three aortic arches; *g*, aorta, *h*, cœliac artery; *i*, exit of the fifth nerve, *k*, part of operculum; *l*, exit of the nervus vagus from the skull; *m*, branches to œsophagus; *n*, nerve going to the rectus abdominis; *o*, nervus lateralis; *p*, first and hypertrophied rib, *q*, posterior part of the skull; *r*, segmented neural spines, *s*, chorda dorsalis, *t*, mandible; *u*, quadrate.

Now we can understand the series of unbroken arches in higher animals as the relics of ancestral vessels which divided for gill circulation and were therefore once of extreme functional importance and utility. But how can we understand the primitive unbroken series of arches in Fishes? Their utility was yet to come!

The frog when adult has, besides its skin, no breathing organs but the lungs. As has been said before, other members of the Frog's class retain gills and aquatic respiration during the whole of life, as for example *Menobranchus*.

Every one kind, however, whether provided permanently with gills or not, develops lungs, and it might easily be imagined that similarly every gilled-creature which has lungs is also a Batrachian.

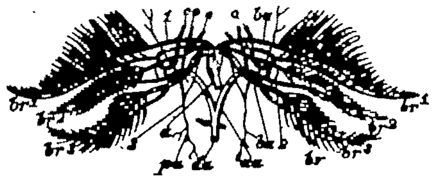

FIG. 79.—The Circulation of a Tadpole in its primitive stage, when nearly all the blood is distributed to the gills, the pulmonary arteries being quite rudimentary, and the vessel (or ductus botalli) connecting together the branchial artery and vein at the root of each gill being minute. *a*, bulbus aortæ; *b*, branchial arteries; *br1*, *br2*, *br3*, the three gills (or branchiæ of each side); *bv*, the branchial veins which bring back the blood from the gills—the hindermost pair of branchial veins on each side unite to form an aortic arch (*aa*), which again unites with its fellow of the opposite side to form *da*, the descending (or dorsal) aorta. The branchial veins of the foremost gills give rise to the carotid arteries, *cc*. *e*, artery going to the orbit; *pa*, pulmonary artery, 1, 2, 3, anastomosing branches connecting together the adjacent branchial arteries and veins.

This, however, would be a mistake.

The Mud-fish or *Lepidosiren*, already referred to more than once, is furnished with both gills and lungs throughout the whole of life. On this account it has been reckoned by some naturalists to be a Fish and not a Batrachian. Its fish-nature, however, has now been thoroughly established, and thus the probability of the existence of lungs within the class of fishes is also established.

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* Continued from p. 369

But what is a lung?

A lung is a sac-like structure capable of being distended with air, supplied with venous blood direct from the heart and sending arterial blood directly to it. Generally the whole of the blood from the lungs goes back to the heart directly, but in one Batrachian—the celebrated *Proteus*—a portion of the blood from the lungs finds its way not into the heart but into vessels of the general circulation. When there is an air-sac which does not both receive blood directly from and return it directly to the heart—*i.e.* when there is no true *pulmonary* circulation—such an air-sac (whether single or double) is termed a *swim-bladder* and a structure of the kind is found in very many fishes. The swim-bladder of ordinary fishes neither receives blood directly from the heart by an artery like the pulmonary artery of higher animals, nor does it return blood directly to the heart.

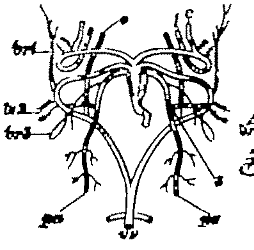

FIG. 80

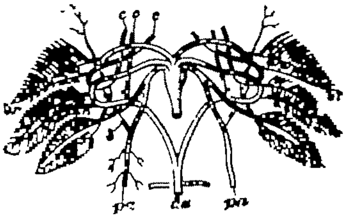

FIG. 81.

FIG 80.—The Circulation in a Tadpole at a more advanced stage, when the gills have begun to be absorbed, the pulmonary arteries to increase, as also the connecting branches (at the root of the gills) between the branchial arteries and branchial veins

FIG 81 —The Circulation in a young Frog Here the gills have been absorbed, and the blood passes directly from the heart to the head, the dorsal aorta, the lungs, and the skin,

The transition, however, from a lung to a swim-bladder is a graduated one. We have just seen that in *Proteus*, though blood is returned from the lungs direct to the heart, yet that not all the blood is so returned. On the other hand in another animal, *Ceratodus*, though blood is not brought to its air-sac directly (which is therefore a swim-bladder and not a lung), yet for all that blood is sent *from* it direct to the heart.

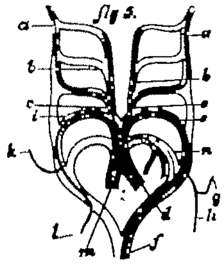

FIG 82

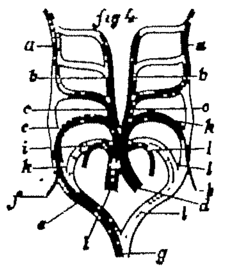

FIG. 83.

FIG. 82 —Diagram representing the main arteries of a Bird (fowl) with the changes induced on the primitive condition (after H. Rathke) *a*, *a*, internal carotids; *b*, *b*, external carotids, *c*, *c*, common carotids, *d*, root of main aortic arch (here right), *e*, arch of the same; *f*, right subclavian (which arises from the anastomosis of the first two right primitive aortic arches); *g*, commencement of the descending aorta; *h*, *h*, left subclavian, *i*, *i*, *i*, pulmonary arteries; *k*, right, and *l*, left, rudiments of the primitive aortic arches

FIG. 83 —Diagram representing the main arteries of a Mammal with the changes induced in the primitive condition (after H Rathke) *a*, *b*, *c*, carotids, as before, *d*, root of main aortic arch (here left), *e*, arch of the same; *f*, commencement of descending aorta, *g*, left vertebral artery, *h*, left subclavian; *i*, right subclavian, *k* right vertebral artery; *l*, continuation of right subclavian; *m*, pulmonary artery; *n*, remnant of left primitive aortic arch.

Ceratodus (or as it is locally called "flat-head") is a fish of Queensland, closely allied to *Lepidosiren*, and is a very noteworthy animal apart from and in addition to its peculiarly transitional structure as regards its air-sac.

It is, indeed, the last of an ancient race, a species of the same genus (known almost exclusively by its teeth) being found fossil in strata of oolitic and triassic date. It was discovered by the Hon. W. Foster, M.C.A. Mr. Gerard Krefft, F.L.S., Curator and Secretary of the Sydney Museum, first described and figured the animal in 1870,* and at once correctly referred it to the genus *Ceratodus*, which up to that time was supposed to be entirely extinct. Its further determination was effected by Dr. Gunther.† He has conclusively shown that *Ceratodus* and *Lepidosiren* are closely allied, and thus finally brought the latter definitively within the class of Fishes, for that *Ceratodus* is a fish no one questions. It is an animal, however, of somewhat amphibious habits, as at night it leaves the brackish streams it inhabits, and wanders amongst the reeds and rushes of the adjacent flats. Vegetable substances constitute its principal food.

Ceratodus and *Lepidosiren* together afford the most remarkable evidence of the persistence of the same type of structure in the Vertebrate sub-kingdom. The group to which they both belong reaches back into the very earliest epoch, which has yet afforded us any evidence whatever of the existence of fishes; while the genus *Ceratodus* seems to have persisted unchanged from the period of the deposition of the triassic strata.

Summary.

Taking a rapid retrospect of the course we have pursued, we find that in seeking to decide as to "What is a Frog?" our inquiry into its absolute structure has made known to us an animal of peculiarly specialised and perfect organisation. This has been shown to us pre-eminently by the study of its skeleton. We have especially noted its skull, its wonderfully short vertebral column, its utterly anomalous pelvis, and its scarcely less anomalous foot The flesh which clothes that skeleton has been seen to exhibit distinct muscles wonderfully like our own, those of the foot, indeed, exceeding ours in number, and being a very marvel of complexity. We have met with a nervous system ministered to by delicate organs of sense, and noted for the ready response to stimuli, made by even separated parts of it as evidenced by strikingly co-ordinated complex movements. We have found the circulation to be carried on by a heart which, at first sight, seems too structurally imperfect to distribute the venous and arterial blood in their respectively appropriate channels. Nevertheless, further examination has shown us that this heart is provided with a special arrangement of parts so delicately co-adjusted as to be able to act thus as efficaciously as does the heart of animals much higher in the scale. Respiration, too, we have seen provided for partly by an effective throat air-pump, partly by a peculiar activity of the cutaneous structures.

We have, moreover, found that this complex adult condition is arrived at by means of a rapid metamorphosis from an immature condition wonderfully different, indeed, but no less perfectly adapted to the life conditions of the tadpole state.

It remains now "to sum up the results" of our investigations through "a series of wider and wider comparisons" to answer, finally, as far as may be, the initial question of this little treatise.

We have, in the first place, seen that the frog belongs to an order far more distinct from cognate ordinal groups than is man's order from other orders of his class mammalia. We have also seen that the frog belongs to an order which is singularly homogeneous, and yet that the class which includes it is remarkably heterogeneous.

Again, we have found that the subordinate groups of the frog's order, families and genera, have very definite

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[*] See "Proc. Zool." Soc 1870, p. [illegible].

[†] See "Phil. Trans" 1871, p 511, Plates xxx. to xlii.

relations to *space*, and that the order, as a whole, is, as far as yet known, remarkably restricted as regards geological *time*.

The comparisons instituted in our survey of the frog's anatomy will enable us now to sum up resemblances; first, as regards the orders of its class, and secondly, as regards the class itself.

1. Its own order, *Anoura*, has been seen to present singular resemblances to the *Chelonia* amongst reptiles. Such are the bony plates of the back of some forms, the bony covering of the temporal fossa in others, the mode of inspiration in the adult, the armature of the jaws in the young. On the other hand, the peculiar elongated tarsus has reminded us of certain mammals, and the median Eustachian opening of *Pipa* and *Dactylethra* has suggested an affinity to crocodiles and birds. It has been plain, however, that these several likenesses, however singular and striking, are not evidences of genetic affinity.

2. The order *Urodela* may well recall to mind the *Lacertilia* amongst reptiles, with which animals the *Urodela* were actually classed by Linneus. Moreover in both groups we find a series of different species, longer and longer in body and shorter and shorter in limb. We have also seen that in both these groups an analogous complication obtains in the muscles of the legs.

3. The order *Ophiomorpha*, as has been before observed, present a general resemblance to serpents, and a special resemblance to certain short-tailed ones; though it is rather to the Amphisbenian Saurians that they may most advantageously be compared. Here, again, however, we meet with the resemblances which, though striking, do not allow themselves to be interpreted as indices of any special relationship by descent.

4. The order *Labyrinthodonta* recalls to mind, as has been said earlier, the Crocodilia amongst reptiles, of which they may be deemed as the prophetic precursors, so to speak, though certainly not the direct ancestors.

Thus the class *Batrachia*, as a whole, presents a very interesting analogy and parallelism with the class *Reptilia*. It is a parallelism, moreover, which reminds us of that which exists between the various orders of Placental mammals and the great subdivisions of the pouched or Marsupial order of mammals. We have carnivorous, insectivorous, arboreal, aquatic, herbivorous, marsupial beasts, as we have carnivorous, insectivorous, arboreal, aquatic and herbivorous placental beasts. The harmonious variations of the placental and marsupial groups thus present us with excellent instances of affinities independently evolved and not due to hereditary influence.

In a similar way it seems probable that the subdivisions (orders) of the class Batrachia, mimic, as it were quite independently, the subdivisions (orders) of the class Reptilia.

The Frogs' class, as a whole, shows as many striking affinities to some or other fishes. It does so in the possession of gills and of a branchial apparatus during one time of life at the least; a large parasphenoid in the skull; the often persistently unsegmented terminal part of the notochord; the single ventricular cavity of the heart; the presence of a *bulbus aortæ*; the development of a *nervus lateralis*; the communication between the urinary canal and the oviduct, and certain other characters of less importance.

The class *Batrachia* agrees both with fishes and reptiles in having the blood cold, more than one aortic arch, and (except in crocodiles) in not having the distinct ventricles.

The class agrees with fishes, reptiles, and birds, in having no complete diaphragm, and no corpus callosum* in the brain, and no single aorta arching over the left bronchus.

We have now arrived at the end of those considerations seemingly best suited to enable us to answer the initial question, "What is a Frog?" The requisite definition might, of course, have been given much earlier, but these inquiries have seemed necessary to enable the reader to understand the technical terms of such definition—to give them, in his eyes, a real meaning.

The Frog is a tailless, lung-breathing, branchiate vertebrate, with four limbs typically differentiated, undergoing a complete metamorphosis, and provided with teeth along margins of the upper jaw.

The course of our inquiry into the nature and affinities of the Frog has not alone served to answer the question with which this memoir opened. Incidental bearings upon deep biological problems have come before us more than once in its course, nor have all the conclusions which seem to have forced themselves upon us been totally negative.

Thus we have met with several instances of the independent origin of remarkably similar structures, such as a shielded temporal fossa and elongated tarsus, which, together with structures like the tooth of the Labyrinthodon, seem to be characters for the existence of which neither the destructive agencies of nature acting on minute oscillations of structure, nor any sexual phenomena, will account.

Again, in the life-history of the Frog, considered even purely by itself, we find a remarkable example of spontaneous transformations due to innate powers and tendencies.

When, however, this process is considered in the light derived from the curious phenomena of transformation so enigmatically presented to us by the axolotl, we have very strongly brought before us the powerful action of internal tendencies lying dormant and latent till made manifest, through the advent of conditions so obscure that as yet they have evaded the most careful and anxious scrutiny of practised adepts.

It would seem to be a negligence not here to point out, that if new forms of life—new species—arise from time to time through congenital variation, not a few of the facts herein quoted point to the probability that such forms have arisen through the evolutions of implanted potentialities definite in nature, in other words, by "specific genesis."

Again, a general survey of the different kinds of relations which the Frog has brought before us, is well calculated to impress us with the overwhelming richness and fulness of nature.

Although, from our ignorance, the natural history of many other animals well known to us may appear less replete with interest than that of the common Frog may now be, yet it cannot be doubted but that the progress of science is capable of revealing to us facts as full of instruction and of as profound a significance in the life history of almost any kind of animal whatever.

Ever fresh, ever fertile, natural history offers to our faculties a pursuit practically inexhaustible. We are not, indeed, denied the gratification of successfully exploring and satisfactorily explaining mystery after mystery, but each secret wrested by our efforts brings before us other ever new enigmas, so that though refreshed by success we need never be wearied by monotony. While we need not regard any problem as absolutely hopeless, no dread of coming to the end of our inquiries need ever chill the warmth of our zeal in the scientific cause. Some may consider such reflections justified by the phenomena presented to them by the natural history of the Common Frog.

St. George Mivart

THE HABITS OF BEES AND WASPS*

SIR JOHN LUBBOCK, in a paper on the Social Hymenoptera (Bees, Wasps, and Ants), especially with reference to their habits, senses, and power of communication with one another, pointed out with regard to the latter, that the observations on record scarcely justify the conclusions which have been drawn from them.

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* As to this structure see Lesson in "Elementary Anatomy," pp. 367, 375.

* Being the substance of a paper by Sir John Lubbock, Bart., F.R.S., read before the Linnean Society on the 19th March, 1874.

Thus Messrs. Kirby and Spence say that ants have a language "not confined merely to giving intelligence of the approach or absence of danger, but co-extensive with all their other occasions for communicating their ideas to each other." The observations, however, on which this statement is based, scarcely seemed to him to be conclusive. The two Hubers, indeed, had clearly shown that ants and bees could make one another cognizant of their state of feeling, could communicate anger, danger, &c., but that was very different from the possession of a true language.

In support of the opinion that Ants and Bees possess a true language, it is usually stated that if one bee discovers a store of honey, the others are soon aware of the fact. Thus Huber says, "Wasps are also acquainted with the mode of imparting information to their companions. When a single wasp discovers a stronghold of sugar, honey, or other article of food, it returns to its nest, and brings off, in a short time, a hundred other wasps; but we are yet ignorant, if it be by visible or palpable signs, they are mutually informed of this discovery."*

This, however, does not necessarily imply the possession of any power of describing localities, or anything which could correctly be called a language. If the bees or wasps merely follow their fortunate companions, the matter is simple enough. If, on the contrary, the others are sent, the case would be very different. In order to test this, Sir John proposed to keep honey in a given place for some time, in order to satisfy himself that it would not readily be found by the bees, and then after bringing a bee to the honey, to watch whether it brought others or sent them,—the latter, of course, implying a much higher order of intelligence and power of communication.

In the first place, then, he kept some honey for some days at an open window in his sitting-room, and no bees came to it. He then brought a bee up from his hives in the garden in his hand, choosing one which was in the act of leaving the hive. He found it frightened the bees less to be brought in the hand than in a bottle, probably on account of the darkness. The bee thus brought up was then fed with honey, which it sucked with evident enjoyment for a few minutes, and then flew quietly away. But though it had given no symptom of alarm or annoyance, it did not return, nor did any other bee come to the honey. This experiment he repeated eight times, with a like result. He therefore procured one of Marriott's observatory hives, which he placed in his sitting-room. The bees had free access to the open air, but there was also a small side, or postern door, which could be opened at pleasure, and which led into the room.

This enabled him to feed and mark any particular bees, and he recounted a number of experiments from which it appeared that comparatively few bees found their own way through the postern, while of those which did so, the great majority flew to the window, and scarcely any found the honey for themselves.

Those, on the contrary, which were taken to the honey, passed backwards and forwards between it and the hive, making, on an average, five journeys in the hour. In these cases it is obvious that the bees which had found the honey did not communicate their discovery to the others; and the postern being small and on one side, few of the bees found it out for themselves. If the honey had been in an open place, no doubt the sight of their companions feasting would have attracted other bees, but in this case the honey was rather out of sight, being behind the hive entrance; and was, moreover, only accessible by the narrow and winding exit through the little postern door.

Sir John had, also, in a similar manner, watched a number of marked wasps with very similar results.

No doubt when one wasp has discovered and is visiting a supply of syrup, others are apt to come too, but he believed that they merely follow one another. He argued that if they communicated the fact, considerable numbers would at once make their appearance, but he has never found this to be the case. The frequent and regular visits which his wasps paid to the honey put out for them proves that it was very much to their taste. Yet they did not bring their companions with them. For instance, on September 19, when a marked wasp paid more than forty visits to some honey, only one other specimen came to the honey during the whole day. Both these wasps returned on the 20th, but not one other. The 21st was a hot day, and there were many wasps about the house; his honey was regularly visited by the marked wasps, but during the whole day only five others came to it.

From these and other observations of the same tendency he concludes that even if bees and wasps have the power of informing one another when they discover a store of good food, at any rate they do not habitually do so, and this seemed to him a strong reason for concluding that they are not in the habit of communicating facts. If they do not, he argues, discuss among themselves the incidents of the day, their adventures in search of food, their success and fortunes in hunting, is it not a fair inference that they have no power of doing so?

Without in any way regarding the facts now recorded as sufficient or conclusive, he thought they indicated that their communications were confined to the feelings, and that there was no power of transmitting information as to matters of fact.

When once wasps had made themselves thoroughly acquainted with their way, their movements were most regular. They spent three minutes supplying themselves with honey, and then flew straight to the nest, returning after an interval of about ten minutes, and thus making, like the bees, about five journeys an hour. During September they began in the morning at about 6 o'clock, and later when the mornings began to get cold, and continued to work without intermission till dusk. They made therefore rather more than 50 journeys in the day. In fact they were just as industrious as bees, and kept longer hours, as they began earlier in the morning. He believed that the wasps which seemed to be idling in our rooms had simply lost their way. He gave also a number of observations tending to show the difficulty which bees have in finding their way. For instance, he put a bee into a bell glass 18 inches long with a mouth 6½ inches in diameter, turning the closed end to the window. The bee buzzed about for an hour, when, as there seemed no chance of her getting out, he released her.

Although, as everyone knows, wasps are easily startled and very much on the alert, still they are very courageous.

On one occasion one of his marked wasps had smeared herself with honey and could not fly. When this happened to a bee it was only necessary to carry her to the alighting-board, when she was soon cleaned by her comrades. But he did not know where this wasp's nest was, and could not, therefore, pursue a similar course with her. At first he was afraid she was doomed. He thought, however, that he could wash her, fully expecting, indeed, to terrify her so much that she would not return again. He therefore caught her, put her in a bottle half full of water, and shook her up and down well till the honey was washed off. He then transferred her to a dry bottle and put her in the sun. When she was dry he let her out, and she at once flew to her nest. To his surprise, in thirteen minutes she returned as if nothing had happened and continued her visits to the honey all the afternoon. The next morning she was the first to arrive.

He also had made some experiments on the behaviour of bees introduced into strange hives, which seemed to contradict the ordinary statement that strange bees are always recognised and attacked.

Another point as to which very different opinions have been established is the use of the antennæ. Some entomologists have regarded them as olfactory organs, some as ears; the weight of authority being perhaps in favour of the latter opinion. In experimenting on his wasps and bees Sir John, to his surprise, could obtain no evidence that they heard at all. He tried them with a shrill pipe, with a whistle, with the violin, with all the sounds of which his voice was capable, doing so, moreover, within a few inches of their head, but they continued to feed without the slightest appearance of consciousness.

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* Huber, "Nat. Hist. of Ants," p. 374

Lastly he recounted some observations to show that bees have the power of distinguishing colours. The relations of insects to flowers imply that the former can distinguish colour, but there had been as yet but few direct observations on the point.

THE CAVENDISH LABORATORY

THIS Laboratory, in which every facility is furnished for the prosecution of physical research, is the munificent gift of William Cavendish, Duke of Devonshire, K.G., Chancellor of the University, who has intimated his intention of presenting it complete to the University.

The building, which is now finished, was erected from the designs of W. M. Fawcett, M.A., of Jesus College, at an expense of about 10,000*l.*

The ground-floor contains a set of rooms for operations requiring great steadiness, such as the measurements of length, time, and mass, and of heat, electricity, and magnetism. A store-room, a workshop, and a battery-room are also provided on the ground-floor.

The first floor contains a spacious lecture-room with a preparation-room, a large apparatus-room, a private room for the professor, and a large working laboratory, fitted with tables standing on beams of their own, so as to be independent of the vibrations of the floor. All the tables in the building are supported in the same way, and there are in every floor small trap-doors, by means of which bodies may be suspended over the tables in the room beneath, and through which electric and other communications may be made.

The upper rooms are intended for acoustics, radiant heat, optics, electricity, and the graphic reduction of observations. There is also a dark room for photographic preparations. The air in the electric room will be kept dry by a contrivance due to Mr. Latimer Clark, and the electric machine worked in this room may be made to furnish electricity for experiments in the lecture-room.

In the tower will be erected an iron tube, which may be filled with mercury so as to measure the greater pressures to which gases and vapours are subjected in the heat-room on the ground-floor. There is also an arrangement by which the electric potential of the air at the top of the tower may be measured either in the lecture-room or in the electric-room.

The laboratory is open daily from 10 A.M. till 6 P.M. under the superintendence of the Professor of Experimental Physics, for the use of any member of the University who may desire to acquire a knowledge of experimental methods, or to take part in physical researches.

NOTES

A REUTER'S telegram from Aden, of March 23, states that the steamer *Calcutta* arrived there from Zanzibar on the previous day with the body of the late Dr. Livingstone. We fear this must be regarded as final, and as shutting out any further hope; we can only now do all possible honour to those remains which the doctor's faithful servants have so religiously preserved. A letter recently received from Zanzibar, by Mr. R. A. Laing, states that the body, after having been exposed to the sun for a month to dry, and then packed in a hollowed tree, was wrapped round with cloth, and the natives carrying it supposed it a bale of cloth, or kaniki.

H.M.S. *Challenger* arrived at Melbourne on the 17th inst.: all well. On her voyage from the Cape of Good Hope, she reached the Antarctic Circle between E. long. 70° and 80°.

IN connection with our leading article this week we see with pleasure that Mr. Mundella gave notice in the House of Commons on Monday, that "at an early day he would call attention to the Report of the Science Commissioners on National Museums, and move that, in the opinion of the House, steps should be taken to render National Museums and Galleries of Art more available for instruction for the purposes of Science and Art." We sincerely hope Mr. Mundella's motion will lead to some decided step in advance.

WE are sorry to have to announce the death of Johann Heinrich Maedler, the distinguished German astronomer, at Hanover, on March 14, at the advanced age of eighty. One of his best-known works is a Map of the Moon, of which he was the joint author with M. Beer. He was appointed Professor of Astronomy and Director of the Observatory at Dorpat in Russia about 1840, and was also the writer of various astronomical treatises:—"Popular Astronomy," Berlin, 1849; "The Existence of a Central Sun," Dorpat, 1846; "Lectures on Astronomy," Mittau, 1845–47, &c.

WE are informed that the Royal Belgian Academy has resolved to place the bust of Quetelet in the hall where its meetings are held. We believe no successor to the Directorship of the Brussels Observatory has yet been named.

AT the last meeting of the Royal Irish Academy, the Rev. Prof. Jellett resigned the office of president, and Wm. Stokes, M.D., D.C.L., F.R.S., was elected in his stead. Dr. Sullivan also resigned the secretaryship of the Academy on being made president of the Queen's College, in Cork, and Dr. E. Perceval Wright, F.L.S., was elected to the post. Dr. R. McDonnell, F.R.S., was also elected to the secretaryship of Foreign Correspondence in the place of Sir W. Wilde.

THE Professorship of Astronomy in the University of Dublin, the holder of which is also Astronomer Royal of Ireland, is now vacant by the resignation of Dr. Francis Brunnow. Since its foundation this professorship has been held by Dr. Henry Ussher (1783), Dr. John Brinkley (1790), Sir William Hamilton (1827), and Dr. Brunnow (1865). The election will be held on April 18. Rumour in Trinity College points to Prof. R. Ball, LL.D., F.R.S., as the most likely successor to Brunnow, a distinguished graduate of the University of Dublin in both pure mathematics and experimental physics. Dr. Ball acquired an extensive knowledge of astronomy during the several years that he acted as the late Lord Rosse's assistant at the Observatory at Parsonstown.

AT a numerously attended meeting of the Fellows of the Royal College of Surgeons, Ireland, held in the College Hall, Dublin, on the 13th inst., it was resolved, by a large majority, that it is not expedient for this college to take part in the proposed conjoint scheme for the examination of medical graduates in Ireland. The conjoint scheme had already been approved of by the Council of the College, by the Medical Professors and Board of Trinity College, Dublin, by the King and Queen's College of Physicians, Ireland, and by the Governors of the Apothecaries Hall.

THE circular of the Board of Trade, respecting Storm Warnings, which appeared in our last, appears to require a few additional remarks by way of explanation. The circular speaks of an explanatory pamphlet which is now before us, and we see from it that the intention of the present system of signals is to give an indication of the *direction* of the wind to be apprehended in every case. The drum is never to be used without the cone. Its signification in Admiral FitzRoy's time was "dangerous winds from nearly opposite quarters successively;" and it accordingly gave no indication of direction *by itself.* Experience has shown that there is a much greater degree of certainty in foreseeing the *direction* than the *force* of a coming strong wind. Furthermore an attempt is made to give a degree of numerical definitiveness to the warnings, which at once admits that they are not infallible. The Committee say:—"Hitherto it has been found that at least three out of five signals of approaching storms (force upwards of 8 Beaufort scale, a 'fresh gale,') and four out of five signals of approaching strong winds (force upwards of 6 Beaufort scale, a 'strong breeze') have been fully justified." We may fairly consider this as a step in the direction of treating weather indications by the laws of exact science.

We are glad to learn that Prof. Alluard, of Clermont-Ferrand, has at last succeeded in surmounting the various obstacles which he met with in the establishment of his proposed observatory on the Puy de Dome, at an elevation of about 1,660 metres above the surrounding country (NATURE, vol. vii. p. 481). The chief difficulty arose from the opposition of the peasant proprietors to the invasion of their rights by the construction of a road and erection of the building. M. Alluard announces that the observatory will be ready to be inaugurated in September next, and has invited his meteorological friends to visit Auvergne on that occasion.

A LARGE deposit of Moa bones has lately been discovered in a swamp at Hamilton, in Otago. Besides *Dinornis*, the swamp contains bones of *Aptornis*, *Harpagornis*, &c. The whole have been secured by the curator of the Otago Museum.

The Sedgwick Geological Prize (Cambridge) has been adjudged to J. J. Harris Teale, B.A., St. John's College. The subject for the next prize will be—"The post tertiary deposits of Cambridgeshire and their relation to deposits of the same period in the rest of East Anglia."

THE French Society of Geography has decided upon holding an International Geographical Congress at Paris in 1878. Rules and programmes will be issued shortly.

THERE has recently been concluded in connection with the Liverpool Free Public Library and Museum, a carefully arranged and excellent course of Free Lectures. This is the ninth winter course in connection with the same institution, from which we are glad to infer that these free lectures have been a success. We should like to see similar courses inaugurated in all our large towns; we believe the results would be in the highest degree beneficial. The following is a summary of the Liverpool course:—Eight Lectures on Art, by Mr. W. J. Bishop; Three Lectures on Natural History, by Mr. T. J. Moore; Six Lectures on the Chemistry of Salt, and of the Manufactures depending on it, illustrated with Specimens, Experiments, Diagrams, &c., by Mr. Edward Davies, F.C.S., &c.; Three Lectures on Geology, by Mr. G. H. Morton, F.G.S., F.R.G.S.I.; Two Lectures on Mineralogy and Mining, by Mr. F. P. Marrat, M.L.G.S.; Six Lectures on Navigation and Astronomy, by Mr. J. T. Towson, F.R.G.S.; Two Lectures on Art and Antiquities, and one on Town Window Gardens, by Mr. Charles T. Gatty; Three Lectures on the Constitutional History of England, illustrated with Historical Maps, by Mr. James Birchall; Four Readings, by Mrs. H. J. Gorst.

THE Austrian amateur navigator, Count Wilczek, writing in the *Neue Freie Presse*, says that there is no ground for apprehension as to the fate of the Austrian Polar Expedition which sailed in the *Tegetthoff*, in the year 1872, and that news will probably be received from the expedition in October or November next. Letters for members of the expedition will be despatched by the Austro-Hungarian Government by means of whaling and other vessels bound for the Arctic seas.

MESSRS. TRUBNER and Co. have in the press and will shortly publish a treatise on "Valleys, and their Relation to Fissures, Fractures, and Faults," by G. H. Kinahan, M.R.I.A., F.R.G.S.I. This work will be dedicated by permission to His Grace the Duke of Argyll.

MR. F. C. S. ROPER, F.L.S., has published a "Supplement to the Fauna and Flora of Eastbourne, together with a list of Eastbourne Cretaceous Fossils."

MESSRS. S. W. SILVER & Co. have just published a "Handbook for Australia and New Zealand," containing a large amount of varied and useful information about the various colonies in that quarter of the world. It is accompanied by a "Seasons'-Chart of the World."

THE additions to the Zoological Society's Gardens during the last week include two Palm Squirrels (*Sciurus palmarum*) from Ceylon, presented by Capt. Forster; a Sonnerat's Jungle Fowl (*Gallus sonnerati*) from South India, presented by Mrs White; two Tench (*Tinca vulgaris*) British, presented by Mr. W. Arnold; a Black-eared Marmoset (*Hapale penicillata*) from Brazil, presented by Mr. F. Graham; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Colonel Carington; two Boat-bills (*Cancroma cochlearia*) from South America, deposited.

CELESTIAL CHEMISTRY*

IT now and then happens in the history of the human race upon this planet, that one particular generation witnesses the most stupendous advancement of knowledge, this advancement generally coming from what one might consider an exceeding small germ of thought. You will at once call to mind several such instances. You will recollect how once a Dutchman experimenting with two spectacle-glasses produced the Telescope, and how the field of the known and the knowable has been enlarged by the invention of that wonderful instrument. Again, you recollect how once Sir Isaac Newton was in a garden and saw an apple fall, and how the germ of thought which was started in his mind by that simple incident fructified into the theory of universal gravitation. You will also acknowledge that each step of this kind has more firmly knit the universe together, has welded it into a more and more perfect whole, and has enhanced the marvellous beauty of its structure.

I think that future times will say that either this generation, or perhaps the next, is as favoured a one as that which saw the invention of the telescope or the immortal discovery of Newton: for as by the invention of the telescope the universe was almost infinitely extended; as from Newton's discovery we learned that like forces were acting in like manner everywhere; so in our time does the wonderful instrument called the Spectroscope show us that like matter is acting in like manner everywhere; so that if matter and force be not identical, then these two, namely, matter and force, may be termed the foundation stones of the universe in which we dwell.

My present object is to bring before you as well as I can some first notions which are to be got out of this general examination of all matter beyond our own planet, in its chemical relations; this examination having been rendered possible by the spectroscope.

In the first instance, before I attempt to deal with chemical ideas in relation to the heavenly bodies, I have two things to do. I must first refer to our earthly notions of chemistry, not of course in their generality, for that would be impossible in the time at my disposal, but to that side of them which touches most intimately what I shall have to say by and by; and I must also refer to the results which we have already obtained with regard to the constitution, so to speak, of terrestrial matter, as it is brought before us by the spectroscope.

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* Revised from short-hand notes of a Lecture delivered at the Quebec Institute, on Tuesday, December 16, 1873.

First, then, with regard to chemistry. What is chemistry? It is a science which deals with the matter which surrounds us, and of which the whole planet and we ourselves are built up. We see everywhere around us an enormous number of apparently perfectly distinct things, some of them having vital properties, some of them lifeless, motionless; but out of this apparently infinite diversity chemistry presents us with an almost perfect simplicity. It tells us that everything which exists here is really made up of only sixty-three different things; that the whole of the animal kingdom, the vegetable kingdom, the mineral kingdom—everything—is made up of only sixty-three different substances. That is a wonderful simplification, and science always simplifies.

Now we may look upon those sixty-three elements in two distinct points of view. We may consider them in their physical relations, or we may regard them in a more purely chemical aspect. If we look upon them in relation to their physical conditions, we find that amongst them are fifty-six solids, two liquids, and five gases. If we look upon them chemically, dropping all distinctions between solids, liquids, and gases, we say that some of them are metals, some metalloids; and of some, it may be truly said that it is very difficult to place them exactly—to determine whether they are on the side of the metals or on the side of the metalloids—in the same way as the biologist finds it absolutely impossible to put his finger upon any particular part of the organic world and say, Here the vegetable, or here the animal, kingdom begins. All these chemical distinctions, then, are quite independent of physical conditions. For instance, I shall have to show you that amongst the most metallic of the metals is a gas. Again, among the metals we have a liquid—mercury; so that we have a complete chain of gas, liquid and solid among the metals, although popularly the term metal is often imagined to apply only to such solids as gold, silver, and iron. On the metalloid side, again, we have gases among them the familiar oxygen and nitrogen; we have the liquid bromine, and so on, added to other unmistakable metalloids, such as phosphorus, sulphur, carbon, and iodine, generally thought of in their solid form.

Now what are the chemist's tools by which he has brought about this marvellous simplicity, what the processes by which he carries on his operations? I answer, in the main *vibrations*. He finds the world composed of molecules in millionfold complexities, combinations, and sizes, and he acts upon these molecules by vibrations. For gross molecules he finds in heat most that he wants, but when the molecules are more delicate, then electricity is called in, and electricity does for these what heat did for the others.

Let me here endeavour to make my meaning clear. I want you to assume a long series of vibrations, long at one end of the series and short at the other. We know that heat consists of vibrations, we know that light consists of vibrations. I will also ask you to think of electricity as connected with vibrations, and I ask you further to assume these vibrations to be short. We get heat from the sun, and among these vibrations are some to which our eye is tuned. We get an immense vibration of heat from the oxyhydrogen flame, a flame the heat of which is due to the formation of the gaseous molecules of water, but we get, practically speaking, no light. Many of the electrical phenomena with which we are acquainted take place unseen, and without heat, showing they are not long-wave phenomena; others are exquisitely visible to us, because the vibrations are within our ken; but, to get associated heat, we want pressure, and with pressure we can render the oxyhydrogen flame luminous. In fact—and here let me be perfectly frank with you—I call your attention to the "*as if*"—it is *as if* we have long heat-waves at one end of a long scale, and short electricity-waves at the other, each with different functions, heat giving us with solids and liquids *visible* phemonena, because of added shorter waves, electricity giving us visible phenomena with gases and vapours, because of added longer waves; heat passing invisibly through gases, electricity passing invisibly through solids; heat bringing about chemical changes in solids and liquids, electricity bringing about similar changes in the case of gases.

Now, this being so, let us assume, for the purposes of the present statement, that the mode of motion heat, with its long waves, chiefly affects the larger molecules, that is, compound bodies, and the mode of motion electricity, whatever electricity may be, chiefly affects the smaller molecules, that is, the atoms of simple substances. We shall find, in accordance with this assumption, that if a chemist wishes to reduce the millions of compound molecules in that very compound molecule a piece of ice, he applies heat, and he gets a physical simplification, but not a chemical one, when water is produced; a still further, and exactly similar, stage is reached when this water takes the form of steam, but it is not till an enormous temperature, with its added short vibrations, or electricity, is employed, that the compound molecule breaks up into the simple things oxygen and hydrogen, unless another vibration is superadded of a molecule of another simple thing (or element) which shall aid in shaking them apart.

As instances of the action of heat, I will show you one or two experiments to indicate that in a great deal of chemical action the heat vibration requisite to bring about that simplification by means of which the simple bodies have been determined to exist as such is supplied by the chemical action itself; it is the heat of arrested motion. In other cases we have to supply the heat artificially; but also bear this in mind, that whenever we apply artificial heat the heat is none of our making. It also is the result of a chemical combination. For instance, if I take some potassium and throw it into water, that potassium will instantly burst into flame. You will see that we have a perfectly cool metal put into perfectly cool water, and, as you see, it at once takes fire in consequence of the heat of combination which has been brought about by the attraction between the potassium and the water. And if I had time I could show you that as the result of that heat-vibration thus introduced the water has been simplified, one of its constituent simple things, hydrogen, has been liberated, and I might have collected it in a bell jar.

Another illustration is to be got from a mixture of water and sulphuric acid. I have, in a test-tube, some ether, and I have the water. When I pour the water into a glass you will see that the ether in the test-tube placed in the glass will remain as if nothing had happened. But now I will pour some sulphuric acid into the water, and what happens? We get an attraction between these two things: we get a heat vibration as the result of chemical combination; and, as the result of the heat vibration produced in that manner, the water gets hot and the ether boils, the boiling point of ether being below that of water.

Here is another experiment, and I have chosen these out of many others which might have been brought before you, to show the changes brought about by heat vibrations. Here we have some bichromate of potassium, and on the application of heat it will be instantly reduced. When I say instantly reduced, probably a few seconds will be required in order to allow the heat vibration to act, and you will then have a change of colour in the solution brought about by the application of heat, artificial, so to speak, in this case, although, as I have already cautioned you, the heat of the Bunsen burner which we employ is really an effect of chemical combination.

But not only have we heat with its long waves to bring about chemical action and its result, simplification, but, as I have said, we have another agent, electricity. I have here two tubes filled with water, and a battery, and in each tube connected with this battery is a strip of platinum. The instant that the circuit is made complete you see that the water is decomposed, bubbles rise from the platinum foil, which bubbles in the one case are bubbles of hydrogen, and in the other case bubbles of the other constituent of the water—oxygen. Here you see, by means not of the long waves of heat, but by means of electricity, we bring about a complete dissociation or a complete separation of the elements of the water which originally was in these two tubes. And if we were to allow the experiment to go on a little longer, you would see that not only is there the evolution of gas in each of the tubes, but that the evolution will be greater in one tube than in the other, for this reason, that in the water there are two equivalents of hydrogen to one of oxygen.

These then are instances of simplification brought about by heat and electricity. I quit this part of the subject by the remark that the ultimate particles of an element are called atoms; that agglomerations of atoms are termed molecules; elementary molecules when the atoms are alike; compound molecules when the atoms are dissimilar. The heat-waves generally help us to get at the molecule, and electricity helps us to get at the atom; and mark, I only say *generally*. It might be universally true if all elementary atoms were alike; but on that point we must be content to say that we do not know. But I might place much evidence before you which indicates that they are vastly different. We can only study them by their vibrations;

for, as Sir Wm. Thomson has calculated, the atoms in a drop of water are so small that if the drop of water were magnified to the size of the earth, the atoms would then be seen not larger than cricket-balls or not smaller than shot.

It must be clearly understood that I here refer to the true atom and not to the atom of the chemists, the weight of which they give as the "atomic weight." It may probably turn out that this is often a molecule, sometimes a complicated one, which great heat or electricity can divide, the latter sometimes more than once. It is clear that if this be so, then the vapour densities as referred to the atomic weight will be "anomalous," because the true atom and not the chemist's atom is in question at these high temperatures.

It is now time for us to pass to the action of the spectroscope. The spectroscope, as you know, is the instrument which enables us to deal with either the refraction or the diffraction of light; that is to say, by means of refraction or diffraction we sort out the rays of any beam which we may choose to use into a spectrum, and we then study by means of that spectrum the nature and conditions of the substance which gave us the light.

And there is more than this. Not only can we deal with the giving out of light as light is being given out by this lamp, or that flame, or that gas before me, but we can equally use the absorption of light by various substances, thus studying the nature and conditions of these substances. You know very well that if this lamp, instead of having a shade of ground glass had a red one, the light that would reach your eye would be red. That simply results from the fact that the red glass stops in the main all light but the red, and allows the red to reach your eye. That then is a case of absorption, as the giving out of light by the wick of the lamp is a case of radiation.

What, then, does the spectroscope tell us with regard to the physical differences in matter? It tells us that if we have matter in a solid state, that is matter the molecules of which are large and are near together, agitated by the waves of heat, or by electricity, we get a spectrum from it of a particular kind, called a "continuous spectrum," because the spectrum is absolutely continuous, the red, yellow, orange, green, blue, violet, are all there, as you see them in the rainbow; whereas, if we deal with a gas or vapour not too dense, that is with a substance the atoms or molecules of which are smaller and further apart than in the former case, similarly agitated by electricity or, in some cases, by heat, you find that instead of having what is called a continuous spectrum, you have a spectrum in which the light is not continuous, but broken. The result of this broken condition is that we have light as it were only here and there in the spectrum. We have in fact bright lines representing a few images of the slit, instead of a rainbow band, complete from the red to the violet, representing continuous images of the slit. Thus you see at once enables the spectroscope to tell us the difference between the rare and the dense states of matter quite independently of what that matter may be, and whether we use radiation or absorption as the test; since a substance with a certain molecular arrangement absorbs precisely the same undulations as it gives out with the same molecular arrangement. No matter what it is, the spectroscope at once tells us whether this matter is in a gaseous or vaporous state, in which case we have lines or bands; or in a state in which the molecules are nearer together, when we get a more or less complete continuous spectrum. This at once partly explains why the almost invisible long waves of the oxyhydrogen flame soon fill a mass of the most refractory metal with waves of all lengths, until it shines out almost like the sun. It would appear that molecules or atoms, when once set vibrating by either long or short waves, perform *all* the vibrations proper to them under the conditions present.

How then about the chemical differences? Here the information afforded by the spectroscope is of a much closer character. In the first place it tells us that if you take any substance whatever in a state of gas or vapour, not only do you get bright lines, which tell you that you are dealing with a gas, but you get different bright lines for every substance, so that you not only know that you are dealing with a gas or vapour, but you know at the same time what particular gas or what particular vapour. This is qualitative spectrum analysis, as the effects depend upon the quality of the atoms or molecules present. Further, we see a change in the spectrum from simplicity to complexity, by which I mean that the lines increase in number and broaden, and that the bands become more complete and their channelled structure, where it exists, comes out better, as we pass from a low to a high pressure. This is quantitative spectrum analysis, the change depends upon the quantity of the atoms or molecules present.

Again, the spectroscope at once enables us in the main (and I say in the main, because I have already referred to the borderland between the metals and the metalloids) to differentiate quite as sharply between metals and metalloids as it does between solids and gases.

A metallic spectrum is always a line spectrum when we employ electricity to produce the vapour. Only certain metals give us line spectra at low temperatures: these are mostly monad metals which vaporise easily.

A metalloidal spectrum is only a line-spectrum when we employ electricity. Long heat-waves in their action upon the molecules only produce bands and channelled spaces. Thus the vapour of sulphur has three spectra, two to be obtained by heat, the line spectrum only being obtained by electricity..

Nor is this all. As we can distinguish the spectrum of a metal from the spectrum of a metalloid by the appearance of the spectrum, so also does the spectroscope enable us to see a difference between the spectrum of a compound molecule and an elemental molecule. Let me explain what I mean:—If we are dealing with a metallic element, we get a spectrum of a particular kind so sharply defined that when any one has once seen it, he always knows that an atom of a metal is being dealt with. In the same way when we are dealing with metalloids, the spectrum is generally so entirely distinct from the spectrum of a metal, that when you have once seen the spectrum of a metalloid produced by the long heat-waves, you will always be able to tell it again, there is no possibility of mistaking it for the spectrum of a metal. So far we have been dealing with the elemental molecules, or perchance atoms of metals and metalloids, but we can take a compound molecule. Let us take the combination between metalloids and metals, such as some of the salts of strontium—the chloride of strontium, iodide of strontium, and so on: here we have compound molecules, that is, molecules no longer built up of one substance, but of two; and the long heat-waves, although they can set them vibrating and therefore make them radiate light, do not shake them asunder as high tension electricity does.

We find that the spectroscope is perfectly competent to separate such spectra from all others, so that when we have once seen the spectrum of, say, iodide of strontium, we shall for ever afterwards know that such spectra are given by such a compound molecule as iodide of strontium. The same remark applies to the compound molecules in which oxygen enters as one of the substances. Such spectra closely resemble the spectra of the metalloids, but the bands are farther apart and lie nearer the violet as a rule, so that it is not difficult to distinguish them.

Now when we have to do with a compound molecule, that is to say, with an association of two molecules or atoms of two different chemical substances, we shall at once see that this question of vibrations instantly comes into play; for if the function of vibration, whether we deal with large molecules and long heat-waves, or small molecules and electricity, is to render more simple what in the first instance was compound, then we ought to get spectroscopic differences.

Let us again take the iodide of strontium; the spectroscope is perfectly capable of letting us see the exact effects, not of every degree of temperature which we employ, but of any great differences of temperature. We can follow each increase of temperature by observing the lines or bands which disappear, or which begin to be visible, as the case may be, as the temperature is increased. And similarly, if we have a mixture at a temperature of dissociation, and gradually bring the temperature down until association takes place, then also the spectroscope is just as competent to help us as it was before when we were dealing with an increasing temperature. We find that as the temperature decreases in the latter case, the peculiar compound spectrum to which I have already referred gets more and more visible at the same time as the elemental spectrum gets less and less visible: the order being one of strict law absolutely capable of prediction the moment you know what are the elemental lines, and the lines of any particular compound which longest resists the action of pressure.

Now this is extremely important in its bearing upon the celestial side, so to speak, of this inquiry, and therefore if you will allow me I will still further enlarge what I have said about this distinction between the metals and the metalloids.

If I take sodium vapour at a very low temperature and at the highest temperature that I can get on the earth either by the long heat-waves or by means of electricity, I find that there is absolutely no difference whatever in the molecular arrangement of that sodium vapour at the extreme points. Spectroscopically it is absolutely the same.

Then if I take, not sodium but another element, such as iron, I find it excessively difficult, by means of the heat-waves, to shake asunder the molecules of iron and the diatomic or polyatomic molecules of iron vapour at all. But we know that by electricity non-atomic iron vapour can be got; and then we may say, at all events so far as the lines in the spectrum are concerned (I do not mean their position, but their general nature), that we get a spectrum from the vapour of iron, similar in character to that of the vapour of sodium; but the spectrum has become more complicated as we pass from the monad metal to one with a higher atomicity.

Suppose that, instead of taking a monad metal like a sodium, with its few-lined spectrum, or a metal like iron, with its high atomicity and its many-lined spectrum, we take a *metalloid*; then we find that those conditions no longer hold good. It is not too much to say that in the case of the metalloids every change of even low temperature brings about a change in the spectrum. It is perfectly true, as I have said before, that by means of electricity we can get a line-spectrum from most of the metalloids. But from the ordinary temperature to the electric spark in the case of a metalloid, instead of getting the perfect similarity that we did in the case of sodium vapour, we get an equally perfect and equally beautiful dissimilarity; so that whilst we say that in the case of sodium we only know of but one spectrum, in the case of sulphur, to take one case, we certainly know of four.

You must let me again remind you that when we employ electricity the spectra of the metalloids present exactly the same appearance as the spectra of the metallic elements, such as iron and sodium, and that it is only when we employ heat-waves that those other changes to which I have referred take place.

One word more, too, on the fundamental difference between the spectrum of a metalloid and the spectrum of a metal on the one hand, and the spectrum of a compound on the other. The metalloid has a spectrum of channelled spaces or bands, sometimes to be found in the central part, that is to say, in the green part, or thereabouts, of the spectrum, whereas in the case of the vapour of metals such as iron, and so on, we get bright lines only, not bands; and these lines increase in number generally toward the violet, while in the case of the compound molecules, such as iodide of strontium, to which I referred, we get a something which is half channelled spaces and bands, and half lines, but in all the cases I have examined, excluding oxides, they are limited to the red end of the spectrum.

Let me attempt briefly to summarise what I have stated. With electricity in the case of all elements we obtain line spectra; as we are here dealing with the most complete simplification of matter that we can attain, let us call this the *atomic spectrum*.

With heat we can obtain a continuous spectrum, from solids, liquids, and some vapours; with electricity we can even obtain a similar spectrum from dense gases. Let us call this the *molecular spectrum*.

In the case of many of the metalloids we get, between these extremes, a channelled space spectrum. Let us term this the *sub-atomic spectrum*.

In the case of some compound molecules, we get by heat in some cases, and by electricity in others, a spectrum which is dissimilar from all these. Let us call this the *compound atomic spectrum*.

J. NORMAN LOCKYER

(*To be continued.*)

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 19.—Preliminary Notice of Experiments concerning the Chemical Constitution of Saline Solutions, by Walter Noel Hartley, F.C.S., Demonstrator of Chemistry, King's College, London.

The author has been engaged in investigating the above subject during the last eighteen months, and his experiments being still in progress, he thinks it desirable to place the following observations on record:—

In the examination of the absorption-spectra, as seen in wedge-shaped cells, of the principal salts of cerium, cobalt, copper, chromium, didymium, nickel, palladium, and uranium, to the number of sixty different solutions, it was noticed that the tinctorial properties of the substances could be ascertained by noticing the absorption-curves and bands, so that, provided water be without chemical action, it could be foreseen what change would occur on dilution of a saturated solution.

The Effect of Heat on Absorption-spectra

When saturated solutions of coloured salts are heated to 100° C. there are (1) few cases in which no change is noticed; (2) generally the amount of light transmitted is diminished to a small extent by some of the more refrangible (the less refrangible), or both kinds of rays being obstructed; (3) there is frequently a complete difference in the nature of the transmitted light. Anhydrous salts not decomposed, hydrated compounds not dehydrated at 100° C., and salts which do not change colour on dehydration, give little or no alteration in their spectra when heated.

Solutions of hydrated salts, and most notably those of haloid compounds, do change; and the alteration is, if not identical, similar to that produced by dehydration and the action of dehydrating liquids, such as alcohol, acids, and glycerine, on the salts in crystals or solution.

A particular instance of the action of heat on an aqueous solution is that of cobalt chloride, which gives a different series of dark bands in the red part of the spectrum at different temperatures, ranging between 23° C. and 73° C. Band after band of shadow intercepts the red rays as the temperature rises, till finally nothing but the blue are transmitted. Drawings of six different spectra of this remarkable nature have been made. The changes are most marked between 33° and 53°, when the temperature may be told almost to a degree by noting the appearance of the spectrum. Though to the unaided eye cobalt bromide appears to undergo the same change, yet, as seen with the spectroscope, it is not of so curious a character, the bands being not so numerous.

With cobalt iodide a band of red light is transmitted at low temperatures; this moves towards the opposite end of the spectrum with rise of temperature until it is transferred to such a position that it consists of green rays only. In this instance the change to the eye is more striking when seen without the spectroscope, because the mixtures of red, yellow, and green rays, which are formed during the transition, give rise to very beautiful shades of brown and olive green. Thus a saturated solution at 16° C. was of a brown colour, at - 10° C. it became of a fiery red and crystals separated, at + 10° reddish brown, at 20° the same, at 35° Vandyke brown, 45° a cold brown tint with a tinge of yellowish green, at 55° a decidedly yellowish green in thin layers and yellow brown in thick, 65° greenish brown, thin layers green, 75° olive-green. An examination of this cobalt salt has shown that there are two distinct crystalline hydrates; the one formed at high temperatures has the formula $CoCl_2.2H_2O$, and is of a dark green colour; the other, which contains a much larger proportion of crystalline water, is produced at a low temperature, and its colour is generally brown, in cold weather inclining to red.

The action of heat on solutions of didymium is characterised by a broadening of the black lines seen in the spectrum, more especially of the important band in the yellow; and in the case of potassio-didymium nitrate, this is accompanied by the formation of a new line. In the case of didymium acetate, which decomposes with separation of a basic salt, the lines thickened on heating.

Thermo-chemical experiments

Regnault (Institut, 1864; "Jahresbericht," 1864, p. 99) has shown that on diluting a saturated solution of a salt, as a rule there is an absorption of heat, but in one or two cases he noticed that heat was evolved. The change in colour that takes place on the dilution of saturated solutions of cobalt iodide, cupric chloride, bromide and acetate is very remarkable. There is every likelihood that this phenomenon is due in each case to the formation of a liquid hydrate. It is impossible of belief that accompanying such a circumstance there should be no measurable development of heat; and the author's experiments have proved that in the above cases, at any rate, the heat disengaged is very considerable, amounting, for instance, on the part of cupric chloride, at least to 2,565 "units when 1 gram molecule of the crystalline salt is displaced in its minimum of water at 16° C. and brought into contact with sufficient to make the addition of 40 Aq." These numbers only roughly approximate the truth. On diluting a solution of cobalt iodide till the red colour appears, the thermal effect must be much greater, as not only does it register several degrees on an ordinary thermometer, but it may be perceived by the hand.

The conclusions indicated by these results are obvious, but it is beyond the scope of this paper to refer to them. The writer hopes before long to complete his experiments with the view of having them communicated to the Royal Society.

Spectroscopic Observations of the Sun, by J. Norman Lockyer, F.R.S., and G. M. Seabroke, F.R.A.S.

Note on the Intracellular Development of Blood-corpuscles in Mammalia, by Edward Albert Schafer.

Linnean Society, March 19.—Dr. G. J. Allmann, F.R.S., in the chair.—The following papers were read:—Observations on Bees and Wasps, by Sir John Lubbock, Bart., F.R.S. (for an abstract of which see another column), followed by an interesting discussion in which the president, Mr. Robert Warren, Major-General Strachey, Mr. A. W. Bennett, Prof. Newton, Prof. Thiselton Dyer, Mr. D. Hanbury, Mr. Elliot of New York, and others, took part.—On *Oniscigaster wakefieldi*, a singular insect from New Zealand, belonging to the family Ephemeridæ, with notes on its aquatic conditions, by R. M'Lachlan.

Zoological Society, March 12.—Prof. Newton, F.R.S., in the chair.—The Secretary called the attention of the meeting to an important addition that had been made to the Society's collection on the 7th inst., by the acquisition of a young male Javan rhinoceros (*Rhinoceros sondaicus*) from Batavia, believed to be the first example of this rhinoceros that had ever been brought alive to Europe.—A letter was read from the Rev. S. J. Whitmee, resident at Samoa, stating that he had forwarded, through Dr. G. Bennett, of Sydney, a *Didunculus* and two curlews for the Society's collection, and giving interesting particulars concerning the habits of this bird, and another peculiar Samoan species, *Pareudiastes pacificus*.—An extract was read from a letter addressed to the Secretary by Dr. George Bennett respecting a *Didunculus*, and other birds, he had received from the Rev. Mr. Whitmee, of Samoa, intended for the Society's collection.—Dr. Gunther, F.R.S., gave some details concerning the recent introduction into this country, by Lord Arthur Russell, of the Ide (*Leuciscus melanotus*, var. *orfus*).—Prof. Huxley read a memoir upon the structure of the skull and of the heart of *Menobranchus lateralis*, describing the structure of the bony skull in the osteo-cranium, and giving a full account of the primordial skull or chondrocranium, which has not hitherto been noticed. The chondrocranium was compared with that of *Proteus*, and that of larval frogs and tritons, and its essentially embryonic character was indicated. The chondrocranium was further shown to be formed by the coalescence of three distinct classes of elements which were termed *parachordal*, *pleural*, and *paraneural*. The heart was described, and the septum of the auricles was shown to be an open network allowing of free communication between the right and left auricular chambers. The structure of the *Truncus arteriosus* was compared with that observed in other amphibians.—Mr. R. B. Sharpe communicated the descriptions of two new species of birds recently procured by Mr. H. T. Ansell, of Gaboon; these were proposed to be called *Centropus anselli*, and *Dryoscopus coronatus*.

Chemical Society, March 19.—Prof. Odling, F.R.S., president, in the chair.—On Dissociation, by Prof. Dewar. The lecturer premised that as he had but little that was new to tell, he must content himself with condensing and epitomising the results of others. After briefly referring to the theories of Priestly and Hutton, he described the famous experiments of Sir James Hall, who obtained a substance identical with marble by fusing carbonate of lime under pressure. He next noticed Grove's discovery that water was decomposed at a temperature lower than that produced by the union of oxygen and hydrogen, and then explained the masterly researches of Deville on the effect of heat in causing the dissociation of carbonic anhydride, carbonic oxide, water, &c. After this the lecturer showed that in dissociation the tension of the vapour evolved is constant for a given temperature and independent of the mass, illustrating it by Debray's experiments on the decomposition of carbonate of lime at a regulated heat, and the evolution of water from certain hydrated salts. The lecture, which was illustrated with diagrams of various curves of tension, concluded with some remarks on the dissociation of the compound of hydrogen and palladium, and with a description of an apparatus devised by the speaker for ascertaining the temperature produced by the explosion of a mixture of oxygen and hydrogen under various pressures.

Meteorological Society, March 18.—Dr. R. J. Mann, president, in the chair.—Mr. R. H. Scott, F.R.S., read a paper On an attempt to establish a Relation between the Velocity of the Wind and its Force (Beaufort scale), with some remarks on anemometrical observations in general. The author stated that he considered that the existing scales of wind force were unsatisfactory. The highest pressure corresponding to force 6 of the land scale was 36 lbs. per square foot, whereas pressures of above 40 lbs. had frequently been registered. He further brought forward proofs of the irregularity in the distribution of such high pressures. He then spoke of the Beaufort scale, and pointed out some of its defects, but stated that speaking generally it might be considered to be a rough classification of the wind force, exact enough for practical purposes, and proceeding by nearly equal degrees. He had recently made experiments at Holyhead and at Yarmouth to test the velocity recorded by the anemometer at each station at the hours when the several figures of the Beaufort scale were reported. The result was a scale which agreed very closely with that given by Schott, as a deduction from theory in his discussion of the observations made by Sir F. Leopold M'Clintock in the *Fox*, and published by the Smithsonian Institution. Inasmuch as the accordance of practice with theory was very great, he proposed this scale for general adoption—

Force	Miles per hour.	Force.	Miles per hour.
0	2·5	7	40·5
1	8	8	48·5
2	13	9	56·5
3	18	10	65
4	23	11	75
5	28	12	90
6	33·5		

The paper then went on to point out from experience gained at Holyhead, Yarmouth, and Falmouth, the very serious discrepancies which had been proved to exist in the records of velocity for the various points of the compass, especially at Yarmouth, and which showed that the influence of local situation, not only as to the contour of the country, but even the very shape and height of the observatory and the adjacent buildings, exercised a most serious influence on the correctness of the data afforded by the instruments. It therefore seemed very dangerous to reason as to the mean motion of the air over the British Isles from the anemometrical records of one or two stations, as has been done by Dove.—The next paper read was by Mr. G. J. Symons, On the Sensitiveness of Thermometers, in which he gave the results of a series of comparisons of the speed with which thermometers with bulbs of various sizes took up the true temperature to which they were exposed. Three series of thermometers were used, a set with spherical bulbs filled with mercury, and varying in diameter from a quarter to three-quarters of an inch. The result was that the small bulb took up the true temperature in about three minutes, while the large bulb took three times as long; a second set were similar in form, but filled with spirit; they were more sluggish, but the small spirit ones were more prompt than large mercurial ones. Lastly, the new patterns of spirit minimum thermometers introduced by Mr. Casella and Mr. Hicks were tested and found as sensitive as ordinary mercurial thermometers. The instruments were all examined by the Fellows at the close of the meeting.—The last paper was by Mr. R. Strachan, On the Weather of Thirteen Autumns.

Royal Astronomical Society, March 13.—Prof. Adams, F.R.S., president, in the chair.—On an occultation of Neptune observed at Walthamstow on April 24, by Mr. Talmage. The planet was seen to skirt along the moon's limb, and was only occulted for a few seconds. The occultation was also watched for at Greenwich by Mr. Criswick, and although the difference of latitude only amounts to a few miles, the planet was never lost sight of.—On a remarkable structure visible upon the photographs of the solar eclipse of December 12, 1871, by Mr. Ranyard. In viewing the photographs by transmitted light a minute partially transparent spot can be traced at a height of about $9'$ from the eastern limb on all the negatives of Lord Lindsay's series, and on four out of the six negatives of Col. Tennant's series. It appears to occupy identically the same place with regard to the dark details of the corona in all the photographs, and cannot therefore be due to any reflection within the camera, for the position of the corona is shifted upon the different plates. On first making the discovery, he had been inclined to think that it must be due to a star seen through the corona, but on further reflection he had been obliged to abandon that idea, for a star would have been represented by a dark or opaque point, whereas this must be due to an object darker than the corona, apparently hiding or cutting out some of its light. On a closer examination of the negatives, with suitable lights, three partially transparent circular arcs concentric with the bright point were detected above it. Such forms are totally different from the corona structure visible on other parts of the plate, and there seemed no alternative but to suppose that they were due to some partially opaque body situated between us and the sun, cutting out or partially intercepting the light of the corona. The structure is similar to that which has often been observed in the nuclei and concentric comæ of comets, and Mr. Ranyard thought that it did not seem unreasonable to suppose that this was really a photograph of a faint though large comet near to perihelion. Mr. Christie said that he had examined the negatives and he did not think there could be any doubt about the existence of the structure. It was distinctly to be traced on Lord Lindsay's series, and also on those taken 120 miles away at Ootacamund by Col. Tennant.

Entomological Society, March 16.—Sir Sidney Smith Saunders, president, in the chair.—Mr. Champion exhibited specimens of *Euryporus picipes* taken near Chatham.—Mr. Edward Saunders exhibited a box of *Buprestidæ* collected by Prof. Semper in the Philippine Islands; and read some notes and descriptions of the new species.—A paper was communicated by Prof. Westwood on several additional species of *Lucanidæ* in the collection of Major F. J. Sidney Parry.

Geologists' Association, March 6.—Prof. Morris, F.G.S., vice-president, in the chair.—On the geology of the Nottingham district, by the Rev. A. Irving, F.G.S. The district under consideration comprises coal-measures, Permian, Bunter, Keuper, and Lias rocks—a border-land between the Palæozoic and Mesozoic epochs. No apparent unconformability exists between the Permian and Triassic series here; while that between the Permian and coal-measures is enormous. (1) *Coal Measures.* There are seven seams of coal at present workable in this field, with many more of inferior quality. The enormous unconformability between the coal-measures and the Permian is shown by the fact that at the Shire Oak Colliery near Worksop, 1,300 ft. of coal-measures are passed through before the "top-hard" is reached, whilst at Stretley, twenty miles to the south, the magnesian limestone rests directly (according to Mr. G. Fowler, C.E.) upon the "top-hard" seam. (2) *The Permian.*—The great unconformability between the Permian rocks and the coal-measures is rendered more significant by the absence of the Lower Red Sandstone (Rothliegende), whilst there are clear proofs of continuous deposition of the Permian and Lower Bunter. In this area stratigraphical evidence points to the Permian and Bunter as but portions of one great unbroken sequence of rocks deposited upon highly disturbed and denuded coal-measures. (3) The *Bunter.*—The Lower Mottled Sandstone is nowhere more than 100 ft. thick. The Himlack stone exhibits the junction of the Lower and Middle Bunter. It is marked by unconformability. A bed of calcareous grit and breccia forms the basement of the pebble beds, or Middle Bunter. This is evidently a shore formation. The author concluded, from its composition and from the general prevalence of current bedding, that it occupied an area of deposition subject to shifting currents, but protected from the open ocean. (4) *Keuper.*—Two sections were given where the "water-stones," consisting of alternating beds of sandstone and marls, are seen resting upon the eroded surface of the bunter. In each case the junction is marked by a bed of highly calcareous breccia; and there is unconformability between the two formations. Footprints of *Cheirotherium* have been observed at Castle Donnington, and recently by the author at Colwick, near Nottingham. Ripple marks, &c., are also commonly met with. (5) The *Rhætic beds.*—The black paper shales were discovered by Mr. Etheridge a short time ago at Elton; there also the author has found a portion of the bone-bed. (6) The *Lias* may be observed capping the hills on the south side of the Trent Valley. Belvoir Castle crowns an escarpment of the Middle Lias (marlstone), abounding in *Rhync. tetrahedra* and *Ter. punctata.* (7) *Drift and Alluvium.*—The greater part of the former appears to have been long since carried down into the valley of the Trent, where extensive gravel-pits are worked, as *e.g.* at Saveley and Beeston.

PARIS

Academy of Sciences, March 16.—M. Bertrand in the chair. The following communications were read:—Note on the employment of flexible laminæ for the tracing of arcs with curvature of large diameter, by M. Resal.—Researches on symmetrical isomerism and on the four tartaric acids, by MM. Berthelot and Jungfleisch. The authors have determined the heat of solution of dextro-tartaric acid, lævo-tartaric acid, racemic acid, and inactive tartaric acid. The authors think it probable from their researches that water decomposes the inactive acid into its two active constituents during the act of solution.—On the crystalline hydrates of sulphuric acid, by M. Berthelot; also a thermo-chemical communication.—Experimental researches leading to a determination of the sun's temperature: a letter from P. Secchi to the perpetual secretary. The author has compared the solar radiation with that of the electric arc from a battery of 50 Bunsen's elements, using for this purpose his "thermo-heliometer." After making necessary corrections for atmospheric absorption the result obtained is 133780°, but the author considers this number only an approximation, and considers it possible that it may have to be raised to 169680°.—Report of the geodesic work relating to the new determination of the French meridian, by M. Elie de Beaumont.—Memoir on the swim-bladder, from the point of view of station and locomotion, by M. A. Moreau.—On an application of the theory of substitutions to linear differential equations, by M. C. Jordan.—On the heat of combustion of different varieties of red phosphorus; a note by MM. Troost and Hautefeuille.—On the conditions which determine the movements of chlorophyll granules in the cells of *Elodea canadensis*; note by M. E. Prillieux. By a microscopical examination the author has sought to distinguish clearly in the example chosen the movements which are affected by light from those produced by lesion of the tissues during the act of preparation for microscopic examination.—The blocks and rolled flints in the Red Sandstone of the drift of Saint-Brieuc; note by M. T. Héna. These flints appear to have been brought from Erquy, 24 kilometres to the north-east of Saint-Brieuc by means of floating ice.—On the laws of the plane distribution of pressures in the interior of the isotropic bodies in the state of limited equilibrium; note by M. J. Bossinesq.—On the friction of glaciers and the erosion of valleys, by M. C. Grad. The author expresses his belief that neither the Alpine valleys, the Italian and Swiss lakes, nor the fjords of Norway and Greenland owe their origin to glacial erosion.—Chemical nature of the sulphide of iron (troilite) contained in meteoric irons, by M. S. Meunier. A reiteration of the view, formerly expressed by the author, that this substance is a variety of pyrrhotine (Fe_7S_8) and not simply a ferrous sulphide (FeS).—On a phosphate of cerium containing fluorine, by M. F. Radominski. This mineral contains cerium, lanthanum and didymium, calcium, magnesium, iron, fluorine, phosphoric acid and traces of water. It was found near Fahlun in Sweden. During the meeting M. Gosselin was elected into the section of medicine and surgery to supply the vacancy caused by the death of M. Nelaton.

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THURSDAY, APRIL 2, 1874

MARY SOMERVILLE

Personal Recollections, from Early Life to Old Age, of Mary Somerville, with Selections from her Correspondence. By her daughter, Martha Somerville. (London: John Murray, Albemarle Street, 1873.)

IT would have been a lasting blot upon the biographers of our time if such an illustrious woman as Mary Somerville—a woman unique, or almost unique from one point of view, though so beautifully womanly from others—had been allowed to pass from among us without a satisfactory memorial of her characteristic thoughts, conversation, and domestic life.

The "Personal Recollections of Mary Somerville" will not satisfy those readers who may have hoped to find in the autobiography of the author of the "Mechanism of the Heavens" and the "Connection of the Physical Sciences" any special expositions of Science or practical hints for a successful method of scientific training. The studied care with which Mrs. Somerville avoided bringing scientific questions prominently forward in conversation has been rigidly preserved in the story of her life, where little or nothing is said of the processes by which she attained so exceptional and distinguished a place in the world of Science, and only passing references are made to the extraordinary success that attended her self-acquired knowledge.

As the record of a life in which the fulfilment of all the natural and conventional claims upon a woman's time was combined with practical and theoretical pre-eminence in the most abstruse departments of physical inquiry, no book can, however, be more interesting and suggestive than this volume, in which the personal recollections of Mary Somerville are noted down for us by her own hand and that of her daughter. The story of her life has, moreover, the special interest that it may, with perhaps equal justice, be made to yield arguments for and against the claims advanced for women's equality to men in intellectual capacity. The champions of such pretensions may well point with triumph to her achievements in the higher branches of analytical geometry. Where, indeed, could another instance be found of a person who, after having had to ask, at the age of 16, the meaning of "the *x's* and *y's* mixed with strange lines," which first excited her notice in the pages of a magazine of fashion, should unaided—for she was in all essentials a self-taught mathematician—have been able to begin her career as an author by producing a work like the "Mechanism of the Heavens," which still ranks as the best exposition that we possess of Laplace's "Mécanique celeste"?

The approval which this work won from the first mathematicians and physicists of the day seems to have surprised no one more thoroughly than the writer herself, who had carried on her studies with such unostentatious industry within her own home, that she was scarcely conscious how exceptional were her attainments. And it may be fairly said of her that by the publication of the "Mechanism of the Heavens," in 1831, she suddenly awoke, at the advanced age of 51, to find herself famous, the one woman of her time, and perhaps of all times, for whatever may be the advantages which are now happily being placed within the reach of women for benefiting by high scientific training, we can scarcely expect to meet with many Mary Somervilles. Her genius was unique of its kind, and wholly exceptional, and this fact seems to have been frankly and generously admitted by all who came in contact with her, who were capable of measuring the depths of her knowledge. But so successfully did she conceal her learning under a delicate feminine exterior, a shy manner, and the practical qualities of an efficient mistress of a household, coupled with the graceful, artistic accomplishments of an elegant woman of the world, that ordinary visitors, who had sought her as a prodigy, came away disappointed that she looked and behaved like any other materfamilias, and talked just like other people. No one, therefore, could possibly have afforded a stronger refutation of the axiom, almost universally upheld half a century ago, that scientific acquirements of a high order are wholly incompatible with the proper exercise of the natural and ascribed functions of a woman's destiny. And accordingly the name of Mary Somerville has always been a tower of strength to the promoters of woman's emancipation from the enactments established by man for her exclusion from the enjoyment of the various social, legal, intellectual, and other privileges, of which he has so long had the virtual monopoly.

Her fame did not rest only on her first book—in which she had verified Laplace's own testimony, that she was the only woman who had ever read his works, which, moreover, were not understood by twenty men in France as well as she understood them—for the list of her writings includes, in addition to those more generally known from their semi-popular form as the "Connection of the Physical Sciences," "Physical Geography," &c.; monographs on the Analytical Attraction of Spheroids, the Form and Rotation of the Earth, the Tides of the Ocean and Atmosphere, and, besides many others of equally abstruse nature, a treatise of 246 pages on Curves and Surfaces of the Second and Higher Orders, which she herself tells us she wrote *con amore*, to fill up her morning-hours while spending her winter in Southern Italy. A truly marvellous *catalogue raisonné* of the results of a woman's knowledge and industry!

It is impossible to speak too highly of the sympathy and hearty recognition of the value of her labours that Mrs. Somerville received from all the most eminent of her contemporaries. In France, Laplace, La Croix, Biot Poisson, Arago, Ampère, and many others welcomed her as one of themselves; in England she enjoyed the intimate friendship of the Herschels, Lord Brougham, Professors Whewell, Peacock, Babbage, Sedgwick, and Brewster, and others pre-eminent in science; and surely no greater tribute could have been paid to the exceptional intellectual superiority of Mary Somerville than that rendered by the University of Cambridge when, at the earnest recommendation of Profs. Whewell and Peacock, her "Mechanism of the Heavens" was introduced into the University studies as "essential to those students who aspire to the highest places in the examinations."

It would not be easy to over-estimate the extent and degree in which Mrs. Somerville's acquirements differed from those of women generally at that period; but then it must be admitted that it is precisely through this exceptional character of her attainments that her case may be adduced in proof of the rule that women are not by nature adapted for studies which involve the higher processes of induction and analysis. If such powers as hers had been more generally granted to women, why is she the only woman on record amongst us who has exhibited them?

There was nothing exceptional in her bringing up, or her opportunities. In fact, no woman of her time and station could have had a more typical experience of life than she had. She was born nearly a century ago, in 1780, and spent her childhood and youth in Scotland, within an ordinary circle of the upper middle-class society of her age and country, and therefore very closely circumscribed by lines of defence against innovations and social changes of any kind. Her father, Captain Fairfax (a brave officer who commanded the *Repulse* during the war), received the news of her having taught herself the first six books of Euclid with the remark—"We must put a stop to this, or we shall have Mary in a strait-jacket one of these days. There was 'X.,' who went raving mad about the longitude!" This gallant captain was, moreover, a genuine good Tory, who took decided views in regard to all questions involving a departure from established precedents, and when his young daughter ventured to express her admiration for the short-cut hair, which was then the badge of a Liberal in politics, he exclaimed, "By G—, when a man cuts off his queue the head should go with it." Her mother, who found all her intellectual cravings amply satisfied with the reading of her Bible, a volume of sermons, and a stray copy of a newspaper, fully concurred in her husband's views of the education suited to young women, and was at great pains to thwart her daughter's unladylike taste for pursuits regarded at the time as the exclusive privileges of men, and to keep her mind and hands closely fettered by the bonds of a household possessed of very limited pecuniary means. The parents of the future authoress of the "Connection of the Physical Sciences" did not, therefore, afford her special facilities for mastering any of those higher branches of knowledge for which she seems to have had an instinctive yearning almost before she knew their names. Indeed, at the age of 10, Mary Fairfax was still a little ignorant savage, running wild over the hills and braes of Burntisland, and scarcely knowing her letters; yet before she was 13 she had surreptitiously possessed herself of some of her brother's books and taught herself Latin enough to construe "Cæsar's Commentaries." At that time she scarcely knew the simplest processes of arithmetic, but at the age of 17 the possession of a copy of "Bonnycastle's Algebra," procured for her by her uncle and future father-in-law, Dr. Sutherland—the only one of her relations who did not absolutely oppose her efforts to acquire knowledge—enabled her to solve the mystery of the X's and Y's; and from that hour till the day of her death, mathematics, in one shape or other, may be said to have formed part of her daily existence. For more than half a century they were the staple occupation of her morning hours when the duties of her house and family had been disposed of; at a very advanced age she began and mastered the study of Quaternions, and other forms of modern mathematics, and at 89 she "still retained facility in the calculus."

The restless activity of her intellect had indeed never slumbered. When she received her first lessons in painting and music, she had begun at once to try and trace out the scientific principles on which these arts are based, and never rested till she had gained some knowledge of the laws of perspective and of the theory of colour, and had learnt to tune her own instruments. In later years she may be said to have been always in the van of discovery—not indeed as an originator but as the readiest and aptest of students—and from the time when Young showed her how he conducted the experiments by which he claimed to have discovered the undulatory theory of light, and Wollaston made her one of the very first witnesses of the seven dark lines crossing the solar spectrum, whose detection laid the basis of some of the most wonderful cosmical discoveries of this or any age, Mary Somerville, to the last day of her long life of nearly 92 years, followed with quick and appreciative understanding every step in the advance of modern research. Age could not quench the fire of her intellect, and even in her 92nd year, when the Blue Peter, as she quaintly remarks, had long been flying at her foremast, and she had soon to expect the signal for sailing, she could interest herself in the phenomena of volcanic eruption, speculate on their effects, and follow with lively sympathy the progress of scientific inquiry, and the issues of passing events.

In reading the personal recollections of this wonderful woman nothing strikes one more than the ordinary and even commonplace conditions under which her great intellect advanced to maturity. In her case the only exceptional features were her natural gifts and her perseverance in cultivating them; and this is precisely the point that should not be lost sight of. Mary Somerville will always present a noble instance of what a woman has been capable of achieving, but it would be straining the argument too far to say that we are justified from her special case to draw general conclusions in regard to women's aptitude for the study of the higher forms of physical science.

EXTINCT VERTEBRATE FAUNA OF THE UNITED STATES

Contributions to the Extinct Vertebrate Fauna of the Western Territories of the United States. By Prof. Joseph Leidy. (Government Printing Office, Washington.)

THIS important volume is the first of five which are to form the "Report of the United States Geologica Survey," and it will be supplemented by a memoir, embracing the same subjects, by Prof. Cope.

The large field for palæontological work recently opened up in the Western Territory of the United States has been as fruitful in the introduction of new and unexpected forms of extinct vertebrate life, as that so ably worked out by Cuvier, the Paris basin. By the establishment of a military station at Fort Bridger, opportunities have been afforded to geologists, which the offensive attitude of the Indian tribes had previously deferred, rendering inaccessible a district, the richness of whose past fauna must have been as remarkable as is its present desolation.

Fort Bridger is a military post about 100 miles E.N.E. of Great Salt Lake City, in the south-west corner of the Wyoming Territory. The valley in which it is situated stands nearly 7,000 ft. above the level of the sea at the base of the Uintah Mountains, which form its southern boundary; the Wind River Range defining it on the north-east, and the Wahsatch Mountains on the west separating it from the Great Salt Lake. The enclosed plain is evidently the remains of an extensive fresh-water lake, which in the Eocene period must have abounded with animal life, and whose borders must have been the haunts of animals, both huge and small, which lived and died by its marshy banks. Green River now runs through the plain, and it, with its smaller tributaries, by cutting up the easily eroded deposit, produces a scenery of a most peculiar character, consisting of flat-topped hills and cliffs, with perpendicular sides, and often most grotesque proportions. Those of the water-courses which do not dry up during the summer months are fringed with vegetation, such as cotton-wood, willow and aspen trees, but most of the country is treeless and barren, reminding the spectator more of the ruins of a colossal city, than of any other existing scenery.

The flat-topped hills, table-lands, and scarp-rocks are termed "buttes," and the fossils are generally found at their bases, having fallen there from the gradual atmospheric disintegration of their sides, along with the *débris* of the deposits. The fossils consist mostly of the bones and teeth of vertebrata, together with lacustrine shells. The bones are generally black or brownish, sometimes yellowish; they are generally distorted and much broken, except the small ones, such as those of the carpus and tarsus. They do not withstand the action of the air at all well.

The remains of mammals, which are very abundant, are mostly of genera which are not found elsewhere. Several, however, approach those of the Paris tertiary basin. The odd-toed Ungulata, or Perissodactyla, are particularly numerous, whilst even-toed Ungulate or Artiodactyla are as remarkably few. True Proboscidians are not found, but if Prof. Marsh is correct in placing Dinoceras in an order by itself, animals equally huge, of an independent type, were far from uncommon. Most of the other mammalian orders are most probably represented, though much has yet to be done in the identification of specimens.

Prof. Leidy has not yet seen any remains of bird, but we, some time ago, called attention to Prof. Marsh's discovery of *Odontornis*, a bird with well-developed teeth in both jaws; quite different from *Odontopteryx* of Owen, which has not true teeth, but teeth-like processes of the jaws.

The remains of turtles are most numerous; many of them were aquatic, and some belong to genera which cannot be distinguished from those now existing. What is also particularly interesting to note is that the remains of Crocodilia, which are not very abundant, are all derived from species of true *Crocodilus*, the old-world form, with the lower so-called "canines" fitting into a notch in the upper jaw, and not from *Alligator*, the genus which is now found in the Mississippi and its neighbourhood, with the lower "canine" fitting into a maxillary socket.

From the large amount of material which has passed through his hands, most of which is deposited in the Museum of the Academy of Natural Science of Philadelphia, there are, some types of animals which Prof. Leidy has been able to work out in sufficient detail to make his results of general interest. Perhaps the most complete of these is *Palæosyops*, a perissodactylate Ungulate, of about the size of the Tapir, portions of the bones and nearly complete sets of the teeth of which have been several times discovered. The dental formula was complete, the typical forty-four teeth being present, all close together in the usual numbers, namely i. $\frac{3-3}{3-3}$, c. $\frac{1-1}{1-1}$, p.m. $\frac{4-4}{4-4}$, m. $\frac{3-3}{3-3} = 44$. The canines were peculiarly large, having much the same proportions as in an average carnivorous animal, like the bear. The molars have a resemblance to *Palæotherium*, the inner lobes of the crowns of the upper molars being, however, more completely isolated. There was a third trochanter to the femur, and three toes, as in the Tapir, were present on the hind feet. *Palæosyops paludosus* is the most common species. It is not known whether the neck was long and curved, as that of *Palæotherium* is now found to have been, or whether it was short and straight, as in the Tapirs. *Limnohyus* is a closely allied genus, named by Prof. Marsh.

Another perissodactylate, *Hyrachyus*, closely resembles *Lophiodon* of France, but has an extra premolar in the lower jaw, and a lobe less in the last lower true molar.

Perhaps *Trogosus* is one of the most interesting of the extinct mammals from the "Mauvaises Terres." It is also perissodactylate, and slightly smaller than the common pig. Its dentition would almost lead to the idea that the long-missing form which may be supposed to connect the Ungulata with the Rodentia, has at last been discovered; for with the usual complement of molar teeth there are no canines, and a huge pair of rodent-like incisors, which, in the lower jaw at least, had an intermediate pair of very small teeth. The large incisors had *persistent pulps*, and were formed in part of a circle; they wore down obliquely, in the same way as in the Cavies; were grooved longitudinally, somewhat as in *Aulacodus*, and were covered with enamel on the anterior surfaces only.

It is not to be wondered at, when small fragments of the skull of an animal so unknown and aberrant as *Uintatherium* (or *Dinoceras* of Marsh) were obtained, that each piece should have been referred to a separate genus and species, and Prof. Leidy, in the latter part of his memoir, puts together, as parts of *Uintatherium*, the tusks, horn-cores, &c., as parts of one and the same animal, which he had considered to be portions of different animals in the earlier part of his work, and which he had no reason for associating until Prof. Marsh had described the complete skull of *Dinoceras mirabilis*, which we figured some time ago. As we also mentioned at the time, Prof. Cope has also named this genus *Eobasileus* and *Loxolophodon*.

Besides the above mentioned, most characteristic forms, some from other territory strata west of the Mississippi River, are described—ungulate, rodent, and carnivorous—many of which are intimately related to those of the Paris basin, and throw further light on them. Prof. Leidy also figures and describes several of the Chelonia and other reptiles which come from the same locality.

The above notice of the results arrived at by American men of Science show that they deserve the careful study of English palæontolologists and geologists, as they have already thrown great light on the fauna of the Tertiary period, and give promise of adding much more to our knowledge of that epoch, so important to the student of the anatomy and classification of the higher vertebrata.

OUR BOOK SHELF

The Laboratory Guide, a Manual of Practical Chemistry for Colleges and Schools, specially arranged for Agricultural Students. By Arthur Herbert Church, M.A. (London: Van Voorst, 1874).

TEACHERS of chemistry will be glad to welcome the third edition of Prof. Church's "Guide," to which much new matter has been added. Being specially adapted for students of agricultural chemistry, the book is necessarily somewhat limited in its scope, but the amount of information conveyed within the small compass of 215 pages is very great, and is moreover lucid and accurate. The book is divided into three portions, the first treating of a chemical manipulation, the second of qualitative analysis, and the third of quantitative analysis. The author's preliminary remarks upon manipulation are excellent, and should be graven upon the mind of every chemical student. In the "Introduction" we are told that the student "must never forget that the experiment is the means, not the end. . . . Merely to make a coloured precipitate or a flash of bright flame is not the end of experimenting."

These remarks are much to the purpose, and we commend them to the notice of chemists of older growth, as well as to beginners. The sudden introduction of equations on p. 8 without any previous explanation of the meaning of symbolic formulæ appears somewhat unsystematic, but the student is recommended by Prof. Church to attend some course of lectures on inorganic chemistry, and to study the corresponding chapters in Roscoe's Chemistry, at the same time that he is working through the "Guide." As the "Guide" is at present arranged, the student will find this absolutely necessary. The classification of the metals adopted by the author calls for remark—iron and manganese are classed as dyads and aluminium as a triad. Further on it is explained that this last metal is only a pseudo-triad, being in reality a tetrad. Why not class it with the tetrads at once? Hexad metals and pentad metals are ignored altogether, although manganese forms a hexafluoride, arsenic, and antimony, penta-haloid compounds, &c. We must protest also against the use of the words "vinculant," "vinculance," "univinculant," &c. No advantage is likely to accrue to the science from this new phraseology, and the terms "atomicity," "monatomic," "diatomic," &c., which are in general use, express the idea perfectly. The tables for qualitative analysis differ but little from those generally adopted. The quantitative processes for the analysis of natural products, soils, foods, &c., will be found very useful. In addition to the direct benefit arising from the issue of books like the present, there is an indirect benefit for which we ought to be also indebted to Prof. Church—we refer to the expulsion from the market of hastily compiled and inaccurate works by so-called "Science Teachers," such as it has been our duty to condemn on former occasions.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Prof. Tait and Mr. Spencer

As is shown by the passage from his *Thermodynamics* which he re-quotes, Prof. Tait holds that "Natural philosophy is an experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth."

I hold, on the contrary, that as there are *à priori* mathematical truths, the consciousness of which results, not from our individual experiences, but from the organised and inherited effects of ancestral experiences, received throughout an immeasurable past; so are there *à priori* physical truths, our consciousness of which has a like origin.

I have endeavoured to show that Prof. Tait himself, by saying of physical axioms that the appropriately-cultivated intelligence sees "*at once*" their "*necessary* truth," tacitly classes them with mathematical axioms, of which this self-evidence is also the recognised character. Further, I have contended that the laws of motion are *à priori* truths of this kind; are enunciated by Newton as such; are adopted from him by Prof. Tait; and are not furnished by Prof. Tait with any such experimental proofs as he asserts are needful for the establishment of physical truths. And I have gone on to show that no experimental proofs of them are possible—that every supposed proof, whether derived from terrestrial phenomena or from celestial phenomena, involves a *petitio principii*.

In the course of the discussion I have examined the reason Prof. Tait gives for asserting that the laws of motion are not to be accepted as valid *à priori*. The reason is that "as the properties of matter might have been such as to render a totally different set of laws axiomatic, *these laws must be considered as resting on convictions drawn from observation and experiment, and not on intuitive perception.*"

The worth of this reason I have tested by asking the origin of Prof. Tait's professed knowledge that "the properties of matter might have been" other than they are. Here is the passage:—

"It will suffice if I examine the nature of this proposition that 'the properties of matter *might have been*' other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments. Is it an intuition? If so, then along with doubt of an intuitive belief concerning things *as they are*, there goes confidence in an intuitive belief concerning things *as they are not*. Is it an hypothesis? If so, the implication is that a cognition of which the negation is inconceivable (for an axiom is such) may be discredited by inference from that which is not a cognition at all, but simply a supposition. Does the reviewer [a critic whose attack I was answering] admit that no conclusion can have a validity greater than is possessed by its premises? or will he say that the trustworthiness of cognitions increases in proportion as they are the more inferential? Be his answer what it may, I shall take it as unquestionable that nothing concluded can have a warrant higher than that from which it is concluded, though it may have a lower. Now the elements of the proposition before us are these:—*As* 'the properties of matter might have been such as to render a totally different set of laws axiomatic' [*therefore*] 'these laws [now in force] must be considered as resting . . . not on intuitive perception:' that is, the intuitions in which these laws are recognised, must not be held authoritative. Here the cognition posited as premiss, is that the properties of matter might have been other than they are; and the conclusion is that our intuitions relative to existing properties are uncertain. Hence, if this conclusion is valid, it is valid because the cognition or intuition respecting what might have been, is more trustworthy than the cognition or intuition respecting what is! Scepticism respecting the deliverances of consciousness about things as they are is based upon faith in a deliverance of consciousness about things as they are not!"

From this passage Prof. Tait has quoted a small part which, standing by itself, appears somewhat strange; but which ceases to appear strange when read along with the rest. In seeking the authority which Prof. Tait has for asserting that "the properties of matter might have been" other than they are, I have tried all possible suppositions; and as he professes to have faith only in experimentally-ascertained truths, I have asked whether this is one; by way of showing, unmistakeably, that in the absence of experimental warrant he must admit it to be, if not a mere hypothesis, then an intuition. Whence results the incongruity I have pointed out.

Prof. Tait says this argument of mine reminds him of a student whose conceptions of algebraic processes were shown by asking—"But what if x should turn out after all *not* to be the unknown quantity?" His imagination suggests to Prof. Tait an analogy too remote for me to perceive; and one which I think few will follow him in perceiving. It seems to me that in this case "the unknown quantity" is the application of his story.

I have to add that Prof. Tait's letter gives the erroneous impression that I have made a gratuitous assault upon his views. Contrariwise, I have said respecting them no more than is needful in self-defence. A critic who thought me greatly in need of instruction respecting the nature of proof and the warrants we have for our ultimate scientific beliefs, quoted, for my benefit, the foregoing passage from Prof. Tait; and he did this in a manner implying that when he had told me what Prof. Tait said, there remained for me no alternative but to abandon my position. As I did not coincide in his general estimate of Prof. Tait's *dicta*, and as this particular question is one of some philosophical interest, I thought it worth while to justify my own belief, and, in so doing, was obliged to assail that of Prof. Tait.

In Prof. Tait's desire to avoid controversy I quite sympathise. Though sometimes scarcely avoidable, it entails, as I know too well, a grievous loss of time. But as Prof. Tait decided not to answer, I think it would have been better to keep silence absolutely, rather than to try and dispose of the matter by tearing a sentence from its context, and telling, *à propos* of it, a story not to the point.

Athenæum Club, March 30 HERBERT SPENCER

Herbert Spencer versus Sir I. Newton

PROF. TAIT is not the only one who has to complain of hard treatment in the pamphlet by Mr. Herbert Spencer, referred to in the Professor's letter of last week. As the unlucky author of the obnoxious criticism that gave rise to the pamphlet in question, I of course come in for a lion's share of the abuse; but neither Prof. Tait nor myself are, after all, treated so cruelly as is Newton, who, though his life was spent in maintaining the experimental character of all physical science, is cited as an authority for the *à priori* character of the most important of all physical truths—the well-known Three Laws of Motion.

Mr. Spencer had asserted that these Laws of Motion are *à priori* truths, and had supported this statement by alleging that Newton gave no proof of them, and therefore intended them to be so regarded. After sheltering myself under the authority of Professors Tait and Thomson, I answered that "the whole of the *Principia* was the proof," whereon Mr. Spencer replies as follows:—

"I have first to point out that here, as before, the reviewer escapes by raising a new issue. I did not ask what he thinks about the *Principia* and the proof of the laws of motion by it; nor did I ask whether others, at this day, hold the assertion of these laws to be justified mainly by the evidence the solar system affords. I asked what Newton thought. The reviewer had represented the belief that the second law of motion is knowable *à priori* as too absurd even for me openly to enunciate. I pointed out that since Newton enunciates it openly under the title of an axiom, and offers no proof whatever of it, he did explicitly what I am blamed for doing implicitly. And thereupon I invited the reviewer to say what he thought of Newton. Instead of answering, he gives me his opinion to the effect that the laws of motion are proved true by the truth of the *Principia* deduced from them. Of this hereafter. My present purpose is to show that Newton did not say this, and gave every indication of thinking the contrary. He does not call the laws of motion 'hypotheses;' he calls them 'axioms.' He does not say that he assumes them to be true *provisionally*, and that the warrant for accepting them as actually true will be found in the astronomically-proved truth of the deductions. He lays them down just as mathematical axioms are laid down—posits them as truths to be accepted *à priori*, from which follow consequences which must therefore be accepted. And though the reviewer thinks this an untenable position, I am quite content to range myself with Newton in thinking it a tenable one—if, indeed, I may say so without undervaluing the reviewer's judgment."

To the sneer in the last sentence, and the remark that follows to the effect that the reviewer had evaded an issue "which it was inconvenient for him to meet," I shall reply by recommending Mr. Spencer to dogmatise either less elaborately or less rashly about the views of a philosopher like Sir I. Newton, whose works are so accessible and whose style is so clear, and at once pass on to call his attention to two passages in Newton's letters to Roger Cotes, who was at the time superintending the printing of the *Principia*.

In speaking of the special sense in which he used the word "hypothesis"—a sense which quite justified him in saying of himself "hypotheses non fingo"—Newton says:—

"In experimental philosophy it is not to be taken in so large a sense as to include the first *Principles or Axiomes which I call the laws of Motion. These Principles are deduced from phenomena and made general by Induction, which is the highest evidence that a Proposition can have in this Philosophy.*" (Letter lxxxi., edited by Edleston.)

And in the next letter he says:—

"On Saturday last I wrote to you representing that Experimental philosophy proceeds only upon Phenomena and deduces general Propositions from them only by Induction. And such is the proof of mutual attraction. And the arguments for y^e impenetrability, mobility, and force of all bodies, and *for the laws of motion are no better.*"

I must confess to feeling a difficulty in reconciling the above extracts with the view that Newton posits the laws of motion "as truths to be accepted *à priori*."

THE AUTHOR OF THE ARTICLE IN THE
BRITISH QUARTERLY REVIEW.

An Experiment on the Destructive Effect of Heat upon the Life of Bacteria and their Germs

I RECENTLY carried out an experiment which I shall not soon have the opportunity of repeating, and which I am consequently anxious to put on record. It is probably now familiar to those interested in the matter, that the experiments of Dr. Sanderson have established the fact that in an infusion of turnips and cheese prepared as directed by Dr. Bastian, heating to a temperature of 102 C. is sufficient to prevent the subsequent development of life (Bacteria) in the infusion even when the exposure to that temperature is only maintained for a few minutes. Boiling for five or ten minutes, according to Dr Sanderson, is not sufficient to prevent the subsequent development of Bacteria, but according to the experiments of Dr. Pode and myself, boiling for ten minutes or a quarter of an hour is sufficient, provided that care has been taken to exclude visible lumps of cheese, and when the infusion is enclosed in a tube which tube is submerged in boiling water. Further, Dr. Sanderson has stated the following most important result, namely, that exposure to the boiling temperature (100° C.) was in all cases sufficient to prevent the subsequent development of Bacteria if it was carried on for so long as *one hour*.

This being the case, it occurred to me that since in all probability Bacteria and their germs, or spores, are killed by *through-heating* to a temperature a little below 70° C. (as established by various experiments in regard to Bacteria, but not in regard to possible germs, and admitted by both sides in the controversy as to their biogenetic or abiogenetic origin), it is desirable to recognise in our experiments the two distinct factors of this through-heating to any given temperature—namely, (1) the temperature to which the infusion to be heated is to be exposed; and (2) the length of time during which it is exposed to that temperature. If one of these variables—the time—be taken as a horizontal, and this line be divided into equal spaces representing periods of five minutes—whilst the perpendicular represents the range of temperature divided into degrees from 65° C. to 120° C—and if the results of observations with a given infusion indicating the time of exposure to a particular degree of temperature required in order to prevent the subsequent development of Bacteria be marked off on such a scheme, we should expect to obtain a series of points defining an asymptotic curve, the time required at the highest temperature being infinitely small, and at the lowest temperature infinitely great. This curve would vary in its character according to the properties of the infusion made use of. It was my intention to determine the principal points in this curve for Dr. Bastian's turnip and cheese infusion, but at present I have only made a tentative experiment at a low temperature. Using tubes of quarter-inch bore and three inches in length half filled with Dr. Bastian's infusion, and then submerged in water maintained at the desired temperature, I found that exposure for *six hours* to *a temperature of* 75° C. *was sufficient* to prevent the subsequent development of Bacteria. The same infusion enclosed in a similar tube and not heated at all, teemed with living Bacteria after four days; the same infusion boiled for ten minutes in an open tube remained barren. I submit this plan for a series of experiments to the readers of NATURE, without attaching much importance to the single but definite result which is above recorded.

I have not seen any reference in the pages of NATURE to the experiments which have been carried on in German laboratories in consequence of Prof. Huizinga's advocacy of Abiogenesis. Dr. Paul Samuelson, experimenting with Huizinga's infusions under the direction of Prof. Pflüger of Bonn, has obtained results which negative the inferences of Prof. Huizinga. Dr. Samuelson's paper appeared in Pflüger's *Archiv*, during the past year, and another experimenter (to whom I am unable to refer explicitly) has obtained equally definite results opposed to the speculations of Bastian and Huizinga.

Paris, Feb. 8 E. RAY LANKESTER

Animal Locomotion

I AM surprised to find that the Duke of Argyll prefers a charge of plagiarism against me in 1874 (NATURE, vol. ix. p. 381), said to have been committed by me in a lecture delivered at the Royal Institution of Great Britain in 1867. As his Grace was present at the lecture in question, and lodged no complaint in writing or otherwise, it appears to me that the charge, if not unfounded and out of place, is at least out of time. As I am not conscious of having perpetrated the plagiarism attributed to me, I wish to apprise your readers that the lecture referred to is published *in extenso* in the Proceedings of the Royal Institution of Great Britain, under date March 22, 1867, and may be consulted by all interested in the present discussion. That I had no wish to appropriate from his Grace, but was, on the contrary, desirous of giving him due credit for what he had done, will, I hope, be evident from the following quotation :—"In order to utilise the air as a means of transit, the body in motion, whether it moves in virtue of the life it possesses or because of a force superadded, must be heavier than it. If it were otherwise, if it were rescued from the operation of gravity on the one hand, and bereft of independent movement on the other, it must float about uncontrolled and uncontrollable, as happens in the ordinary gas balloon. The difference here insisted upon was, I have learned since writing the above, likewise pointed out by his Grace the Duke of Argyll, in his very able and eloquent article in *Good Words*, entitled the 'Reign of Law.' . . . This article, I am glad to find, has been reprinted in a separate form with numerous illustrations, and should be read by all interested in the subject of aëronautics." ("On the various Modes of Flight in Relation to Aeronautics;" Proceedings Royal Institution of Great Britain, March 22, 1867.)

The only passage in the lecture bearing upon the point at issue *is opposed* to his Grace's explanation of the direction of the down stroke of the wing and in accordance with that originally given by me and defended by Mr. Wallace* in NATURE, vol. ix. p. 301. *It cannot consequently be regarded as a plagiarism.* The Duke, it will be remembered, contends that the wing of the bird strikes *vertically downwards* during the down stroke. I, on the other hand, believe that the wing, during the down stroke, invariably strikes *downwards and forwards*. In this Mr. Wallace agrees with me. The passage in question runs as follows :—

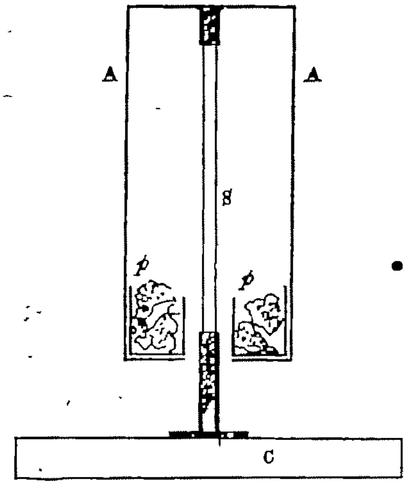
"All wings obtain this leverage by presenting oblique surfaces to the air, the degree of obliquity gradually increasing in a direction from behind *forwards and downwards* during extension, when the sudden or effective stroke is being given, and gradually decreasing in an opposite direction during flexion or when the wing is being more slowly recovered preparatory to making a second stroke. The effective stroke in insects, and this holds true also of birds, is therefore delivered *downwards and forwards*, and not as the majority of writers believe, vertically, or even slightly backwards. This arises from the curious circumstance that birds, when flying, actually fall through the medium which elevates them, their course being indicated by the resultant of two forces, viz., that of gravity pulling vertically downwards, and that of the wing *acting at a given angle in an upward direction*. The wing of the bird acts after the manner of a boy's kite, the only difference being that the kite is *pulled forwards* upon the wind by the string and the hand, whereas in the bird the wing is *pushed forwards* on the wind by the weight of the body and the life residing in the pinion itself." (*Op. cit.*, March 22, 1867.) The Duke, it is true, compares the expanded motionless wings of a bird when sailing to a kite, while I, as stated, attribute a kite-action to the wings both when they rise and fall. The kite-action in the one instance is, however, not to be confounded with the kite-action in the other. That the wings invariably strike *downwards and forwards* during the down stroke, and *upwards and forwards* during the up stroke, and act as kites in either case, is a matter of observation, but still more of experiment. I have again and again witnessed the movement in the crow, cormorant, wild duck, and other birds, and repeated experiments with natural and artificial wings serve more and more to convince me that what I state is correct. But for the downward and forward and upward and forward curves made by the wings during the down and up strokes, progressive flight would be impossible. The curves in question, when the bird is advancing, unite to form *waved tracks* on either side of the body, thus representing the paths pursued by the vibrating wings in every form and variety of flight.

With regard to the poetical quotation introduced by me in my lecture and alluded to by his Grace, I venture to think that few will regard this as a case of plagiarism.

Edinburgh, March 23 J. BELL PETTIGREW

Electric Experiment

THE following striking experiment to show the rapidity of the influence of sulphuric acid in removing the invisible film of moisture that in ordinary circumstances adheres to the surface of glass and deprives it of its quality as an electric insulator, was recently shown to the Natural Philosophy class in the University of Glasgow by Sir William Thomson, and as it may be interesting to some of your readers, I send you an account of it. The apparatus used were a gold-leaf electroscope, and one of the ordinary table insulators long used in this University, of which the following is a description. A A is a hollow cylinder of brass, the lower part of which can be readily detached, replaced, and fixed in position by a bayonet-joint. The cylinder is supported at the top by the glass rod S, which passes through a circular opening in the bottom of the cylinder and is fixed to the sole plate C. In the lower part is placed a circular canal of lead containing a number of pieces of pumice stone *p p*, which for insulating purposes are moistened with a few drops of strong sulphuric acid. On the previous evening the pumice *p p* was moistened with a few drops of water, the cylinder closed and left till morning. The experiment was then performed thus The gold leaves of the electroscope were connected with A A by a fine wire and a charge communicated; the gold leaves at first repelled each other, but almost immediately collapsed. This was repeated once or twice, to show distinctly that there was no insulation.



The pumice containing water was then removed, and was replaced by other pieces moistened with sulphuric acid (in both cases the moistening was so little that the pumice had the appearance of being quite dry) and the vessel was closed. As the experiment was made towards the end of the lecture and time was pressing, a warm hand was placed on the side of the insulator to accelerate the drying process by creating connective currents in the air. Whether this hastened the effect or not sensibly it is impossible to say, but the insulation at once began to improve, and in less than five minutes it was shown to be perfect by the gold leaves remaining diverged to their full extent.

The University, Glasgow, March 21 D. M'FARLANE

Fertilisation of the Fumariaceæ

The accompanying note has been given me by my friend Mr. J. Traherne Moggridge, and I should feel obliged if you would insert it in NATURE with the view of eliciting the communication both of other similar phenomena, and of some explanation of them.

ST. GEORGE MIVART

Mentone, March 18

Note on apparently useless Colouring in the Flowers of a Fumitory (*Fumaria capreolata var. pallidiflora, F. pallidiflora* Jord.)

I observe that in this plant at Mentone the flowers attain their brightest colouring after the ovaries are set, and when fertilisation is no longer necessary, or indeed possible. During the period previous to impregnation, the flowers are pale and nearly white, and the pedicels erect or horizontal; afterwards they become pink, or even crimson, and the pedicels are recurved, and the colour of the petals, which retain their form and position until the ovary has nearly attained its full size, intensifies with the lapse of time.

If the reverse had been the case there is little doubt that we should have regarded the bright colouring as specially adapted to attract insects, and as existing for that purpose, insects being, according to Prof. F. Hildebrand,* important agents in the fertilisation of fumitories; but here, as the brighter flowers are those which no longer need or are capable of profiting by the interference of insects, this explanation ceases to be possible.

This little fact, therefore, would seem to be one which might be classed with those which teach us that, side by side with the developments and modifications which are plainly beneficial to the organism of which they form a part, there are others, which, as far as we can see, are neither useful nor harmful to their possessor, though they may, and frequently do, supply features which especially attract our attention and admiration.

J. TRAHERNE MOGGRIDGE

OCEAN CURRENTS

TWO papers which Mr. Croll has recently published "On the Physical Cause of Ocean Currents" (*Philosophical Magazine* for Feb. and Mar. 1874), bring the main question at issue between him and myself into very distinct view; and as the results of the *Challenger* Temperature-survey of the Atlantic, lately made public by the Admiralty, afford (as it seems to me) important data towards the settlement of this question I shall be glad to be allowed to point out what seem to me their chief bearings upon it.

The position taken by Mr. Croll is, that all the great movements of ocean-water, deep as well as superficial, depend on the action of winds upon its surface. And whilst freely admitting that Polar water finds its way along the floor of the great ocean-basins into the equatorial area, he affirms that this is merely the reflux of the current which has been driven into the Polar basins by the agency of winds.

On the other hand, it is fully recognised by myself, that the *current* movements of *surface*-water are, for the most part, produced by the agency of winds; but these movements, I contend, all belong to a *horizontal circulation, which tends to complete itself*,—a surface indraught being produced wherever a surface outflow is kept up, as we see in the horizontal circulations of the North and South Atlantic, the North and South Pacific, and the Indian Ocean, depicted in Mr. Croll's own map. But I maintain that the *deep* movements of ocean-water are the result of a *vertical circulation*, which is maintained by the continuance of a disturbed equilibrium between the Polar and equatorial columns, occasioned by the surface-action of Polar cold and equatorial heat.

As Mr. Croll is unable to understand why I should speak of Polar cold, rather than equatorial heat, as the *primum mobile* of this vertical circulation, and accuses me of an ignorance of the fundamental principles of physics in so regarding it, I may be allowed first briefly to explain myself; since others may experience the same difficulty, from some want of precision on my part in stating my case. The eminent physicists, however, with whom I have had the advantage of discussing this point, do *not* share Mr. Croll's objection, but hold my statement to be perfectly correct.

Heat applied to the *surface* of any body of *fresh* water, whether by solar radiation, or by the experimental application of a heated plate, will raise the temperature of the *surface-film*, without producing any downward convection. Limited downward convection, however, is occasioned in *salt* water by the sinking of the surface-films which are concentrated by evaporation; but this convection I found in my Mediterranean observations, which have been fully confirmed by those of the *Challenger* in the equatorial area, to be practically limited to the first fifty fathoms. Water in a long trough may thus be superficially heated (as I have experimentally ascertained), by the application of surface-heat to one-sixth of its length, until the temperature of its whole surface-film is raised to 100° or more; but the further application of surface-heat expends itself in vaporisation, and does not communicate itself in any sensible degree to the mass of water beneath, which, therefore, *can not be put in motion* by such application. On the other hand, the moment that *surface-cold* is applied, a downward convection is produced, as Mr. Croll may easily ascertain for himself if he will only try the experiment; and the continued application of such surface-cold to any one portion of the surface will maintain a constant movement through the entire mass of the liquid, until thermal equilibrium is restored by the cooling-down of the whole. But if the restoration of this thermal equilibrium be prevented by the application of heat to another part of the surface, the disturbance of equilibrium will be kept up, and a *vertical circulation* maintained, as long as these two opposing agencies are in operation. If Mr. Croll cannot see that this *must be* the case, I am not responsible for his failure to apprehend that which theory and experiment alike sanction.

I re-affirm, then, that *cold* applied to the *surface* has exactly the same motor power as *heat* applied at the *bottom*; and that its motor agency is more potent than that of heat applied at the surface, simply because the former is diffused by convection through the entire mass of the water, which it keeps on *cooling* and *moving*, whilst the latter is limited to the surface-film, and expends itself in producing evaporation.

Mr. Croll objects to this, that, if it were true, nearly the whole mass of oceanic water must have an almost Polar temperature. I accept this issue; and refer to the *Challenger* temperature-soundings, as justifying it. If he will look at the section taken across the equator, he will find that—*as I had predicted*—Polar water there lies within a very short distance from the surface. At less than 100 fathoms' depth, the temperature falls from 78° at the surface to 55°, and the isotherm of 40° is reached at about 320 fathoms. Below this lies a *stratum of more than 2,000 fathoms' thickness*, whose temperature, ranging downwards from 40° to 32°·4, shows it to consist mainly of Polar water. And as, from the data supplied by the Mediterranean and Gulf of Suez temperature-soundings, a body of equatorial water secluded from all connection with the oceanic circulation might be expected to have the uniform (or isocheimal) temperature of 75° from 50 fathoms downwards, it is clear that the influence of Polar cold here extends itself upwards within 100 fathoms of the surface.

Again, Mr. Croll says that I have made no allowance for the *excess* of salinity in equatorial water, which, according to him, must counterbalance the increase of specific gravity produced in Polar water by the reduction of its temperature. Here, again, he is unfortunate as regards his facts. He appears to have overlooked the observations proving the *lower* salinity of inter-tropical water, which I had cited as furnishing an additional indication that Polar water is constantly rising from the bottom towards the surface in the equatorial area. These observations have been most remarkably confirmed by those taken by the physicists of the *Challenger*. For, whilst in the extra-tropical area the sp. gr. of surface-water was in excess of that of bottom-water, in the equatorial area it was reduced to an almost precise correspondence with it, due allowance for temperature being of course made.

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* "Ueber die Bestaubungsvorrichtungen bei den Fumariaceen," in Pringsheim's "Jahrbucher," vol. vii part 4, p. 423 (1870). Reviewed in "Bull. Soc. Bot. de France," xix. (1872), p. 145.

According to Mr. Croll's doctrine, the whole of that vast mass of water in the North Atlantic, averaging, say, 1,500 fathoms in thickness, and 3,600 miles in breadth, the temperature of which (from 40° downwards) as ascertained by the *Challenger* soundings, clearly shows it to be mainly derived from a Polar source, is nothing else than *the reflux of the Gulf Stream.* Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic Basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf Stream to be about 1-900th that of the North Atlantic underflow; and as it is admitted that a large part of the Gulf Stream returns into the Mid-Atlantic circulation, only a branch of it going on to the north-east; the extreme improbability (may I not say impossibility?) that so vast a mass of water can be put in motion by what is by comparison such a mere rivulet—the north-east motion of which, as a distinct current, has not been traced eastward of 30° W. long.—seems still more obvious.

Lastly, the *Challenger* observations in the South Atlantic have proved exactly what I had anticipated, viz., that the bottom-temperature is lower, and that the Polar underflow lies much nearer the surface in this ocean than in the North Atlantic. Now this case appears to me to afford the *experimentum crucis* between Mr. Croll's doctrine and my own. For my prediction of this result was based on the fact, that, as there is here a perfectly open communication between the Polar and equatorial areas, the vertical circulation would take place more freely. On the other hand, according to Mr. Croll's doctrine, it would have been expected that there should be a far smaller reflux, or no reflux at all. For, though a portion of the equatorial current passes southwards when it meets the coast of South America, there is no ground whatever for believing that it ever goes near the Antarctic circle; and if it did find its way thither, there is no "closed basin" from which it can drive back a return current.

As it is usually considered in scientific inquiry that the verification of a prediction affords cogent evidence of the validity of the hypothesis on which it is based, I venture to submit that so far my case has been made good.

WILLIAM B. CARPENTER

THE DEATH OF DR. LIVINGSTONE

THE daily papers have obtained from the London office of the *New York Herald* the following telegram, containing details of the death of Dr. Livingstone, dated Suez, Sunday, March 29:—

"The *Malwa* arrived off Suez at eleven on Saturday night, having Mr. Arthur Laing and Jacob Wainwright on board, with the body of Livingstone. He had been ill with chronic dysentery for several months past. Although well supplied with stores and medicine, he seems to have had a presentiment that the attack would prove fatal. He rode a donkey, but was subsequently carried, and thus arrived at Muilala, beyond Lake Bemba, in Bisa country, when he said, 'Build me a hut to die in.' The hut was built by his followers, who first made him a bed. He suffered greatly, groaning night and day. On the third day he said, 'I am very cold; put more grass over the hut.' His followers did not speak or go near him. Kitumbo, Chief of Bisa, sent flour and beans, and behaved well to the party. On the fourth day Livingstone became insensible, and died about midnight. Majuahra, his servant, was present. His last entry in diary was on April 27. He spoke much and sadly of home and family. When first seized he told his followers he intended to change everything for ivory, to give to them, and to push on to Ujiji and Zanzibar, and try to reach England. On the day of his death the followers consulted what to do. The Nassick boys determined to preserve the remains. They were afraid to inform the Chief of Livingstone's death. The secretary removed the body to another hut, around which he built a high fence to ensure privacy. They opened the body and removed the internals, which were placed in a tin box and burned inside the fence under a large tree. Jacob Wainwright cut an inscription on the tree as follows:—'Dr. Livingstone died on May 4, 1873,' and superscribed the name of the head man Susa. The body was preserved in salt, and dried in the sun for twelve days. Kitumbo was then informed of the death, and beat drums and fired guns as a token of respect, and allowed the followers to remove the body, which was placed in a coffin formed of bark, then journeyed to Unyanyembe about six months, sending an advanced party with information addressed to Livingstone's son, which met Cameron. The latter sent back a bale of cloth and powder. The body arrived at Unyanyembe ten days after advanced party, and rested there a fortnight. Cameron, Murphy, and Dillon together there. Latter very ill, blind, and mind affected. Committed suicide at Kasakera; buried there. Here Livingstone's remains were put in another bark case, smaller, done up as a bale to deceive natives who objected to the passage of the corpse, which was thus carried to Zanzibar. Livingstone's clothing, papers, and instruments accompany the body. When ill Livingstone prayed much. At Muilala he said, 'I am going home.' Chumah remains at Zanzibar. Webb, American consul at Zanzibar, is on his way home, and has letters handed to him by Murphy from Livingstone for Stanley, which he will deliver personally only. Geographical news follows. After Stanley's departure the Doctor left Unyanyembe, rounded the south end of Lake Tanganyika, and travelled south of Lake Bemba, or Bangneoleo, crossed it south to north, then along east side, returning north through marshes to Muilala. All papers sealed. Address Secretary of State, in charge of Arthur Laing, a British merchant from Zanzibar. Murphy and Cameron remain behind."

These details are few but intensely touching. We believe that the Peninsular and Oriental Company's Bombay steamer *Malwa*, with Dr. Livingstone's body on board, is due on April 13 at Southampton. The body will be landed at that port and conveyed to London, by railway, for interment in Westminster Abbey; it is to be regretted that the faithful Chumah does not accompany his master's remains. It is impossible that Government will fail in doing what the whole civilised world takes for granted it will do—pay all possible honour to the remains of H. M. Consul, and of probably the greatest traveller that this or any other country ever produced.

REPORT OF PROF. PARKER'S HUNTERIAN LECTURES "ON THE STRUCTURE AND DEVELOPMENT OF THE VERTEBRATE SKULL"

THE new Hunterian Professor, Mr. W. Kitchen Parker, has just completed his course of eighteen lectures at the College of Surgeons, embodying in them the results of his researches on that most difficult problem, the development of the vertebrate skull. The plan pursued by Mr. Parker has been to describe first in their adult state, and afterwards in the various stages of their development, the skulls of certain prominent vertebrates which should serve as types for the other members of the group, and to deduce from the facts thus established the principles on which the cranium is constructed in the whole sub-kingdom. The types selected were the shark, skate, salmon, axolotl, frog, snake, fowl, and pig.

I.—*Morphological Elements of the Skull.* Nothing can be more hopeless than the attempt to unravel and explain the vertebrate skull by the study of adult forms only. The modification of face and brain-case, in the long line of creatures which begins with the lamprey and ends with man, are so endless that, until the study of embryology put the matter on a new and firm foundation, the best observers failed signally to produce a true "theory of the skull," the most elaborate attempt of the kind—the "vertebral theory" started by Goethe and Oken in Germany, and perfected by Prof. Owen in England—having resulted only in a convenient working hypothesis.

When, however, instead of starting with the highly differentiated skulls of adult animals, the embryos of these animals from their earliest conditions are made the subject of investigation, a new light is shed on the whole question. It is found that the skulls of all the vertebrata which have yet been thoroughly worked out, originate in, practically, a precisely similar manner; and even in some of the more advanced stages it would be hard to point out very essential differences between the skulls of a fish, a bird, or a mammal. Before entering upon the description of the skull of the shark, the first type to be gone into,* it will be advisable to consider the distinct elements of which the *primordial cranium* of any vertebrate animal is made up.

a. On either side of the anterior termination of the notochord, or primitive axis of the body (Fig. 1, Ch.), is developed a cartilaginous plate (I.M), which as a rule unites both above and below the notochord with its fellow of the opposite side. These plates taken together were termed by Rathke the *investing mass*: in the language of Prof. Huxley, they constitute the *parachordal elements* of the primordial skull or *chondro-cranium*.

b. In front of and below the investing mass, cartilaginous thickenings with intervening spaces are developed in the side walls of the body, enclosing, rib-like, the pharyngeal cavity. These are the *visceral arches* (pleural elements, Huxley), the spaces between them the visceral clefts. The usual number of these bars is eight, although in certain exceptional cases they may be increased to nine (Hexanchus) or ten (Heptanchus). Taken from before backwards, the pleural arches are named as follows:—1. trabecular (Tr), 2. mandibular (Mn), 3. hyoid (Hy), 4-8. branchial (Br. 1—5), the clefts separating them being in like manner, the mouth (M), the tympano-Eustachian passage (Ty Eu) and the branchial clefts (Cl 1—5). At a very early period, the mandibular arch gives off a forward process, the palato-pterygoid arcade (Pl Pt) which in certain cases takes on the form of a distinct pleural element. In the branchiate vertebrata (Fishes and Amphibia) all these arches with the single exception of the trabecular may bear functional gills, the presence of these organs being the chief physiological test of their serial homology.

FIG. 1.

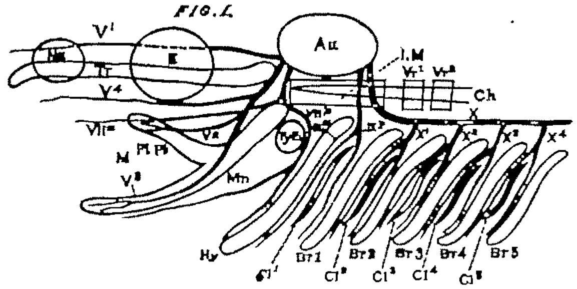

FIG. 1.—DIAGRAM OF VERTEBRATE SKULL. Ch, Notochord; Vr¹, Vr², first and second vertebræ; I M, investing mass; Au, auditory capsule; E, eye; Na, nasal capsule; Tr, trabecular; Mn, mandibular; Hy, hyoid; Br. 1–5, branchial arches; Pl Pt, pterygo-palatine process; M, mouth; Ty Eu, tympano-Eustachian passage; Cl 1–5, branchial clefts; V¹, orbito-nasal; V², maxillary; V³, mandibular; V⁴, palatine division of the trigeminal nerve; VII^a, vidian; VII^p, hyo-mandibular divisions of portiodura; IX, glossopharyngeal; X, vagus.

FIG. 2

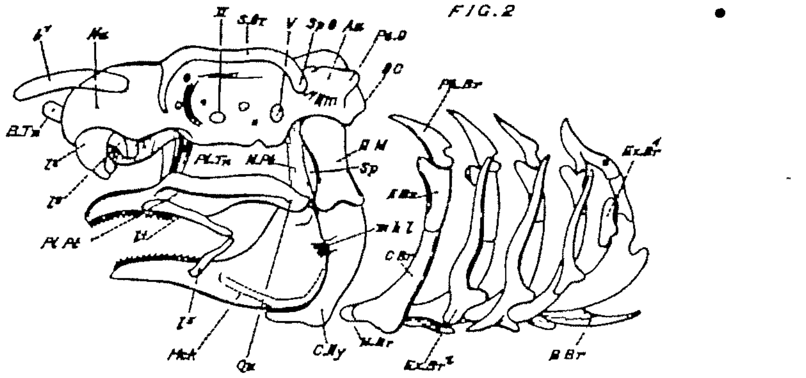

FIG. 2.—SKULL OF SHARK. O.C., occipital condyle; Pt O, pterotic, and Sp O sphenotic process; S Or, supra-orbital ridge; B.Tr, basi-trabecular; l¹, l², l³, l⁴, l⁵, labial cartilages; Pl Tr, palato-trabecular ligament; M Pt, meta-pterygoid; Mck, Meckel's cartilage; H M, hyo-mandibular; C Hy, cerato-hyal: Ph.Br, E.Br, C.Br, H.Br, and B.Br, pharyngo-, epi-, cerato-, hypo-, and basi-branchial; Ex.Br, extra-branchials; Sp, spiracle; II, Optic foramen.

Far more important, however, in the determination of these elements of the skull, are the morphological landmarks afforded by the cranial nerves, especially by the 5th, 7th, 9th, and 10th pairs. It is the constant habit of these nerves to fork above a visceral cleft, one of the branches thus formed supplying the posterior face of the arch in front of the cleft, the other the anterior face of the arch behind and (see Fig. 1). The orbito-nasal or ophthalmic (V¹) and the palatine (V⁴) divisions of the trigeminal are the special trabecular nerves, the former passing over the optic nerve, the latter below it. The posterior primary subdivision of this nerve passes behind the mouth-cleft, and divides into two branches, both of which are distributed to the anterior (or in the altered position of the three foremost arches, outer) side of the mandibular arch; the mandibular, or inferior maxillary nerve (V³) passing along the original direction of the arch, the superior maxillary (V²) following the pterygo-palatine process. The seventh nerve, or portiodura, divides above the tympano-Eustachian passage, its anterior branch (VII^a) going to the inner (posterior) side of the mandibular arch, and its posterior (VII^p) division to the outer side of the hyoid. The glossopharyngeal (IX.), in like manner, is distributed to the inner side of the hyoid and the front face of the first branchial, the hinder face of which, as well as all the remaining branchial arches, is supplied by the great tenth nerve, or vagus (X).

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* The development of the skull in the Marsipobranchii (Lamprey and Hag), the lowest group of craniate Vertebrata, has not yet been studied with sufficient accuracy to allow of the determination of its parts with any degree of certainty.

By Rathke, to whom are due the first accurate observations on the development of the skull, the trabecular arches were looked upon as mere forward processes of the investing mass, and were called *trabeculæ cranii*, or "rafters of the skull." This misconception of their true nature arose from the fact that they very soon coalesce with the investing mass, and are only to be found distinct in extremely early stages.

c. Round the organs of smell and hearing cartilaginous investments are formed, known respectively as the nasal (Na) and auditory (Au) capsules. The latter become, at a very early period, united with the investing mass, while the nasal capsules come into close relations with the anterior or distal ends of the trabeculæ.* These are the *para-neural elements* of the primordial skull.

d. Certain cartilages may be developed in relation with and external to the visceral arches, called from this circumstance "extra-viscerals." Of this nature are the labial cartilages, which take so large a share in the formation of the skull of many cartilaginous fishes.

e. Lastly, the general membranous roof and walls of the brain-case may chondrify to a greater or less extent, but this chondrification is in nearly every case continuous with the trabeculæ and nasal capsules in front and below, and with the investing mass and auditory capsules behind.

Not only is the originally membranous cranium thus strengthened by deposits of cartilage, but osseous deposits may take place either in the primordial skull itself, or in the subcutaneous tissue surrounding it. The latter are called "investing-bones," or *parostoses;* the former may be of two kinds; when occurring as mere calcifications of the substance of the cartilage, they are known as *endostoses,* when having the structure of true bone, as *ectostoses.*

II.—*Skull of the Shark* (*Lesser Spotted Dog-fish*, Scyllium canicula).—The skull of the shark is one of the best examples of the chondro-cranium in its least altered state, being entirely uncomplicated by the development of investing bones, and covered simply by a close-set series of superficial calcifications.

The brain-case is much flattened both above and below. Seen from above, it is greatly excavated in its central portion by the orbits, but expanded in front by the rounded nasal capsules (Fig. 2, Na), and behind by the more or less quadrate auditory capsules (Au). The cartilaginous roof of the skull, or *tegmen cranii*, is interrupted by an oval membranous space, or "fontanelle," situated between the hinder boundaries of the nasal sacs, a position peculiar to the sharks. The upper surface of the otic capsules exhibits three well-marked elevations for the semi-circular canals, and just within that for the anterior canal, a small rounded aperture, the remains of the primitive involution of the integument from which the organ of hearing arose. An elevation on the hinder end of the posterior canal marks the position of the epiotic ossification so well developed in the osseous fish; the pterotic is also indicated by a large outstanding process (Pt O) which forms the postero-external angle of the skull, and the sphenotic (post-frontal of Cuvier) by the posterior portion of the supra-orbital ridge (S Or) when it coalesces with the auditory capsule (Sp O). The anterior extremity of this ridge forms in like manner the pre-frontal process. One very noteworthy point, observable both in an upper, under, or side view, is the presence between the nasal capsules of a short rod of cartilage (B, Tr) representing the median basal portion, or keystone of the trabecular arch, and hence called the basi-trabecular.

Viewed from behind, the skull presents a large foramen magnum bounded below and at the sides by the well-developed occipital condyles, between which is a slight elevation, showing the point where the notochord originally entered the investing mass. External to the occipital foramen, and marking the original boundary between the parachordal and otic elements of the skull is the foramen for the exit of the 9th and 10th nerves. The trigeminal foramen, which always points to the anterior limit of the otic region, forms a large aperture in the side wall of the brain-case (V), as also does the optic foramen (II).

The jaws are very loosely united to the other parts of the skull, and consist of an upper and a lower dentigerous arch, the former of which is connected with the skull by two bands of ligamentous fibres. The lower arch (Mck), which articulates with the posterior end of the upper, is the homologue of Meckel's cartilage, the rod which forms the foundation of the lower jaw in all vertebrata, but which as a rule, owing to the great development of investing bases, is reduced to a more slender style, or is even suppressed altogether. The posterior portion of the upper dentigerous arch (Qu) answers to the quadrate, a bone which in all Teleostei, as well as in Amphibia and Sauropsida, gives attachment to the mandible. The remainder of this "upper-jaw" represents the series of bones (pterygoid, meso-pterygoid, and palatine) which in the osseous fish connect the quadrate with the fore-part of the skull, the meta-pterygoid or proximal end of the mandibular arch being represented by the band of fibrous tissue (M Pt) which connects the quadrate with the auditory capsule.

Close behind the attachment of the meta-pterygoid ligament, a large phalangiform cartilage (H M) is articulated to the auditory capsule; this represents the hyomandibular of the osseous fish, the largest bone in the suspensory apparatus of the lower jaw, and the uppermost portion of the hyoid arch. Between this cartilage and the meta-pterygoid is a space (Sp), which in the recent state forms a communication between the cavity of the mouth and the exterior. This is called the spiracle, and answers to the tympano-Eustachian passage of the higher vertebrata. The distal portion of the hyoid arch consists of a large and strong cartilage, the cerato-hyal (C Hy), below which is a basal piece, common to both sides (shown in the figure by dotted lines), the basi-hyal. This is an extremely simple mode of segmentation of the hyoid arch, and approaches nearly to the primitive condition.

The cerato-hyal is connected with the mandible by a ligament—the mandibulo-hyoid ligament (mhl).

There are five branchial arches, all of which are split up into four segments, called, after the names originally given by Prof. Owen to the corresponding parts in the Teleostei, pharyngo-epi-, cerato-, and hypo-branchial. The inferior median piece, or basi-branchial (B Br), occurs only in the hinder part of the series.

The extra-viscerals are represented by the labial cartilages (l^1—l^5) and by the extra-branchials (Ex Br 1—5), between which and the branchial arches extend cartilaginous rods, acting as supports to the septa between the gill-pouches. The last arch, however, bears no gill and has no extra-branchial corresponding to it. The hyoid also is devoid of an extra-visceral, although it bears a series of greatly divided cartilages, which support the anterior wall of the first gill-sac; this arch, consequently, carries a half-gill. The branchia of the mandibular arch is represented by a vascular plexus (pseudo-branchia) on the anterior side of the spiracular opening.

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* The Sclerotic, the fibrous (mammalia), or cartilaginous (Sauropsida and Ichthyopsida) capsule developed around the organ of sight (E) never really forms part of the skull, although in the sharks and rays, and some osseous fish, it is articulated with the side walls of the brain case by a cartilaginous pedicle. The form of the skull is, however, greatly governed by the presence of these optic capsules.

NOTES

AN International Horticultural Show is to be held at Florence in May, from the 11th to the 25th. The *Société Royale Toscane d'Horticulture* offers 100 medals of gold, 221 of silver, and 131 of bronze, and five grand *prix d'honneur* are offered respectively by the King of Italy, the Minister of Agriculture and Commerce, the province of Florence, the town of Florence, and the lady patronesses. Prince Demidoff and Prof. Parlatore have also placed gold medals for special classes, at the disposal of the committee. Coincidently with the Show the International Botanical Congress will be held at Florence under the presidency of Prof. Parlatore. The programme of subjects for discussion includes questions on the nature and functions of hairs on plants, on cell circulation, on the latex, on the automic movement of the leaves of plants, on the causes which determine the direction of the root in the germination of a seed, on the causes which influence the direction of the growth of branches, especially of weeping trees, on the analysis of the organs of reproduction between cryptogams and phanerogams, as well as many other subjects more widely known, and subjects of debate such as the origin of *Bacteria*, the determination of fossil plants by their leaves, the distinction between *species*, *race*, and *variety*, and the origin of insular and alpine floras. The President and Secretaries of the *Société Royale Toscane d'Horticulture* announce their readiness to communicate with any botanists who wish for further information with a view to attending the Congress. The official language of the Congress will be Italian, but papers may be communicated and discussions conducted in any language. Representatives to the Congress have been appointed from the various countries of Europe, and from Egypt, Australia, Mexico, Brazil, &c. Among the names of those who are expected are announced the following English botanists :—Messrs. Hooker, Trimen, Ball, Hiern, Hogg, Maw, Murray, Allmann, and Binney. As a measure of precaution against the introduction of the *Phylloxera*, the importation of vines and of other fruit-trees into Italy has been rigorously prohibited since October 31 last.

THE Syndics of the Cambridge Botanic garden in their annual report state that the Curator has nearly completed the re-arrangement of the herbaceous plants, and it is hoped the laborious task will be finished in the ensuing year. The plant houses are in a good state of repair, but over-crowded. The Professor and Curator are unable to see in what manner the number of plants kept in them can be materially reduced without injuring the efficiency of the garden. Several of the finest and most valuable specimen plants now threaten to grow through the roof of the houses. The Syndics acknowledge some donations of foreign seeds and plants, but they are under the necessity of discouraging gifts of seeds of plants belonging to warmer regions, because of the want of room for their proper cultivation.

WE are very glad to hear that negotiations are pending for the transfer of the valuable Museum of Natural History, which was formerly in the possession of the East India Company, from the India House, where it has been for some time stored, to South Kensington, where it will at last be available for reference and study. This desirable transfer we strongly recommended in an article which appeared about a year ago (NATURE, vol. vii. p. 457).

A PURSE of 540 guineas has recently been presented by members of the British Association and other friends to Mr. W. Pengelly, F.R.S., F.G.S., as a testimony to the high value of his labours in conducting the exploration of Kent's Cavern, Torquay, and of his other services to science. After the presentation it appeared that many of Mr. Pengelly's friends and advisers had been left in ignorance of what was proposed. To enable all such persons to join in this mark of appreciation the hon. sec. to the testimonial fund, Mr. J. E. Lee, F.G.S., Villa Syracusa, Torquay, is prepared still to receive subscriptions up to the 17th of April.

A GERMAN Natural History and Anthropological Society for Eastern Asia has now existed for a twelvemonth, having been established on March 22, 1873. The headquarters is at Tokio, and the Society consists of fifty-two members, twenty-three being resident at Yokohama, twenty at Tokio, seven in Hiogo, and two at Singapore. Herr von Brandt, the Minister for the German Empire in Japan, is its president. The Society has already published a volume of "Proceedings," containing several interesting and important papers on the subjects for the cultivation of which the Society was founded, especially on the ancient customs and history of Japan.

A MONUMENT to Antonio Bertolini, author of "Flora Italica," has been inaugurated at Bologna.

DR. ASCHERSON, of Berlin, has gone to Egypt as a member of a commission of exploration. Prof. Planchon, of Montpellier, has been sent by the French Government to the United States to inquire into the new vine disease caused by the *Pemphigus vitifolia*. Other botanists at present occupied with foreign exploration are, Sig. Pichler in the east, and Messrs. Lorentz and Hieronymus.

THE chairs of Botany at the Universities of Genoa and of Modena were announced vacant at the end of January.

THE new edition of Pritzel's "Thesaurus" will be edited by Prof. Jessen.

PROF. STEFANO DE' ROSSI has just started in Rome a *Bullettino del Vulcani*. The learned geologist has undertaken to chronicle and to comment upon all the volcanic phenomena which are observed in Italy and the surrounding islands. Two parts of the periodical have been published, giving details of every commotion felt during 1873. The ground was in such activity that Prof. Stefano de' Rossi has been able to report more than three hundred separate phenomena. The mean number of seismic commotions in the whole Peninsula is almost *one daily*.

AN aeronautical experiment of great importance took place on March 22. The balloon "Etoile Polaire" was sent up with two aeronauts, M. Sivel and Croce-Spinelli, to test if the respiration of an air rich in oxygen would enable observers to reach a high level without being suffocated by the rarity of the surrounding medium. The experiment was suggested by M. W. de Fonvielle in his "Science en Ballon," and an apparatus was constructed by M. Paul Bert, Professor at the Sorbonne, and a Member of the National Assembly. The "Etoile Polaire" started at 11.40 A.M. from the La Villette gasworks, and at 12.4 P.M. had reached 5,000 metres. The temperature, which was + 13° C. on the ground, had sunk to – 10° C. M. Croce-Spinelli was almost suffocated, but by using the oxygenised-air respirator he recovered. His pulse, which was beating 86 on the ground, was beating 140, and with the respirator 120 only. These experiments were conducted from 12.4 to 1.30 P.M., when the balloon had reached 7,400 metres, where the thermometer sunk to – 24°. No observation was taken during the descent, which took place at 2.12 P.M. at Bar-sur-Seine, 120 miles from Paris. On landing, the temperature was + 17°. M. Croce-Spinelli had with him a little hand spectroscope supplied by M. Janssen. He states that all the aqueous lines belonging to the vapour had disappeared, and that the solar rays *D* and *F* were growing very dark. When not using the respirator the sky seemed quite dark, but the blue colour was restored when respiring oxygenised air. The measurement of the balloon was 2,800 cubic metres. It was elevated with 1,650 metres of lighting gas, carrying with it 380 ballast, of which no more than 40 were left for descending from 7,400 metres. The altitude is higher than any French aëronaut has reached up to the present time, but Mr. Glaisher, without the help of any oxygenised air, navigated the atmosphere on several occasions to that distance from the sea-level—as on August 28, 1862, April 18, 1863, June 26, 1863, &c. He had no feeling of suffocation except when he reached 10,000 metres on September 5, 1862. It is to be supposed that with proper care and with persons properly trained and selected, the method, although efficacious, is not required except at higher level or for the purpose of increasing the comforts of aërial travellers. It is very probable that it is useless to keep several mixtures and that pure oxygen is better as being more efficacious and less bulky.

THIS year's International Exhibition at South Kensington opens next Monday.

AT the annual general meeting of the Royal College of Physicians of London, held on March 30, Sir George Burrows, F.R.S., was re-elected President of the College.

ASTRONOMERS will be interested to learn that among the numerous able men whom the President of the republic of Ecuador has gathered to that city in order to develop the University of Quito, there has appeared one, Father Menten, whose interest in astronomy has been such as promises to settle the long-mooted question as to an observatory in that city. Menten has now returned to Quito laden with a portion of the instrumental outfit that he was ordered to secure at Munich. Among the apparatus is a six-inch meridian circle. Father Menten was for some time a pupil of the eminent Argelander.

THE Royal Academy of Belgium has announced the following subject for its prize essay for 1875:—The relation of heat to the development of phanerogamic vegetation, especially with respect to periodic phenomena, and the value of the dynamic influence of solar heat on the evolution of plants.

IT has been proposed to hold a fungus show in Scotland; a preliminary meeting is to be held in Marischal College, Aberdeen, on April 14.

MR. J. C. MELLIS, late Commissioner of Crown Property at St. Helena, announces as in the press a work on the geology, fauna, flora, and meteorology of St. Helena. Separated so far as the island is from any mainland, and showing no trace of any former connection with a continent, there are many questions of interest as to the origin of its flora and fauna. This work is stated to be based on Mr. Mellis's own observations, and promises to be of great value.

MR. WATSON has brought to England a series of paintings by a Japanese artist illustrating some of the customs of the Ainos, who inhabit the island of Yezo, or Sesso as it is sometimes spelt, north of Niphon. Not much is known of the Ainos, but they are regarded as the aborigines of Yezo, who have been driven inland by the fringe of Japanese settlements all round the coast. The first of the series is said to represent the traditional origin of the race. A woman is in a cave weeping, and a dog is carrying to her a red flower, apparently a rose. It does not, however, appear to be known what this symbolises. A coloured drawing of a male Aino executed with great care and regard to detail, shows bows and arrows of native make and a sword of Japanese manufacture. A cord is worn round the head to help to form a support for a weight, as they never carry burdens on their backs. The portrait of a female shows the broad tattooing round the upper and lower lip and on the arms, as the sign of marriage. Both the male and female wear earrings, which is a contrast to the Japanese, who never wear personal ornaments. The woman is playing on a kind of guitar with five strings. In the views of the interior of the dwellings all cooking utensils have their Japanese names written against them by the artist, so that their use may be known though their shape is not familiar. Some of the series illustrate whale harpooning, which is done with a two-pronged harpoon, and many are devoted to the festival of the bear. After killing a bear they appear to sit round it in state solemnity, as if worshipping it and offering it food and drink. Dresses of many colours are worn, and the bear is decorated. Their commerce is represented as the collecting of seaweed, drying it, packing it in bundles and selling these by weight to Japanese. The drawings of the articles used for food, laminaria, crabs, holothuria, cuttle-fish, &c., are drawn with great detail and delicacy of colouring.

THE first volume of "Repertorium annuum Literaturæ Botanicæ Periodicæ," has appeared, published at Harlem under the editorship of Van Bemmalen. It professes to give an account, for the year, of all botanical papers read in full or in abstract before Societies, and also to notice memoirs and communications to Societies.

DR. L. JUST, Professor of Agricultural Chemistry in the Polytechnic School at Carlsruhe, has just published, with the co-operation of several distinguished men of science, an Annual of Botany, intended to form a complete record of all botanical works published during the year.

THE Bureau des Longitudes is no longer connected with the National Observatory of Paris. It is to have its own budget, library, and a public building will be arranged for its private use. The most influential member and president is M. Faye, who is making a public appeal to French astronomers asking them to devote themselves to the spectroscopic observations of the sun.

MR. W. S. CLARK, president of the Massachusetts Agricultural College, publishes a lecture On the Circulation of Sap in Plants, containing a great amount of information as to our knowledge of the subject, and a number of tables and diagrams to illustrate the maximum and minimum pressures of the sap in different trees. Mr. Clark maintains, in opposition to some recent statements, that there is a flow of crude sap upwards in the wood, and a flow of organisable material essential to the life of the plant proceeding from the leaf to the root through the bark and cambium layer, from which the growth of the season is formed.

THE annual *réunion* of the Sociétés Savantes des Départements of France, will take place at the Sorbonne from April 8 to 11.

ONCE every year French astronomers hold a general meeting at the office of the Minister for Public Instruction. It will take place this year in the beginning of April.

FRENCH officers belonging to the general staff are regularly attending the observatory to be trained in astronomy. A special building has been constructed for their use by M. Leverrier.

DR. HYRTL, the eminent Professor of Anatomy in the University of Vienna, delivered his final lecture on March 16, and took leave of his class. In the afternoon he met an assemblage of more than three thousand pupils and friends, who presented him with addresses and other expressions of esteem, which were feelingly acknowledged by him.

THE additions to the Zoological Society's Gardens during the past week include two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Mr. J. Hayward; a White-necked Crow (*Corvus scapulatus*) from the Gold Coast, West Africa, presented by Capt. E. Whitehead, 42nd Highlanders; an Eland (*Oreas canna*), born in the Gardens; two Yellow-headed Conures (*Conurus jendaya*) from south-east Brazil, purchased.

CELESTIAL CHEMISTRY*

II.

WE have now gone through as briefly as I can manage to do it, the principal points in chemistry and the principal points in spectrum analysis with which we have to deal when we are dealing with the chemical substances of our own planet. You see the point of inquiry is the chemical and spectroscopic study of the differences between atoms and molecules. We have now to apply these two lines of thought to a consideration of what I have called Celestial Chemistry.

In the first place, how is the study of Celestial Chemistry carried on? To answer this question I will give you an idea of the way in which the spectroscope is added to the telescope. We, of course, require a spectroscope in order to bring into play refraction or diffraction, and we equally, of course, require a telescope in order to collect the light which comes to us from the various light sources which are to be found in the sky. You now see a photograph of what is technically called the eye-piece end of the most perfect and most powerful telescope in the world, which I am proud to say is in England. It is the telescope belonging to Mr. Newall of Gateshead. We see the principal telescope and the various finders, which enable the observer to find the most delicate object which shines in the sky.

You will get an idea of the enormous quantity of light collected by this instrument, when I tell you that the diameter of the pupil of the eye is less than a quarter of an inch, and that the diameter of the object-glass of this telescope is 25 inches, and that the quantity of light utilised depends upon the squares of the diameters. Of course, when we have to deal with feeble celestial objects lying at an immense distance from us, and which give but little light, it would be absolutely impossible to use the prism with any effect unless in the first instance we thus collected a large amount of light to work with.

Now then, having our spectroscope attached to our telescope in the way indicated, to what must we turn our attention? You will see in a moment that it is useless to consider such a body as the moon. I say useless, of course bearing in mind that we are here dealing with chemical considerations, because the light which we get from the moon is simply sunshine second hand, and the moon has no atmosphere. The same remark applies to a certain extent when we deal with the planets, because they also, as you know, are lit up like the moon by the sun, but they have atmospheres, and much is to be learned from them in this way. What we have to do in the main to get a just general outline, in order to study this Celestial Chemistry is, to confine our attention to those bodies which shine by their own light. And if you think the matter over for a minute you will see that there are two distinct classes of such bodies. In the first place there are the *Nebulæ*, and *Comets*, and *Meteors*, which shine by their own light. And again, there are the *sun* and the *fixed stars*, which shine also by their own light; but with this important difference, that while we get the initial light radiated by a nebula, a comet, or meteor, the light which we get from a star is not the initial light, but a difference light; a great deal of the light radiated having been stopped by the atmosphere of the star. So that in the case of a sun, by which of course I mean a star—our sun being merely the star which is nearest to us—although it shines by its own light, we get a difference light while we get what we may call the total light from the first class.

And here let me add that it is the chemical composition of the atmosphere of the star which thus stops the light which we can study. If the stars had no atmospheres there could be no star chemistry, because their spectra would be continuous, and in that case neither qualitative nor quantitative spectrum analysis would be possible.

We deal then with radiation in objects of the first class and with absorption in objects of the second class.

Let us commence our study of those objects which shine by their own light by the study of the comets and the meteors. It is unfortunate that since the more general use of the spectroscope, no large comet has made its appearance, but still some small ones have been observed.

There is a spectrum which in the main is common to a great many of the compounds of carbon; and here I may parenthetically remark that carbon is, of the sixty-three elements to which I have referred, the only element which with the electric power that we can employ in the arc presents us with the distinct appearance of a compound spectrum. Side by side with this generic spectrum of the compounds of carbon I will show you the spectrum of the head of a comet which was observed some years ago; and I may add that several other comets have been observed since, with the indication that their spectra are to a very large extent, if not absolutely, the same; so that we may say there is a probability that, in the case of some of the comets at all events, we are dealing with a class of bodies in which a compound of carbon is concerned, or if not that, that the molecules of the comet resemble somewhat those of the chemical substances which give us such spectra.

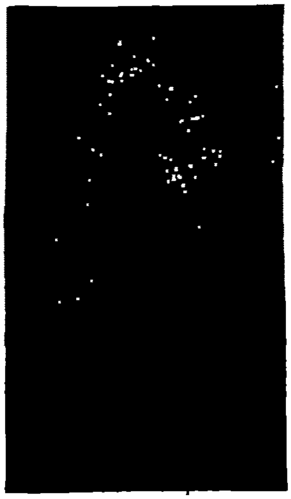
FIG. 1.—Head and Envelopes of a Comet.

Closely connected with comets by the recent hypothesis of Schiaparelli, are meteors, and falling-stars. So far as I know at present, in the case of falling-stars the cometary spectrum has not been seen, but it has been noticed in several shooting-stars that the vapour of sodium is present, indicated by the double bright line in the yellow part of the spectrum. Now the meteorites are large meteors which have fallen to the

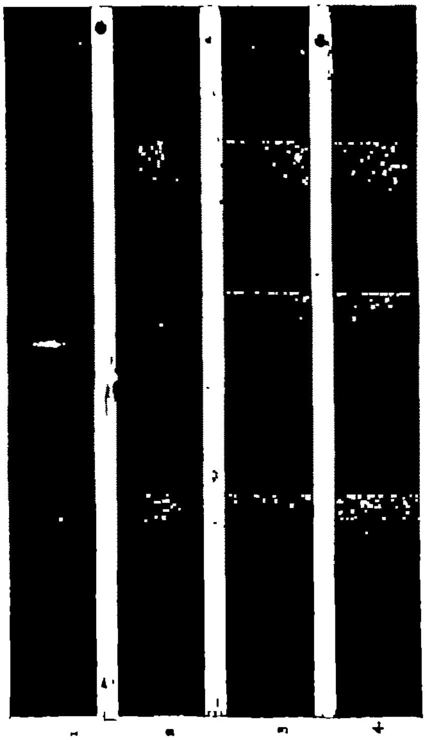

FIG. 2.—1, Spectrum of Brorsen's comet, 2, Spectrum of Winnecke's comet, 3, Spectrum of carbon in olefiant gas; 4, Spectrum of carbon in olive oil. (Huggins.)

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* Revised from short-hand notes of a Lecture delivered at the Quebec Institute, on Tuesday, December 16, 1873. Continued from p. 414.

earth, and this being so, we can chemically examine them as easily as substances which have consolidated here. These bodies may be roughly divided into iron meteorites and stone meteorites, and roughly again into meteorites which contain elementary metallic molecules, and others which contain compound molecules, that is, mixtures of metals with the metalloids.

The observations of the spectra of nebulæ we owe mainly to Mr. Huggins, a distinguished English observer, who has given very much attention to this branch of inquiry. One thing we know for certain about the nebulæ from these observations is this: we are dealing either with gases alone or with solids banging about in gases, and one of the bright lines which we observe tells us that we are dealing with hydrogen gas; so that the same method of inquiry which, applied to comets, tells us that probably in some cases we are dealing with a compound of carbon, tells us that we are chiefly dealing, in the case of nebulæ, not with a compound of carbon or with a compound of anything, but with a true element—hydrogen.

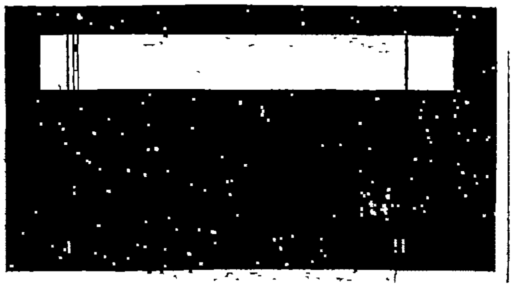

FIG. 3.—Spectrum of the nebulæ.—1, 2, 3, lines observed. Above, the solar spectrum is shown from *b* to F; below, the bright lines of magnesium, nitrogen, barium, and hydrogen, in the corresponding part of the spectrum.

That, you see, is a very great step; and a very few years ago it would have been considered presumptuous almost to think that man could ever tell what substances were building up the nebulæ which lie at such infinitely remote distances from us.

So much then for comets and meteors and nebulæ. These exhaust the bodies in space which shine by their own light, the light not being subsequently absorbed by an atmosphere through which it passes; we will now therefore pass to the class of Stars.

With regard to stars, I have a diagram to bring before you, which summarises in a convenient way a good deal of the work which has been very carefully done by Father Secchi of Rome. I shall have to refer to several other diagrams afterwards; but this, I think, is the best one to place before you in the first instance.

The spectra in the diagram are the spectra of various stars. You will at once see that there is a difference between those spectra, and you will see that there is a double difference between some of them. In the first place, you have an extreme simplicity in some cases and complication in the others. But you see, also, that the question of simplicity and complication is not the only question, that is a question merely of degree; but there is a difference in kind. For instance, you will at once acknowledge a difference in kind between the spectrum of α Herculis and Sirius.

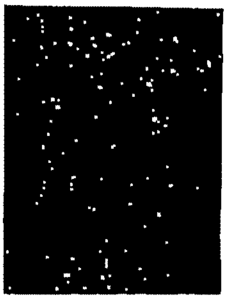

FIG. 4.—Ring nebula in Lyra, with its spectrum.

The diagram of the former, although it has been made in no laboratory, and although it deals with no metalloids or no compounds which have been got upon the earth, is as good a diagram as I can put before you to explain what I mean by the channelled structure of the spectrum of the metalloids as opposed to the line spectrum of the metals.

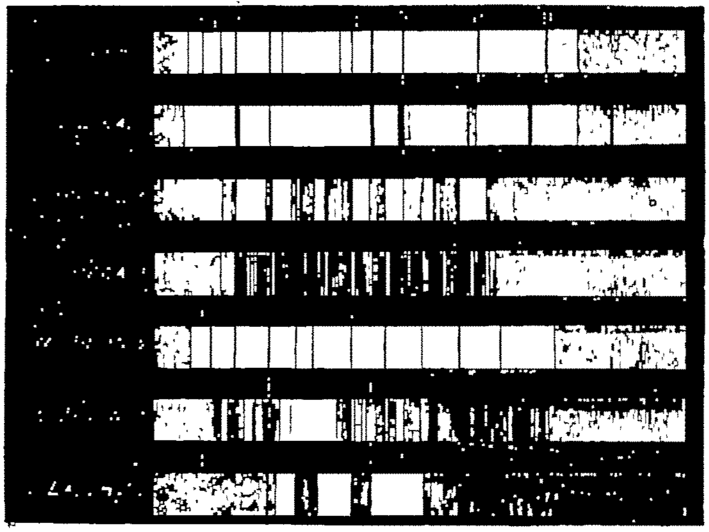

FIG. 5.—Various Stellar Spectra (Secchi).

Bearing in mind the great simplicity of the spectra of stars like α Lyræ and Sirius, and the greater complexity of the spectral lines in a star like the sun; bearing in mind, also, the difference in kind between the spectra that I have referred to, we can divide all the stars which shine in the heavens which we have already observed, into three classes. This has been done by Rutherfurd and Secchi.

Let me strengthen what I have to say by showing you rather more elaborate drawings of three stars belonging to these different classes. You will now see the importance of the considerations which I have brought before you regarding the spectra of the metals and the metalloids. There is the channelled space spectrum of the star α Herculis; there is the banded spectrum of the star α Orionis; and there is the equally banded spectrum of a star in the constellation of the Scorpion. In all these cases you will see we are dealing, not with the first class in which we have simple spectra, but with the second and third classes.

Now let me contrast on another diagram the spectra of two stars, one in the first class and the other in the second. Let us contrast the spectrum of Sirius with the spectrum of the star in Orion, to which I have already referred. In the spectrum of

Sirius observe—for this is a very important point—the extreme thickness of the lines, which are the lines due to hydrogen, and contrast the thickness of these lines and the simplicity of the spectrum with the thinness and great number of the lines in the star in Orion, and the complexity of its spectrum; and remembering that both these maps are on the same scale, let me point out that all the lines which are so thick and so obvious in the spectrum of Sirius, are altogether wanting in the spectrum of the star in Orion.

I hope I have convinced you, by the sight of these diagrams, that supposing the observations on which they are based to be true, we have in the stars which shine three perfectly different kinds of absorption of light going on in the atmospheres of those stars. We have an absorption which we may call a simple absorption, seeing that the lines are few in number; we have an absorption of the same kind, but different in degree, which we may call complicated, seeing that the lines are still lines, but that they are very much increased in number. And, again, in the third class we have an absorption of a different kind altogether; instead of having an absorption of lines we have an absorption of bands. This I shall venture to call a metalloidal absorption.

Of course if we were merely limited to the spectrum of these distant stars, in spite of the enormous skill and care which Mr. Huggins and Father Secchi have brought to bear upon this inquiry, we probably should never go very much further; but you know that the sun is, after all, our sun, merely for the reason that it is the nearest star; and therefore it is clear to you that if we observe the sun with anything like the attention that it deserves, bearing in mind its comparative nearness to us, we ought to be able to get out of the sun a great number of facts which will help us the better to understand the various appearances in the different stars.

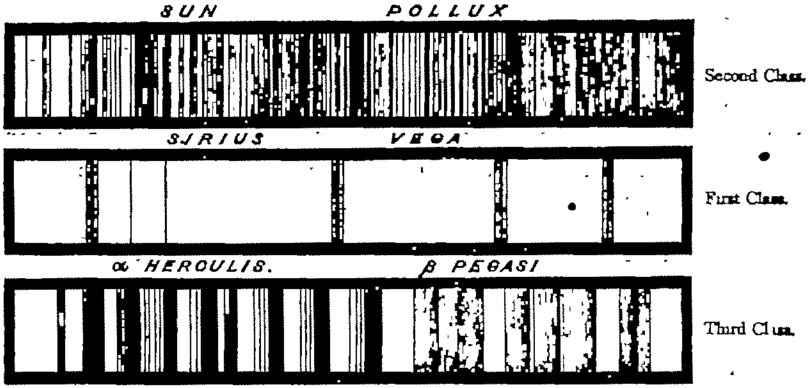

FIG. 6—The Three Classes of Stars (Secchi).

I need not say to you that a great deal of trouble has been taken to understand the sun, to study its physical and even its chemical constitution; and if you will allow me, I will put before you two or three considerations having reference to the sun, which have a bearing of considerable importance upon Celestial Chemistry.

In the first place, let me call your attention to the sun as we see it ordinarily. We see that on the sun there are spots, and that on the limb there is a dimming; both the dimming of the limb and of the spots being due to the absorption of the sun's atmosphere which is at work, as I have already told you in the case of the stars, and which separates the stars as a class from the comets, meteors, and the nebulæ.

Next consider the sun as it is seen in an eclipse. Some of you may be surprised to learn that the sun, as we see it every day, is not by any means the whole sun, but only, so to speak, the kernel of an enormous mass of vapour extending for thousands and tens of thousands of miles around the visible sun.

Now in an eclipse, when all the sun ordinarily visible is hidden, we get indications of a very bright something extending to some little distance above the visible sun. On this point I may specially call attention to a photograph taken in the eclipse of 1870, at Syracuse, in which, outside the dark moon which is covering the sun, and therefore outside the sun, a mass of light which we know to be due to vapours surrounding the sun is indicated.

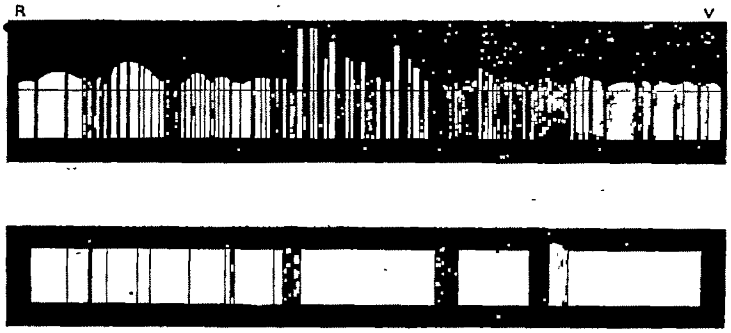

FIG. 7—Spectra of α Orionis and Sirius (Secchi). In the spectrum of α Orionis the brightness of the spectrum is represented by the height

In a photograph taken in India in the year 1871, under somewhat different conditions, we were fortunate enough to record a great deal more of the sun's surroundings. In this, surrounding the dark moon, we have an immense mass of something not very bright extending to a very considerable distance indeed above the visible sun.

Now, by studying these phenomena in the case of the sun, of course we are studying similar things in the case of every star; and what do we find? We find, in the case of the sun, that surrounding the visible sun there extends to a very considerable distance an atmosphere of an element that we have not here, and which is probably lighter than hydrogen. Immersed in this, and therefore extending to a smaller distance from the sun, is another envelope, which has been called the chromosphere, consisting, in the main, of hydrogen. The brightest part of this lies pretty close to the sun. This region is excessively bright—so bright, that by a certain method it can be seen without any eclipse whatever. Immersed in this hydrogen and therefore still nearer the sun there is an enormous quantity of vapours of the different elements existing in the sun, in what we may term a reversing layer, and it is to the absorption of the elements in this layer that the absorption of the sunlight, and therefore, so to speak, the creation of the spectrum of the sun, is in the main due.

I will now direct your attention to two photographs of the solar spectrum, and reminding you that the complexity of a spectrum depends upon the number of elements, and upon the pressure at which the vapours of those elements exist in the atmosphere of any star, you will gather from these photographs a pretty good idea of the extreme complexity of the sun's reversing layer to which I have referred.

In a photograph of any part of the sun's spectrum each of the lines of course has its story to tell, not only so far as the substance which is at work doing that particular part of the absorption is concerned, but also even so far as the quantity of that substance is concerned; because not only will a certain substance absorb particular waves of light, but it will absorb many waves, or few, or none at all, according to the quantity of that particular substance in the envelope surrounding the sun. Now there is a great deal of calcium in the sun, and therefore the absorption lines of the calcium are very thick, the absorption lines of the other metals which do not exist in such great quantities in the sun being very much thinner.

Having brought before you these various points connected mainly with the sun, so far as its physical constitution goes, let us consider what is the chemical constitution of the sun.

I have already told you that surrounding the sun is an envelope composed in the main of hydrogen, and of a new element, and that nearer to the sun is a region of vapours of great complexity, containing at least one new element. This region contains, besides hydrogen, and dealing with known elements, magnesium, sodium, titanium, calcium, nickel, chromium, iron, manganese, aluminum, copper, zinc, barium, cobalt, and so on, and latterly we have had reason to suppose that some six or seven new elements must be added to the list—potassium, lead, cerium, uranium, strontium, and cadmium. Further, if instead of the new "atomic weights" of the elements we take the old "combining weight" we find that the arrangement of these layers round the sun follows the vapour densities of the various substances either absolutely or very closely.

This then is the verdict of the prism with regard to the chemical constitution of the sun, the nearest star that we can get at; and I think you will acknowledge that if the prism had done nothing else it would have done good work. But I think it has done very much more, because it has enabled us not only to chronicle those things as existing in the sun, but in connection with the other facts which I have already brought before you it has enabled us to place the sun in its proper position amongst the stars. For instance, I have already called your attention to the first, second, and third classes of stars. Is the sun in the first, the second, or the third class? Does its spectrum contain few or many lines? or are there channelled spaces or bands? Its spectrum is not excessively simple; there are no channelled spaces or bands; and therefore the sun is to be placed in the second class of stars. Can we then go beyond this chronicle of facts, which I am afraid some of you may have considered rather dry?

You know that what a scientific man has to do in any research is not merely to add fact to fact, and to go blindly looking after facts irrespective of order. What he has to do after he has accumulated a certain number of facts is, to try whether it is possible to arrange them in order. If you wish to get a law out of any accumulation of facts in physics, in chemistry, or astronomy, you must first get your facts into order or you will never do it. Is there any possible order into which we can group these various facts to which I have referred? I venture to think there is.

Call to mind the three classes of stars. Is there any other physical quality tacked on to those differences? Yes. The stars with the simplest spectra are on the whole the brightest stars in the heavens; and the channelled spaced stars are on the whole the dimmest stars in the heavens. Of about 500 stars which have been already observed, over 300 are of the complicated second order or type. There are a great many bright stars of the first order, but an extremely small number, only, I think, about 27 of the third order with the channelled space spectrum. Now, if this be true, and if it be fair to assume that the star which is the brightest is on the whole the hottest, and I think it is fair to say so, if you take all other things as equal, then you come to a generalisation of this kind, that the brightest and hottest stars in the heavens have the simplest spectra, and the dimmest, reddest, coldest stars have a spectrum entirely different. If this be so, can we connect these facts? I think so. Grant these facts (and the future alone will show whether they are facts), and the thing is clear. We may group them all together by supposing that in the stars of the first and second classes there are dissociating forces at work which, from considerations which I have not time to bring before you now, we can imagine to be infinitely higher, or at least considerably higher, than any dissociating force that we can get here even with the electric spark. If you imagine in these stars an atom-severing force greater than we can obtain here, you can at once group in a working hypothesis all the facts which I have brought before you, and in a simple way; and let me add that simplicity in Science is a very great evidence of truth.

If you assume that at the highest possible temperature—here I use the word temperature for want of a better—of a star you have work done in continuation of the work done in terrestrial furnaces, that is to say, if instead of having 63 elements which we have here with our furnaces, there is a much smaller number, taking into consideration the increase of temperature, you will see at once that the brightest and hottest star in the heavens should have the simplest spectrum, because there you have the fewest elements, and that the coldest, reddest, dimmest star should have a spectrum indicating metalloids and compounds because you have there a low temperature, at which the metals do not exist except in combination. And if you think this matter over you will see that this suggestion of a higher temperature giving us a simpler condition of what we with our paltry temperatures call chemical elements, instead of making these stellar spectra complicated, difficult to recollect and to understand, puts them all in one line easily to be grasped, and a line which I venture to think is somewhat coincident with the probabilities of the case.

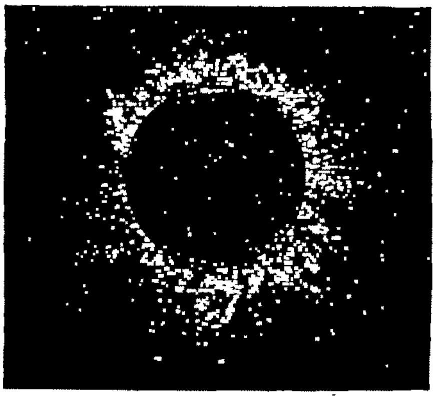

Fig. 8.—The Corona (Indian Eclipse, 1871).

Does the work stop there? Has it nothing to say to comets and meteorites? Here again it has a question to ask. The beautiful hypothesis of Schiaparelli, which is accepted by all astronomers, has made it perfectly clear that a comet is nothing more than one of an infinite number of meteors or meteorites, or whatever you like to call them, travelling in cometary orbits round the sun, and to which the showers of shooting stars are due. We know that there is a comet connected with that particular meteor swarm which gives us the November meteors, and we know that there is another comet connected with that particular meteor swarm which gives us the August meteors; and we assume that in all probability there are millions, or any enormous number that you like, of meteors, or meteorites, or shooting stars peopling a part or the whole length of that concrete orbit so to speak.

How is it then that there is only one comet amongst all that infinite number of potential meteors or meteorites? Here again I am sure that the future will enable the prism to throw an immense light, and we already have a glimmering. If, for instance, you assume that out of a star of the second class, in the reversing layer of which there are no metalloids, portions of the atmosphere are, by forces which we know to be at work, thrown bodily from the sphere of the star's attraction into space, those vapours on being cooled would give us very much the same kind of chemical composition that we get in the well-known masses called iron meteorites, composed principally of iron and nickel. While on the other hand, in the case of stars of the third class, in which it seems excessively probable that we have both metalloids and compounds, and very little pure metal, that is to say, metal not in combination, in the reversing layer, we have also the large class of silicate meteorites, the origin of which is possibly due to such stars in exactly the same way as the origin of the iron meteorites would be due to stars of the second class.

If this be so, then it would seem that a comet is simply a meteorite which contains something which is volatile at a very low temperature. Amongst the vapours known to chemists, which are volatile at the lowest temperature, are the hydrocarbons. I have already pointed out to you that as far as observations have gone on comets we have been able to detect nothing but the possibility of a spectrum of carbon, or of a compound of carbon.

Here again dimly and darkly the prism is pointing us to a possible connection between all the stars in heaven and all the comets and all the meteors which flit through the celestial spaces and fall upon our own earth.

I have already referred to the verdict of the prism in connection with the nebulæ, and there can be very little doubt, I think, that before the world is very much older the prism will also be perfectly competent to connect nebulæ with stars as it may possibly have already connected comets and meteors with them; but this point certainly is at present one of great difficulty, and it is a difficulty which no student of science will care to get out of, since in matters of this kind a difficulty is a matter of the highest importance, showing you as it does that part of the field of nature which requires most study.

I quite feel that this enormous subject, which modern science is opening up, is one the importance of which is so great and the interest in which is so general that I am sorry that the task of talking about it has not fallen upon the shoulders of one who is more competent to do it than I am. I hope, however, that feeble as my advocacy may have been, you are prepared to agree that the time will come when Celestial Chemistry, as investigated by means of the prism, will be acknowledged to be one of the most important branches of modern science.

J. Norman Lockyer

SCIENTIFIC SERIALS

Journal of the Franklin Institute, February.—In this number Mr. Prindle has a paper (with numerous illustrations) On Recent Improvements in Construction of the Gunpowder Pile-driver.—A long and instructive paper by Mr. Loiseau, On Artificial Fuel, gives a *résumé* of what has hitherto been done in this direction; the author describing his own method, in which a mixture of 5 per cent. clay and 95 per cent. coal-dust, moistened with milk of lime, is moulded into oval lumps, which are then bathed in a waterproofing liquid (rosin dissolved in crude benzine) and dried. With 14 men only, a production of 150 tons per day can, it is said, be easily attained.—Prof. Houston announces the discovery of a new allotropic modification of phosphorus, obtained by boiling good phosphorus repeatedly in potassium hydrate (under certain conditions). This new modification retains for an indefinite time, apparently, the liquid state, even when exposed to temperatures considerably below the melting-point of ordinary phosphorus, from which it also differs in its non-oxidation on exposure to air, and, consequently, its not shining in the dark.—Mr. Chesebrough describes the construction of the Detroit River Tunnel; and Prof. Thurston has a note relative to the estimation of the chemical value of coals containing large quantities of ash.—Among the "Items and Novelties," it is stated that Prof. Thurston has gone very carefully into the subject of a scheme published by Mr. Chesebrough, for keeping canals open in winter by warming the water. The professor's calculations are given, and he finally arrives at an estimate of 5,412,500 dols. as the first cost of apparatus for a canal 350 miles long, 70 feet deep, in the latitude of Central New York; and 1,670,200 dols. for the maintenance per annum. He thinks the scheme deserving of investigation.

Astronomische Nachrichten, Nos. 1,976 and 1,977.—These numbers contain a paper by M. Lohse on the estimation of the depth of sun-spots, and at the same time to ascertain the influence of solar refraction. The principle of his method is as follows:—When a spot having its umbra concentric with penumbra, when seen from the vertical, is seen near the sun's limb, the umbra becomes excentric, and the depth of the umbra p' will be given by the formula $p' = \frac{\epsilon}{\cos \alpha}$ when ϵ is the excentricity and α the heliocentric position-angle of the spot from the axis, and assuming the spot steady, p' should remain constant as α and with it ϵ change. This, however, is found not to be the case, for p' gradually increases as the spot gets from the limb, showing it was raised by refraction at the limb, and therefore this change in the value of p' gives a measure of the sun's refraction. The same author also contributes a paper on the effect of the atmosphere of Venus in the transit over the sun, and he recommends the examination of its atmosphere with the spectroscope for absorption-bands.—J. Hortard gives the observations of transits for longitude of Nikolagew.—J. Palisa gives a large number of ring-micrometer observations on the minor planets and a few comets.—Dr. Holetschek gives the position of some seventy comparison-stars for planets and comets.—E. Stephan gives a list of ten new nebulæ discovered and observed at Marseilles; all seem excessively small. The elements of the new comet are given by Wilhelm as follows:—

T = March 9·8125 Berlin time.
$☊ = 48° 17' 37''·6$
$\Pi = 309° 27' 46''$
$i = 52° 29' 52''·3$
log. $q = 8·591600$

Memorie della Soc. degli Spettroscopisti Italiani, Oct. and Nov. 1873.—These numbers contain an interesting paper by P. Rosa, assistant at the Observatory at Rome, on the variability of the sun's diameter. He discusses the observations at Greenwich and other places from the year 1750 to 1870, during which time some 13,000 measurements of the solar diameter were made. From these observations he has constructed curves showing the variation of diameter, together with the variation in the number of spots. The agreement of these two curves is not very strong, but on his constructing secondary curves from every fourth year of these primary curves, beginning with any year, the resemblance becomes striking. There are also monthly curves given, showing the mean variation in diameter of the sun for each month, taking a mean of ten years for each curve. These show two maxima in March and September, and two minima in January and June. On examining the curve of variation in diameter, a marked minimum occurs about the year 1792. The author sets forth a theory to account for this variation, that since a comet, when at its perihelion, throws off large quantities of matter, so the sun when at its periastre may throw off matter and become reduced in size, and if such is the case its periastre happened in 1792.—G. Lorenzoni contributes papers on observations on the chromosphere, and on observing contacts in eclipses of the sun with the spectroscope.—G. de Lisa gives observations on solar spots in September, October, and November 1873, made at Palermo, giving a mean of about ten spots a day.—Prof. Young gives a note on the use of M. Rutherford's gratings in the place of prisms for the spectroscope.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 19.—On the Attractions of Magnets and Electric Conductors, by George Gore, F.R.S.

Being desirous of ascertaining whether in the case of two parallel wires conveying electric currents the attractions and repulsions were between the currents themselves or the substances conveying them, and believing this question had not been previously settled, I made the following experiment:—

I passed a powerful voltaic current through the thick copper wire of a large electromagnet, and then divided it equally between two vertical pieces of thin platinum wire of equal diameter and length (about six or seven centimetres), so as to make them equally white hot, the two wires being attached to two horizontal cross wires of copper.

On approaching the two vertical wires symmetrically towards the vertical face of the one pole of the horizontally placed magnet, and at equal distances from it, so that the two downward currents in them might be equally acted upon by the downward and upward portions respectively of the currents which circulated round the magnet-pole, the one was strongly bent towards and the other from the pole, as was, of course, expected; but not the least sign of alteration of relative temperature of the two wires could be perceived, thereby proving that not even a small proportion of the current was repulsed from the repelled wire, or drawn into the attracted one, as would have occurred had the attraction and repulsion taken place, even to a moderate degree, between the currents themselves; and I therefore conclude that *the attractions and repulsions of electric conductors are not exerted between the currents themselves, but between the substances conveying them.*

Some important consequences appear to flow from this conclusion, especially when it is considered in connection with Ampère's theory of magnetism, and with the molecular changes produced in bodies generally by electric currents and magnetism.

As every molecular disturbance produces an electric alteration in bodies so, conversely, the discoveries of numerous investigators have shown that every electric current passing near or through a substance produces a molecular change, which is rendered manifest in all vessels, liquid conductors, and even in the voltaic arc by the development of sounds, especially if the substances are under the influence of two currents at right angles to each other. In iron it is conspicuously shown also by electro-torsion, a phenomenon I have found and recently made known in a paper read before the Royal Society.

Numerous facts also support the conclusion that the molecular changes referred to last as long as the current. De la Rive has shown that a rod of iron, either transmitting or encircled by an electric current, emits, as long as the current lasts, a different sound when struck; and we know it also exhibits magnetism. The peculiar optical properties of glass and other bodies with regard to polarised light discovered by Faraday also continue as long as the current. A rod of iron also remains twisted as long as it transmits and is encircled by electric currents; and in steel and iron the molecular change (like magnetism) partly remains after the currents cease, and enables the bar to remain twisted.

That the peculiar molecular structure produced in bodies generally by the action of electric currents also possesses a definite direction with regard to that of the current, is shown by the rigidly definite direction of action of magnetised glass and many other transparent bodies upon polarised light; also by the difference of conductivity for heat and for electricity in a plate of iron parallel or transverse to electric currents; by the stratified character of electric discharges in rarefied gases, and the action of electric currents upon it; and especially by the phenomenon of electro-torsion. In the latter example an upward current produces a reverse direction of twist to a downward one, and a right-handed current developes an opposite torsion to a left-handed one; and the two latter are each internally different from the former. As each of these four torsions is an outward manifestation of the collective result of internal molecular disturbance, and possesses different properties, these four cases prove the existence of four distinct molecular movements and four corresponding directions of structure; and the phenomena altogether are of the most rigidly definite character.

As an electric current imparts a definite direction of molecular structure to bodies, and as the attractions and repulsions of electric wires are between the wires themselves and not between the currents, repulsion instead of attraction must be due to *difference of direction of structure* produced by difference of direction of the currents.

Although the Ampèrean theory has rendered immense service to magnetic science, and agrees admirably with all the phenomena of electro-magnetic attraction, repulsion, and motion, it is in some respects defective; it assumes that magnetism is due to innumerable little electric currents continually circulating in one uniform direction round the molecules of the iron; but there is no known instance of electric currents being maintained without the consumption of power, and in magnets there is no source of power; electric currents also generate heat, but a magnet is not a heated body.

If, however, we substitute the view that the phenomena of attraction and repulsion of magnets are due, not to continuously circulating electric currents, but (as in electric wires) to definite directions of molecular structure, such as is shown by the phenomena of electro-torsion to really exist in them, the theory becomes more perfect. It would also agree with the fact that iron and steel have the power of retaining both magnetism and the electro-torsional state after the currents or other causes producing them have ceased.

According to this view, a magnet, like a spring, is not a source of power, but only an arrangement for storing it up, the power being retained by some internal disposition of its particles acting like a "ratchet," and termed "coercive power." The fact that a magnet becomes warm when its variations of magnetism are great and rapidly repeated, does not contradict this view, because we know it has then, like any other conductor of electricity, electric currents induced in it, and these develop heat by conduction-resistance.

According also to this view any method which will produce the requisite direction of structure in a body will impart to it the capacity of being acted upon by a magnet; and any substance, ferruginous or not, which possesses that structure has that capacity; and in accordance with this we find that a crystal of cyanite (a silicate of alumina) possesses the property, whilst freely suspended, of pointing north and south by the directive influence of terrestrial magnetism, and one of stannite (oxide of tin) points east and west under the same conditions.

Geological Society, March 11.—John Evans, F.R.S., president, in the chair.—The following communications were read:—On the relationship existing between the *Echinothuridæ* Wyville Thomson and the *Perischoechinidæ* McCoy, by R. Etheridge, jun. In this paper the author referred in the first place to the peculiar characters of the genera *Calveria* and *Phormosoma* Wyville Thomson, and especially to those in which they approach the cretaceous genus *Echinothuria* S. P. Woodward, and which led Prof. Wyville Thomson to include these three forms in his group Echinothuridæ. He remarked that an overlapping of the interambulacral plates, more or less like that occurring in these three genera, is met with also in *Archæocidaris* McCoy, and *Lepidechinus* Hall, belonging to the group of palæozoic Echini which McCoy proposed to call Perischoechinidæ, and which is characterised by the presence of more than three rows of plates in the inter-ambulacral areas. As there is no overlapping of these plates in the other genera referred to this group, it includes two types of structure. The author then discussed the characters presented by the test in the genera of the Perischoechinidæ (namely, *Archæocidaris, Palæchinus Perischodomus, Lepidechinus, Eocidaris, Melonites*, and *Oligoporus*), and pointed out that although we have no conclusive evidence of the presence of membranous interspaces along with the overlapping plates in *Archæocidaris*, the fragmentary condition in which the remains of that form are usually found would lead us to infer their existence. No known palæozoic genus exhibits the want of distinction between the ambulacra and interambulacra on the ventral half of the test seen in the recent genus *Phormosoma*. In *Melonites* and *Oligoporus* the author described an increase in the number of rows of plates in the ambulacra, and he indicated that all the Perischoechinidæ differ from the later Echini by the increased number of perforations in the ocular and genital plates.—On the discovery of Foraminifera, &c., in the boulder-clays of Cheshire, by William Shone, jun. In this paper the author described the occurrence of Foraminifera, Entomostraca, and some other small organic bodies in the boulder-clay at Newton by Chester and at Dawpool. They were found partly in the interior of specimens of *Turritella terebra*, and partly free in the boulder-clay.—On the occurrence of a Tremadoc area near the Wrekin in South Shropshire, with description of a new fauna, by Charles Callaway. The author stated that in an exposure of light green, micaceous shales dipping south-east at 50° at Shineton near Cressage, which are represented as of Caradoc age in the Geological Survey Map, he found a series of trilobites and other fossils which induced him to regard these Shineton shales as belonging to the Lower Tremadoc series. He described as new species:—*Asaphus eos, Conocoryphe salteri, C. angulifrons, Platypeltis croftii, Conophrys salopiensis, Lichapyge cuspidata, Lingulella nicholsoni, Metoptoma sabrinæ*, and *Theca lineata*. The author regarded these shales as the equivalents of beds containing *Dictyonema*, found near Malvern and at Pedwardine.

Anthropological Institute, March 24.—Prof. George Busk, F.R.S., president, in the chair.—The President exhibited and described an Ashanti skull. The specimen, with other bones of the body, was taken by Surgeon-Major Gore from an outlying camp which had been deserted on the approach of the British troops. It presented the characteristics rather of a female than a male skull, but Mr. Gore affirmed that he had never heard of the Ashantis carrying about the bones of a woman. Women, in fact, held such an inferior position, that it could scarcely be believed that the Ashantis would take trouble in the preservation of their remains. If the skull exhibited belonged to a man, he could not have been a military leader, but he might have had such a rank in his tribe as entitled him to the honours that were evidently bestowed on his remains. The paper gave full descriptions and detailed measurements.—A paper was read by Rev. Dunbar I. Heath, On the Origin and Development of the Mental Function in Man. He thought that in the ordinary view the mind is considered as a central essence. Around it is the brain, and still further on the outside the world surrounds the brain. It would conduce towards explaining the facts of mental function if we supposed a material film to coincide with the outside surface of the brain, which might be specialised under the name of Psychoplasm. To that film he would confine mental, as distinguished from cerebral, function; so that the mind would be imaged, not as being the centre, but between brain and world. The paper explained mental growth on that hypothesis.—Mr. W. L. Distant read a paper On the Mental Differences between the Sexes. The question discussed in the paper was—Is there clearly proved to be mental difference between the sexes, and is that difference one of kind or only of degree? Authorities were quoted to show the undoubted physical differences, such as weight of brain, form of skull, &c., also the now moderately well-established fact that in primitive races the hair of women approximates more closely to that of man than obtains in a higher state of civilisation. But it having been clearly proved that the advance of man is shown by a higher form of skull and increase of the cranial capacity, an attempt was made to show some of the conditions that had retarded woman in the mental struggle. The result seemed to prove that the mental divergences might be greatly accounted for—firstly, by sexual selection, difference of education, and force of custom; secondly, by physiological conditions; and that as the race progresses, the cranial capacity of the sexes, though not becoming identical, which is a physical impossibility, will yet become much less distinct and divergent, which is a moral certainty if based on moral conclusions.

Physical Society, March 21.—Dr. J. H. Gladstone, F.R.S., in the chair.—J. H. Fleming, read a paper On the new contact-theory of the Galvanic Cell. After discussing the most recent views regarding the contact and chemical theories, Mr. Fleming exhibited the action of his new battery in which metallic contact of dissimilar metals is completely avoided. The battery consisted of thirty test-tubes of dilute nitric acid alternating with the same number of tubes of sodium pentasulphide, all well insulated. Bent strips of alternate lead and copper connected the neighbouring tubes. By this device the terminal poles are of the same metal. On connecting with a coarse galvanometer, the needle was violently and permanently deflected. Tested by the quadrant electrometer the potential was shown to increase regularly with the number of cells. The sixty cells on first immersion showed an electromotive force exceeding that of Daniell's cells. The principle upon which the action depends is that in the acid lead is positive to copper; in the sulphide it is negative. Mr. Fleming further showed how by using the single fluid nitric acid and the single metal iron, a similar battery could be constructed, provided one-half of each iron strip was rendered passive. In this form also no metallic contacts occurred.—Prof. F. Guthrie illustrated by experiment the distribution of a current of electricity in passing from one pole to another across a conducting medium. This was shown in the case of solids by the stratification of iron filings in the sheets of tin-foil and lead. A current of electricity was passed between two points in a horizontal line lying on the surface of metal placed vertically in the magnetic meridian, and the distribution explored by means of a freely suspended magnet needle. As the needle was gradually lowered its direction of deflection was observed to change at a certain point from east to west. This point was ascertained by experiment to be at a distance below the horizontal line, in which the current entered and left the plate, equal to one-third of the interval between the poles. A similar effect was shown in a liquid conductor.—Prof. G. C. Foster, Dr. Wright, and Dr. Gladstone took part in the discussion of the communications.

Royal Horticultural Society, March 18.—Scientific Committee. Dr. Hooker, P.R.S., in the chair.—The Rev. M. J. Berkeley brought for exhibition Montagne's original drawings of *Artotrogus*. He pointed out that this was only the $\frac{1}{1000}$ inch in diameter, while *Volutella*, with which Mr. W. G. Smith had supposed it might be identical, was from $\frac{1}{160}$ to $\frac{1}{300}$ inch in diameter. Montagne had also found a second species of *Artotrogus*, and of this he showed a drawing. He also remarked that a knowledge of the resting spore of *Peronospora infestans* was a great desideratum. It was to be hoped that, as 100*l.* had been presented to Prof. de Bary to investigate the whole subject, that that would be a matter on which he would throw some light.—Mr. Smee communicated a paper on a disease at present very destructive to *Daphne indica*. Numerous diseased plants were exhibited, and the opinion of the Committee was requested upon them. Prof. Westwood said that as the young leaves of the Daphne were entirely free from acari or the young larvæ of *Coccidæ* or *Aphidæ*, although the adjoining full-grown leaves were much diseased, he was not inclined to regard the disease as originating from the attacks of any of these insects, although it might be due to punctures of some flying species of *Capsidæ*, such as *Phytocoris campestris*, which attacks the buds and young foliage of the common Chrysanthemum, flying from plant to plant.—Prof. Westwood adverted to the Tea Bug of Assam, which he believed to be identical in Upper India, Java, and Ceylon, and not a new species. The insects of Java were often identical with those of Assam, but he supposed that in this case the insect might have been conveyed from one to the other. Dr. Hooker said that this was very probable. The Ceylon tea-plant was the so-called "hybrid variety" introduced from Assam, and was probably sent from Ceylon to Java.—Prof. Thiselton Dyer exhibited a specimen of an Acacia with a curious white balaniform exudation of insect origin, from the Botanic Garden at Cape Town. Prof. Westwood stated that the insect upon the Acacia was quite new to him, and was closely allied to the *Cbnops cataphractus*, a rather rare British insect, allied to the *Coccidæ*; the specimens were females, which had emitted a mass of waxy matter, striated in ridges; the waxen mass was in many places covered with minute larvæ, differing in form from the ordinary larvæ of the *Coccidæ*.

General Meeting.—Mr. H. Little in the chair.—The Rev. M. J. Berkeley called attention to pods ripened in the gardens of Mr. W. Terry, of *Vanilla aromatica* (it has fruited this year abundantly in the Victoria house at Kew); a charming specimen of *Aloe plicatilis*—a miniature tree in form, with fine flowering spikes—came from Mr. J. T. Peacock's collection.

NEWCASTLE

Chemical Society, Feb. 26.—Dr. Lunge, president, in the chair.—A paper was read by Mr. J. Pattinson On the rate at which bleaching powder loses its available chlorine. The examination of a number of samples of bleaching powder, from time to time, during about twelve months, was undertaken with the view of making a contribution towards the solution of this question, "How much available chlorine does bleaching powder lose in a given time?" and also to the further one, "Does weak bleaching powder, say containing 32 per cent. of chlorine, retain its strength better than a stronger bleaching powder?" Three sets of samples were obtained from different manufactories on the Tyne, each set consisting of three samples. It was intended that the three samples of each set should be taken from the same portion of lime—one when it contained about 33 per cent. of available chlorine, one when it contained about 35 per cent., and the third when it contained about 37 per cent.—and with this object the lime was placed in a box in the chlorine chamber, so that it could be easily removed in order to take out the samples at each stage. On examining the tables given in the report it is seen that, with reference to the question as to the relative stability of weak and strong bleaching powder, there is practically no difference in the rate at which they lose available chlorine.

GLASGOW

Society of Field Naturalists, March 20.—Annual Meeting.—Mr. J. Allan, vice-president, in the chair.—Mr. P. Cameron, jun., exhibited two sawflies new to Britain: *Blennocampa alterrima* Klug, taken by Dr. Buchanan White at Braemar; *Hoplocampa pectoralis* Thomson, taken by the Rev. T. A. Marshall, F.L.S., at St. Albans. The only recorded locality is Gothland, where it was captured for the first time by Prof. Boheman.—Mr. Cameron also exhibited the two new sawflies described by him in the last number of the *Entomologists' Monthly Magazine*: *Taxonus glottianus*, of which a single specimen was taken at Kenmuir Bank near Glasgow in May; and *Nematus graminis*, a not uncommon species in the district, the larva of which feeds on grasses. The Annual Reports of the Secretary and Treasurer having been read and adopted the Officers and Council for the ensuing year were elected.

PHILADELPHIA

Academy of Natural Sciences, December 2, 1873.—Dr. Ruschenberger, president, in the chair.—*Fertilisation of Yucca*.

Mr. Thomas Meehan detailed at length the discoveries of Dr. Engelmann and Prof. Riley in regard to the fertilisation of the Yucca by the aid of a small night moth, *Pronuba yuccasella* of Riley, and observed that in this region the fertilisation was effected by this insect every year. In the Rocky Mountains of Colorado in 1871, he saw the *Yucca angustifolia* everywhere seeding in great abundance; but in his journey in 1873 he saw not a solitary seed-vessel in any of the plants, and he suggested that perhaps some periodical insect might take the place of the *Pronuba* in that country.—Note on a Fungoid Root Parasite. Mr. Thomas Meehan exhibited a small Norway spruce, in which the branches and leaves were all of a golden tint. He explained that when plants had little food, or lost their fibres in wet soil by which they could not make use of food, the yellow tint was generally exhibited in the leaves of plants. The similarity of the appearances suggesting, he examined and found the roots thickly enveloped by the mycelia of a fungus, which destroyed the young fibres as fast as they were developed. He had supposed it was one of the small microscopic forms of fungi; but in October of the present year the mycelia developed into a brown agaric with a pileus about two inches broad, but the exact species of which he could not positively determine.

Dec. 9.—Mr. Vaux, vice-president, in the chair.—On the Expansion of the Coma in *Asclepiadaceæ*. Mr. Thomas Meehan exhibited some seed-vessels of *Gonolobus obliquus*, and remarked that, though the hairy appendage to the seed known as the coma in asclepiadaceous plants was of course well understood, he knew of no one who had placed on record any observation in regard to the suddenness of the expansion after the seed left the capsule. It was indeed so very rapid, that the common expression of "like a stroke of lightning," was scarcely an exaggeration. It was only with difficulty that the eye could follow the motion. In the seed-vessel each set of long silky hair was drawn up into a close linear fascicle; but on the instant of the seed being relieved from its case, the coma expanded into a perfect hemisphere. Some of the hair formed a right angle, and others more or less acute ones, each seeming to have its fixed place to fall back to. It was generally supposed that these hairy appendages, and others of a similar character in seeds, were for the express purpose of aiding in seed distribution by wind; but he had failed in so many instances to see the advantages, that it often seemed as if it were the seed profiting by developed organs, rather than that these were especially formed for an express purpose. In the case of the *Gonolobus*, it did seem as if there were better grounds than perhaps in any other case for believing that the hairy appendage is designed expressly to facilitate distribution by wind or air currents. The seeds are heavy, and are borne on the plant but a few feet from the ground; they would fall there in a few seconds on the opening of the capsule, if the mass of hair remained long in its closely compact condition.—On Lingula in a Fish of the Susquehanna. Prof. Leidy.

PARIS

Academy of Sciences, March 23.—M. Bertrand in the chair.—The following communications were read:—Thermal study of the phenomena of solution; reaction of water upon nitric acid, by M. Berthelot. As the result of his investigations the author finds that the heat evolved by the addition of an equivalent of water to acids and bases generally decreases in accordance with a law analogous to a geometrical progression when the equivalents of water (n) increase in arithmetical progression. A formula is obtained approaching $Q = \frac{A}{\rho^n}$ where the quantity of heat is Q and ρ, a number near unity. The author discussed the relationship between this formula and the analogous one obtained by M. Becquerel for the electromotive force of acid and alkaline solutions, viz., $= x \frac{a}{r^n}$.—On an operation of transfusion of blood performed by M. Béhier at the Hôtel Dieu: note by M. Bouley.—On the origin of the Muscado mace and of mace in general, by M. H. Baillon.—On the pathogenetic rôle of ferments in surgical maladies; new method of treatment for amputations: note by M. A. Guérin.—On the plane distribution of pressures in the interior of isotropic bodies in the state of limited equilibrium; mode of integration of the differential equations: note by M. J. Boussinesq.—On the law of astronomical attraction on the masses of the different bodies of the solar system, and particularly on the mass and duration of the sun, by M. E. Vicaire. The author seems to think it far from being demonstrated that the number called the *mass* of the sun is a real measure of the quantity of matter contained in it.—Programme of a system of geography founded on the exclusive use of decimal measures, of an international meridian 0°, and of stereoscopic and gnomonic projections, by M. B. de Chancourtois.—On the refraction of compressed water, note by M. Mascart.—Reply to the critical observations of M. H. Sainte-Claire Deville, on a method for the determination of vapour densities, by M. Croullebois. The author attempted to defend the apparatus, of which a description had previously been communicated to the Academy.—On the compounds of hydrogen with the alkaline metals, by MM. L. Troost and P. Hautefeuille. The authors have obtained compounds of potassium and sodium with hydrogen, having the formulæ K_2H and Na_2H, and have studied the tensions at every 10° of the hydrogen evolved on heating these compounds from 330° to 430°. K_2H dissolves a further quantity of hydrogen; Na_2H dissolves only a very small quantity of this gas. The authors find that lithium heated to 500° in hydrogen gas at 760 mm. pressure absorbs seventeen times its volume of the gas, while thallium under the same conditions absorbs only three times its volume.—On some bronzes from China and Japan, by M. H. Morin.—On the exotic terrestrial lombricians of the genera Urocheta and Pericheta, by M. E. Perrier.—On some general facts which arise from comparative androgenesis, by M. A. Chatin.—Atmospheric dusts, by M. G. Tissandier. The author has determined the suspended matter in the air of Paris and made analyses of atmospheric dust.—Researches on the formation of superphosphate of lime, by M. J. Kolb.—On the systems of curve-planes, algebraical or transcendental, defined by two characteristics, by M. Fouret.—Explicit condition that a conic may have a fifth-order contact with a given curve, by M. Painvin.—Two new theorems on the wave surface, by M. A. Mannheim.—On a Greek sundial found by M. O. Rayet at Heracleum of Latmos.—On the magnetisation of steel, by M. E. Bouty.—Calorific effects of magnetism in an electro-magnet with several poles, by M. A. Cazin.—Researches on trichloracetates and their derivatives, by M. A. Clermont. The author has obtained trichloracetyl-urea, by acting upon trichloracetate of urea with phosphoric anhydride, and also by the action of trichloracetyl chloride upon urea. The same substance was obtained by this last reaction by Tommasi and Meldola in this country in January.—On some endosmotic properties of the membrane of the shell of birds' eggs, by M. U. Gayon.—On the red colouring-matter of the blood, by M. Béchamp.—On the employment of potassium bisulphate for the distinction of native sulphides, by M. E. Jannettaz.—Observations on the spermatophores of decapod crustacea, by M. Brocchi.—Differentiation of induced and spontaneous movements.—Study of the action of some reputed anæsthetic agents on the functional irritability of the stamens in *Mahonia*, by M. E. Haeckel.—Experimental study upon "ammoniémie," by MM. V. Feltz and E. Ritter.

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THURSDAY, APRIL 9, 1874

BRITISH QUADRUPEDS

A History of British Quadrupeds. By Thomas Bell, F.R.S. Second Edition. (Van Voorst).

THIS excellent work having originally appeared in 1839, a second edition in 1874 deserves more than a passing notice. In a country like our own, which has been well populated for so many centuries, and in which the people are increasing at a rate only possible in connection with vast strides in our knowledge of sanitary laws, it is not difficult to form several deductions with regard to the nature of the changes which must be taking place in its fauna, together with their ultimate tendencies. As time progresses, works on the zoology of our island, now not many in number, nor large in size, must dwindle to the proportions of those that might be written on a country like China, in which by degrees nearly every wild species has been exterminated. As there, form after form must die out, giving place to the increase in numbers of the one dominant species, man; till in time a history of British quadrupeds will be better studied from the works of Hume and Lingard than from those of White and Bell. These and other considerations make it a question of more than ordinary importance what stress is to be laid, in scientific investigation, either for the purpose of classification or of minute study on the present geographical distribution of animals. On all sides we see remarks which show most clearly that their authors do not fully realise the true bearing of distribution. They think that it is in opposition to the Darwinian hypothesis; that the camel being found in Africa and Asia, whilst its only close ally, the llama, is a native of the Andes, is a significant fact in favour of the doctrine of "special creation," and the tapirs of Sumatra and South America, only, point in the same direction. But when we begin to realise how the whole fauna of countries can be and have been wholly changed within the extremely brief geologic time of man's existence, and that most palæontological evidence is in the same direction, it is clear that the stress which must be laid on the present distribution of any particular form is not so great as might have been imagined from the results obtained by earlier workers on the subject.

The strong predilection of our countrymen for sport also makes it highly improbable that any important fresh species of terrestrial mammals should be added to our fauna, and so we find that Mr. Bell's second edition includes only a single land animal which is not to be found in the first, namely, *Sorex pigmæus*, the Lesser Shrew, the smallest British mammal, with a total length, tail included, of less than 2½ in. The case is different, however, with respect to the aquatic species which visit our shores. The rapid strides made in our knowledge of the Cetacea by the excellent researches of Prof. Reinhardt, Flower, Turner, and others have considerably increased the number of existing genera and species; and this, taken in connection with the improvement in our powers of identification from the skeleton alone, has added so many as ten well-authenticated species new to our fauna. The claims of the Greenland and Atlantic Right whales are however very feeble, and only a single specimen of Cuvier's whale, that described by Prof. Turner from Shetland, is known. Amongst the Phocidæ, also, a specimen obtained by Mr. J. H. Gurney, and identified by Prof. Flower, adds the Ringed Seal; whilst the Hooded Seal has been twice obtained from our eastern coast. Several changes have also had to be made among the Cheiroptera. None have had to be added, but some have been removed, on account of previous imperfect identification. The magnified views of the nose and head of each of our native species at the end of the different chapters, when taken in connection with the carefully compiled tables of measurements, will make it easy for anyone who obtains specimens of these rarely seen animals to identify them without much labour.

Mr. Bell in this edition of his work has, for several reasons, had to avail himself of the assistance of other workers in the same subject, for its completion. The Cheiroptera and Insectivora have been carefully revised and brought up to our present state of knowledge by Mr. R. F. Tomes; but the latter part of the book, including all the new matter on the seals and whales, has been undertaken and most efficiently executed by Mr. E. R. Alston. This latter gentleman, from his acute discriminative powers and persevering industry, has made the portions of the work which it has been his good fortune to superintend the standard literature of the subject on which he treats.

Throughout the book there is an ease and elegance of style which is rarely found, now-a-days, in connection with the frequently but too dryly stated facts of science. This adds an attractiveness to the subject which implants and developes an extra charm in the mind of the reader, leading him on, by its inherent value, to the perusal of page after page, till he has finished the book, and unconsciously acquired an amount of zoological information, that, but for the manner in which it is presented to him, he would never have taken the trouble to acquire. As an example we may quote the description given of the wide-spread superstitions and prejudices which exist with reference to bats in general.

We read, "That the ancient Greek and Roman poets, furnished with exaggerated accounts of the animals infesting the remote regions with which their commerce or their conquests had made them acquainted, should have caught eagerly at those marvellous stories and descriptions, and rendered them subservient to their fabulous but highly imaginative mythology, is not wonderful; and it is more than probable that some of the Indian species of bats, with their predatory habits, their multitudinous numbers, their obscure and mysterious retreats, and the strange combination of the character of beast and bird which they were believed to possess, gave to Virgil the idea, which he has so poetically worked out, of the Harpies which fell upon the hastily-spread tables of his hero and his companions, and polluted whilst they devoured the feast from which they had driven the affrighted guests"—rather active measures for a Pteropine bat, no doubt, but none the less within the limits of human exaggerative powers.

We notice that Mr. Alston, in naming the families of the animals of which he writes, uses the termination -idæ on all occasions, as in Phocidæ, Balaenopteridæ, &c.; but in the earlier part of the work, when the generic name from which that of the family is derived, ends in -a, the termination -adæ is employed, so that we find the words, Ursidæ, Musteladæ, Talpadæ, &c. With all due deference to Mr. Bell, and in spite of the first line of Lucretius' poem, which commences with "Æneadum genetrix," we cannot help feeling that for the sake of uniformity and the feelings of the many propounders of scientific names who are not so well versed in the dead languages as they might be, it is better to continue the now nearly universally employed -idæ on all occasions.

The illustrations of the species described maintain the general character of the work, some being evidently new, as in the case of the deer. Many chapters have a picturesque and respectively appropriate sketch as a conclusion; and we notice that in the additional chapters, instead of fresh sketches, there are in their place (we say it with regret) views, both in profile and from above, of parts of the skeletons of the subjects of the text.

SCLATER AND SALVIN'S "NOMENCLATOR AVIUM NEOTROPICALIUM"

Nomenclator Avium Neotropicalium, sive avium quæ in Regione Neotropica hucusque repertæ sunt nomina systematicè disposita adjecta sua cuique speciei patria. Accedunt generum et specierum novarum diagnoses. Auctoribus Philippo Lutley Sclater et Osberto Salvin, (Londini: sumptibus auctorum, 1873). 1 vol. fol., 164 pp.

THE naturalists whose names are attached to the present work have been for some years working together on American ornithology. Besides numerous papers and articles of greater or less importance published in the "Ibis," the "Proceedings of the Zoological Society," and elsewhere, they completed in 1869 a quarto volume of "Exotic Ornithology," containing one hundred coloured lithographic plates representing new or rare birds of South and Central America, with accompanying letterpress. These works are understood to be all written with a view to the ultimate incorporation of the results arrived at in an "Index Avium Americanarum," or complete treatise on the ornithology of Central and South America. In further progress towards this end the authors now give us a "Nomenclator" or list of the generic and specific names of the species of birds as yet ascertained by them to occur in these countries, which form the "neotropical region" of Mr. Sclater—one of the six principal regions into which he has proposed to divide the earth's surface zoologically. After the name of each species is added the "patria" or "habitat," indicating the exact locality in which the species has been observed.

The neotropical region is now well known to be the richest in the world, ornithologically speaking; the "Nomenclator" contains the names of no less than 3,565 species of birds which, as the authors have convinced themselves by personal examination, are found in it. About 2,000 of these belong to the great order Passeres, and rather more than 1,500 to the nineteen other orders of birds met with in the neotropical region. One order alone is unrepresented in South and Central America, namely, the Apteryges, which is confined to New Zealand; but on the other hand the neotropical region possesses two peculiar forms of bird-life of ordinal rank (the *Opisthocomus* and the *Tinami*) which are unknown elsewhere. Besides these, many extensive families are entirely restricted to the limits of this region; for instance, the Tanagers with 302 species, the Humming-birds with 387 species, the Dendrocolaptidæ with 217 species, and the Formicariidæ with 211 species. A few Tanagers and Humming-birds have invaded the neighbouring nearctic region (*i.e.* America north of Mexico), but the great bulk of these large groups of birds and of several other less numerous though equally distinct families, is essentially neotropical.

Nor must it be supposed that we are yet by any means fully acquainted with the riches of the neotropical region. The active ornithologists of the day are making continual additions to the long list—chiefly through the exertions of collectors in various parts of the Andean Chain, where almost every valley appears to contain distinct species of birds. At a recent meeting of the Zoological Society, twenty-four new species of birds (several belonging to new genera) were described from a single district in Peru, and Mr. Gould is constantly recording additions to the long series of humming-birds which he has so admirably monographed. Besides this, the anatomy and osteology of the greater number of exotic birds is almost utterly unknown, so that there is ample work in the neotropical region alone for many future generations of ornithologists.

The two collections upon which the "Nomenclator" has been principally based are those of Mr. Sclater and of Messrs. Salvin and Godman. The former of these contains an unrivalled series of the American species of the great order Passeres, and a set of representatives of the other higher orders, down to the end of the parrots—, altogether about 7,000 specimens. The latter collection is still larger and more general, embracing the whole series of American birds. It is especially rich in Central American forms, the owners having themselves visited several districts of the Central American Republics, and employed private collectors in other districts for the enrichment of their cabinets.

The "Nomenclator" gives us a summary of all the species represented in these two great collections, and of other species examined by the authors, but of which they have not yet succeeded in obtaining specimens.

In an appendix are added characters of nine new genera, and of thirty-one new species, founded on specimens contained in one or other of the above-mentioned collections.

OUR BOOK SHELF

The Mishmee Hills; an Account of a Journey made in an Attempt to penetrate Thibet from Assam to open new Routes for Commerce. By T. T. Cooper, F.R.G.S., Acting Political Agent at Bhamo. (London: Henry S. King and Co., 1873.)

MR. COOPER is already well known as an enterprising traveller and delightful story-teller through his "Travels of a Pioneer of Commerce in Pig-tail and Petticoats;" the present narrative is one of the most attractive published for a long time; it is one of the few books now published one feels inclined to read through at a sitting. Mr. Cooper tells his story without apparent effort, and in simple, unaffected style. For many centuries China has had the monopoly of supplying the Thibetans with tea, of which they are most extensive consumers. The Lamas of Thibet have the exclusive privilege of retailing this tea, and both they and the Chinese naturally do all in their power to prevent the possibility of any rivalry in the lucrative trade. It was on this account that Mr. Cooper was prevented from completing his intended journey from Shanghai overland to India. In the present work the author describes an attempt which he made to penetrate into Thibet from the Indian side, for the purpose of discovering whether it would not be possible to open up a way for the introduction into that country of the abundant produce of the Assam tea-plantations. He proceeded from Calcutta to Sudiya, on the north-east frontier of Assam, from which, after making all due preparations, he set out on his adventurous journey in the latter part of 1869. Notwithstanding that Mr. Cooper was accompanied by a Khamtee chief, Chowsam--a fine manly fellow—who knew the country well, and was feared and respected by the people through whose country Mr. Cooper had to pass, the latter, amid great hardships, succeeded in penetrating north-eastward along the Brahmapootra, only about 100 miles, when, through the determined opposition of the Thibetan officials, he was compelled to turn back. No doubt Mr. Cooper failed in accomplishing the object on which he had set his heart, but his journey has been the means of giving to the world a book full of interesting information about the peoples and the countries where he sojourned, both in Assam and the districts just beyond its north-eastern frontier. The book contains a great deal of information on the present and past condition of Assam and the Assamese, and much information on the state of the tea-cultivation in that country. Mr. Cooper is particularly observant of men and manners, and most readers will find in his book a great deal that is quite new concerning the small tribes that live along the route by which he attempted, in the interest of commerce, to enter Thibet; his description of the Khamtees is especially interesting. Mr. Cooper does not pretend to give any scientific record of the natural history of the country through which he passed, though he makes occasional observations that may interest naturalists. The following description of the land-leeches which pestered him during his journey, seems to us particularly interesting:—

"Of all the hardships and unpleasant sensations experienced in the Assam jungle none have left a more disagreeable recollection than the attacks of land-leeches. Often, on sitting down, I could count a dozen of these little animals hurrying from all directions to their prey. In length they are about an inch, while their thickness does not exceed that of an ordinary sewing needle. Their mode of progression is curious in the extreme. Fixing one extremity by means of its bell-shaped sucker firmly on a leaf or on the ground, the leech curves itself into an arch, the other end is then advanced till the creature resembles a loop, again to expand into an arch, but the movement is quicker than words can describe it; the rapidity with which they thus progress along is quite startling. As they occasionally rear themselves perpendicularly and sway about from side to side, taking a survey round them in quest of prey, the observer cannot fail to conceive a dread of the bloodthirsty little creatures. They exercised quite a fascination over me. I could never resist watching them whenever I took a seat. Their power of scent was evidently keen. At first they would hold themselves erect, then suddenly, as though they had just discovered my whereabouts, they would throw themselves forward and with quick eager strides make towards my unfortunate body, and it was a long time before I could restrain a shudder at their approach, but use does wonders, and at last I used to flip them off my clothes and hands, Khamtee fashion, with great indifference. There are several species of leeches in Assam, but I have only come in contact with three kinds: the common brown one, just described; the red, or hill leech, which is larger than the former and of a light red colour, inflicting a venomous, though not dangerous, bite; and the hair-leech, so called by the Khamtees from its great length and extreme tenuity. This last description of leech lies in wait in the grass, and as animals feed it enters the nostrils and fixes itself firmly in the interior, where it takes up permanent quarters, causing the poor beasts great irritation. It seems to inflict itself entirely on animals, which is fortunate, or man would suffer greatly from this scourge of the jungle."

Mr. Cooper has done well in telling the world the story of his travels.

Transactions of the Albany Institute, vol. vii. (Albany, U.S., 1872.)

THIS institution is one of the oldest of its kind in America, having been originally founded upwards of eighty years ago, just after the conclusion of the American War of Independence. At present it is one of the most comprehensive and active of the American societies, its sphere of work embracing all departments of literature and science. In an eloquent annual address, which is the first paper in this volume, Orlando Meads, one of the oldest members of the Institute, sketches its history, and gives reminiscences of some of the most eminent men who have been connected with the Society, including several who have left their mark on the country. A characteristic feature of this volume is the reports of what has been done during the year, both in America and Europe, in the various divisions of science and literature, the institution being divided into three departments—Physical Sciences and the Arts, Natural History, and History and General Literature, and these again into a number of classes. Thus we have in the present volume, reports on botany, zoology, chemistry, and general literature. Of the papers in this volume we may notice one On Nitro-glycerine, as used in the construction of the Hoosac Tunnel, by Prof. G. M. Mowbray. The author traces the history of the dangerous article, gives an account of his own investigations regarding it, and describes the method in which it was used in boring the Hoosac tunnel.—On certain new Phenomena in Chemistry, by Verplanck Colvin, describes some very remarkable experiments in amalgamation made by the author. From Newton to Kirchoff, by Dr. L. C. Cooley, traces in an interesting way the progress of research on Light during the period indicated; and in Researches in the Theory and Calculus of Operations, by J. A. Paterson, we have a most elaborate and intricate investigation on the theory of the actions of various forces of Nature. Mr. C. H. Peck contributes a Synopsis of New York Uncinulæ.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Microscopic Examination of Air

IN support of the conclusions arrived at by Mr. Douglas Cunningham, of Calcutta, in his "Microscopic Examinations of Air" (NATURE, vol. ix. p. 330), and in illustration of the method which he employed, perhaps I may be allowed to describe some observations of the same kind which I made three years ago but have not had leisure to continue or prepare for publication.

A sentence in Dr. Parkes' "Manual of Hygiene," alluding to the importance of minute examination of the air, turned my thoughts in that direction. The instrument which I constructed for the purpose was contrived after the manner of a weathercock, presenting the wide mouth of a funnel to the wind, while the tube of the funnel was bent upwards and had an orifice about 1-20th of an inch in diameter. Close above this orifice was placed a glass slide, held by springs and bearing a drop of glycerine on its lower surface. The tube and glass slide were protected by a roof and two cheek-boards, which formed the vane of the weather-cock. The glass slide was so placed that the current of air issuing from the narrow orifice of the bent tube, under pressure of the wind on the wide mouth of the funnel, impinged on the centre of the drop of glycerine, and a large proportion of any solid matters carried by the air was caught on the glycerine. After a day or two, according to the weather, the slide was removed, a thin disk of glass was placed on the glycerine-drop, and the contents were then examined under the microscope, a duplicate slide being left in the aeroscope for the next observation.

This instrument depended for its function on the wind. If there was no wind, there was no current through the tube, and nothing was caught on the glycerine; but in general there was wind enough, and the captures were ample, often embarrassing by their multitude. The observations were mostly made in the neighbourhood of London, at the Greenwich Observatory. The nature of the captures varied according to the direction and velocity of the wind, the state of the weather, and the season of the year. A north-west wind, blowing over London, brought soot and globules of coal-tar, textile fibres, nondescript *débris*, a few vegetable spores, now and then an epithelial scale or two, always a number of half-cooked starch-grains (identified by their reaction with iodine and by traces of concentric lamination giving a black cross with the polariscope), and sometimes microscopic bread-crumbs (half-cooked starch-grains in meshes of gluten). The starch-grains were the most constant capture of all, in all seasons and for all directions of wind. They seem to be very durable. (If I remember rightly, M. Pouchet found starch-grains in all specimens of dust, even the most ancient, obtained from the neighbourhood of human dwellings.) A southerly wind, blowing from the country, brought a great variety of vegetable spores and pollen-grains and *débris*, with a smaller proportion of matters characteristic of town air. The size and quantity of the captures depended mainly on the velocity of the wind. Once or twice a strong wind swept a living acarus, or an entomostracan, or the shell of a diatom into the glycerine. In dry windy weather a quantity of siliceous sand was caught, which gave trouble by tilting the disk when in preparation for the microscope, and the larger grains had to be removed with the point of a needle.

The most interesting variation in the character of the organic captures was that which depended on the season of the year. In January and February scarcely anything was found (besides *débris* and inorganic matter) but a few fragments of mycelium of some fungus; but with the first fine weather in March the glycerine began to yield good returns. Spherical grains of poplar pollen were caught in large numbers, thirty or forty in a single drop, though the nearest poplar tree was a quarter of a mile distant. These were soon followed by the triangular pollen of birch and hazel—trees depending, like poplar, on the agency of the wind for fertilisation. From this time onward, through spring and summer, a great number and variety of pollen grains were caught. Cryptogamic cells increased in number through the summer, and reached their maximum in the autumn, when brown septate spores and others of various kinds, which my imperfect knowledge did not enable me to identify, appeared in abundance. If left for some days, they began to germinate. Towards winter their number diminished, the latest being minute dark brown oval spores of some species of agaricus (?). The winter months were comparatively barren.

I did not find any Bacteria, but there were numbers of excessively minute particles, of which I could not tell the nature. Once, after leaving the aeroscope for several days, I found the glycerine swarming with a minute *torula* which had evidently multiplied in that pabulum. In fact the glycerine was fermenting.

Among these facts, the only one which seems to have any bearing on the question of the propagation of infectious diseases is the great prevalence of cryptogamic spores in the air in autumn, when those diseases are especially rife.

To avoid fallacy in the results obtained, I used to place two drops of glycerine on the same slide, but only directed the air-current against one of them; both were examined under the microscope, and the difference credited to the air. By using glycerine that had been boiled with carmine, many of the organic captures were made more distinct: the nuclei of epithelial scales and of many other cells were brightly stained by the carmine. Glycerine had the disadvantage of absorbing moisture in damp weather and swelling to an inconvenient bulk. At such times I used oil instead, with good effect. My plan of examination was to sweep the whole disk in successive parallel zones, by the aid of mechanical stage-movement, and make record of every organic body that could be recognised. Such as I had not seen before were sketched with pen and ink and coloured chalk in a book devoted to that purpose.

It will be seen that my observations entirely support Mr Cunningham's, as to the abundant presence of living spores in the air. I was satisfied that this branch of research, in the hands of one thoroughly familiar with these microscopic forms, would lead to results of great interest, and I heartily congratulate Mr. Cunningham on the valuable work which he has produced.

HUBERT AIRY

Animal Locomotion

MR. WALLACE'S last letter seems to call for a word of explanation from me. I did not refer to the up stroke of the bird's wing because this was not the point in dispute. But in reply to Mr. Wallace's latest stricture—that I appear "to ignore the great downward reaction, added to gravitation, during every up stroke"—I would say (1) that the downward reaction is not great, (*a*) because, as Mr. Wallace has himself observed, of the valvular action of the feathers; (*b*) because of the convex form of the upper surface of the wing; and (*c*) in some cases, because the wing is less expanded in the up stroke. (2) As to the effect of gravitation, this was already allowed for in determining the resultant motion consequent on the down stroke, and must not be reckoned twice. Just as with an arrow shot from a bow, so with the bird; the motion consequent on the down stroke lasts long enough for the wings to be raised before it is spent. Mr. Wallace is certainly right in saying that the down stroke should counteract the downward reaction of the up stroke, but this downward reaction being slight cannot require "a highly-inclined upward motion," and what is more, it cannot require that the under surface of the wing should be directed forwards as Dr. Pettigrew asserts.

Again, I do not say the movement of the wing as a whole is downward and backward, but that the action of its surface is in that direction. The Duke of Argyll is no doubt correct in maintaining that the wing as a whole moves in a perpendicular line, or perhaps with a slight forward overlap.

I cordially agree with Mr. Wallace that the matter is not to be settled by "discussing theoretically, but by observation and experiment;" still the elementary principles of mechanics may surely be heard in evidence without disadvantage even at the outset of the inquiry.

JAMES WARD

Trinity College, Cambridge, March 30

Rudimentary Organs

IN a former communication (NATURE, vol. ix. p. 361) I promised to advance what seems to me a probable cause—additional to those already known—of the reduction of useless structures. As before stated, it was suggested to me by the penetrating theory proposed by Mr. Darwin (NATURE, vol. viii. pp. 432 and 505), to which, indeed, it is but a supplement. Epitomising Mr. Darwin's conception as to the dwarfing influence of impoverished conditions, progressively reducing the average size of a useless structure, by means of free intercrossing; the present cause may be defined as the mere cessation of the selective influence from changed condition of life.

Suppose a structure to have been raised by natural selection from 0 to average size 100, and then to have become wholly useless. The selective influence would now not only be withdrawn, but reversed; for, through Economy of Growth—understanding by this term both the direct and the indirect influence of natural selection[*]—it would rigidly eliminate the variations 101, 102, 103, &c., and favour the variations 99, 98, 97, &c. For the sake of definition we shall neglect the influence of Economy acting below 100, and so isolate the effects due to the mere withdrawal of Selection. By the condition of our assumption, all variations above 100 are eliminated, while below 100 indiscrimi-

[*] See former communication, NATURE, vol. ix. p. 361.

nate variation is permitted. Thus, the selective premium upon variation 99 being no greater than that upon 98, 98 would have as good a chance of leaving offspring (which would inherit and transmit this variation) as would 99: similarly, 97 would have as good a chance as 98, and so on. Now there is thus a much greater chance of variations being perpetuated at or below 99, than at or above 100; for at 100 the hard line of Selection (or Economy) is fixed, while there is no corresponding line below 100. The consequence of free intercrossing would therefore be to reduce the average from 100 to 99. Simultaneously, however, with this reducing process, other variations would be surviving below 99, in greater numbers than above 99; consequently the average would next become reduced to 98. There would thus be "two operations going on side by side—the one ever destroying the symmetry of distribution" round the average, "and the other ever tending to restore it." It is evident, however, that the more the average is reduced by this process of indiscriminate variation, the less chance there remains for its further reduction. When, for instance, it falls to 90, there are (numerically, though not actually, because of Inheritance) 89 to 9 in favour of diminution; but, when it falls to 80, there are only 79 to 19 in such favour. Thus (theoretically) the average would continue to diminish at a slower and slower rate, until it comes to 50, where, the chances in favour of increase and of diminution being equal, it would remain stationary.

Having thus, for the sake of clearness, considered this principle apart, let us now observe the effect of superadding to it the influence of the Economy of Growth—a principle with which its action must always be associated. Briefly, as this influence would be that of continually favouring the variations on the side of diminution, the effect of its presence would be that of continuously preventing the average from becoming fixed at 50, 40, 30, &c. In other words, the "hard line of Selection," which was originally placed at 100, would now become progressively lowered through 90, 80, 70, &c.; always allowing indiscriminate variation below the barrier, but never above it.

It will be understood that by "cessation of selection from changed conditions of life" I mean a change *of any kind* which renders the affected organ superfluous. Take, for example, the exact converse of Mr. George Darwin's illustration, by supposing a herd of cattle to migrate from a small tract of poor pasture to a large tract of rich. Segregation would ensue, the law of battle would become less severe, while variation would be promoted in a cumulative manner by the increase of food. The young males with shorter horns would thus have as good a chance of leaving progeny as "their longer-horned brothers," and the average length would gradually diminish as in the other case. Of course, as the predisposing cause of impoverished nutrition would now be absent, the reducing process would take place at a slower rate. Moreover, it is to be remarked that this principle differs, in an important particular from that enunciated by Mr. Darwin, in that it could never reduce an organ much below the point at which the Economy of Growth (together with Disuse) ceases to act. For, returning to our numerical illustration, suppose this point to be 6, the average would eventually become fixed at 3.

That the principle thus explained has a real existence we may safely conclude, theoretical considerations apart, from the analogy afforded by our domestic races; for nothing is more certain to breeders than the fact that neglect causes degeneration, even though the strain be kept isolated. It will be observed that, if this principle has a real existence, it is of considerable importance, theoretically, since it must act, to a greater or less extent, in all cases where Disuse and the Economy of Growth are in operation; and although in the initial stages of reduction, when the purchase, so to speak, of the last-named principle is great, its influence would be comparatively trivial, this influence would be more and more felt the smaller the organ became—*i.e.*, the nearer the point at which the Economy of Growth ceases to act. The Cessation of Selection should therefore be regarded as a reducing cause, which co-operates with other reducing causes in all cases, and which is of special importance as an accelerating agent when the influence of the latter becomes feeble.

George J. Romanes

Lakes with two Outfalls

On June 22, 1863, the late Captain Speke published his map, giving (on native authority) *four* outlets from Lake Victoria Nyanza, converging to *one* valley or water-flow—the Nile.

On June 27 and on July 20 I wrote to the *Athenæum*:—"I think that this native information will prove to be erroneous;" that I thought "that no lake can have more than one outlet;" and I added, "May I lay the question as to the matter of fact before the readers of the *Athenæum*?" In reply, the Black Loch in Dumfriesshire was stated to have *two* outlets to *two* distinct valleys or water-flows—one to the Nith, the other to the Ayr. The Loch, however, has but one outlet, and that *artificial*. The water-parting has been cut through by man—a mill-lead made to Lord Bute's Borland mill, and the one outlet is an iron sluice in a stone dam. All this is beautifully shown in Sir Henry James's admirable 25-inch Ordnance Maps.

Dr. Bryce ("Geology of Arran," 3rd edition, p. 3) says that Loch-an-Davie has two outlets to two different valleys. It has, however, but one outlet, to the south—to Glen Iorsa, as I stated in the *Athenæum*, July 22, 1865. The new inch Ordnance Map of Arran gives one outlet, but unfortunately to the north, instead of to the south. I will not refer to my letter on the two outlets to two valleys from the Norwegian Lesjeskaugen Lake, which you did me the honour to publish last September, and with which Prof. Stanley Jevons agreed. But I quote the above cases to show that even the highest authorities make mistakes as regards lakes and their outlets. I cannot, however, suppose any mistake in Prof. Bell's account of the two outlets to two valleys from Shoal Lake, published in Nature, vol. ix. p. 363, by Prof. Dawson. I would then, in deference to these authorities, modify my dictum by saying, that if by a rare possibility a lake may be found to exist on a water-parting having at opposite ends two outlets to two different valleys, I should still doubt the possibility of a lake at its one lower end having a multiplicity of outlets converging to one valley or water-flow, as in the case of the Victoria Nyanza. And this owing to the extreme improbability that the erosion at each outlet should continue at precisely the same rate.

The outlet of every lake in the wide, wide world is always being lowered from erosion, as are valleys themselves. Valleys exist only in the dissolution of hills. They are mere water-flows. They are the perpetually changing effects of atmospheric disintegration, and the erosion of rain and rivers, and consequently every water-parting is a valley-parting.

Alresford, March 14 George Greenwood

A Beech pierced by a Thorn Plant

On the road from this to Belfast there is a thorn hedge with beech trees at intervals, and thorn plants have grown right through the middle of the trunks of two of the beeches. I do not know whether this is sufficiently uncommon to be worth mentioning in Nature. Joseph John Murphy

Old Forge, Dunmurry, co. Antrim

KINETIC THEORY OF THE DISSIPATION OF ENERGY

In abstract dynamics an instantaneous reversal of the motion of every moving particle of a system causes the system to move backwards, each particle of it along its old path, and at the same speed as before when again in the same position—that is to say, in mathematical language, any solution remains a solution when t is changed into $-t$. In physical dynamics, this simple and perfect reversibility fails on account of forces depending on friction of solids; imperfect fluidity of fluids; imperfect elasticity of solids; inequalities of temperature and consequent conduction of heat produced by stresses in solids and fluids; imperfect magnetic retentiveness; residual electric polarisation of dielectrics; generation of heat by electric currents induced by motion; diffusion of fluids, solution of solids in fluids, and other chemical changes; and absorption of radiant heat and light. Consideration of these agencies in connection with the all-pervading law of the conservation of energy proved for them by Joule, led me twenty-three years ago to the theory of the dissipation of energy, which I communicated first to the Royal Society of Edinburgh in 1852, in a paper entitled

"On a Universal Tendency in Nature to the Dissipation of Mechanical Energy.

The essence of Joule's discovery is the subjection of physical phenomena to dynamical law. If, then, the motion of every particle of matter in the universe were precisely reversed at any instant, the course of nature would be simply reversed for ever after. The bursting bubble of foam at the foot of a waterfall would reunite and descend into the water: the thermal motions would reconcentrate their energy and throw the mass up the fall in drops reforming into a close column of ascending water. Heat which had been generated by the friction of solids and dissipated by conduction, and radiation with absorption, would come again to the place of contact and throw the moving body back against the force to which it had previously yielded. Boulders would recover from the mud the materials required to rebuild them into their previous jagged forms, and would become reunited to the mountain peak from which they had formerly broken away. And if also the materialistic hypothesis of life were true, living creatures would grow backwards, with conscious knowledge of the future, but no memory of the past, and would become again unborn. But the real phenomena of life infinitely transcend human science, and speculation regarding consequences of their imagined reversal is utterly unprofitable. Far otherwise, however, is it in respect to the reversal of the motions of matter uninfluenced by life, a very elementary consideration of which leads to the full explanation of the theory of dissipation of energy.

To take one of the simplest cases of the dissipation of energy, the conduction of heat through a solid—consider a bar of metal warmer at one end than the other and left to itself. To avoid all needless complication, of taking loss or gain of heat into account, imagine the bar to be varnished with a substance impermeable to heat. For the sake of definiteness, imagine the bar to be first given with one half of it at one uniform temperature, and the other half of it at another uniform temperature. Instantly a diffusing of heat commences, and the distribution of temperature becomes continuously less and less unequal, tending to perfect uniformity, but never in any finite time attaining perfectly to this ultimate condition. This process of diffusion could be perfectly prevented by an army of Maxwell's "intelligent demons"* stationed at the surface, or interface as we may call it with Prof. James Thomson, separating the hot from the cold part of the bar. To see precisely how this is to be done, consider rather a gas than a solid, because we have much knowledge regarding the molecular motions of a gas, and little or no knowledge of the molecular motions of a solid. Take a jar with the lower half occupied by cold air or gas, and the upper half occupied with air or gas of the same kind, but at a higher temperature, and let the mouth of the jar be closed by an air-tight lid. If the containing vessel were perfectly impermeable to heat, the diffusion of heat would follow the same law in the gas as in the solid, though in the gas the diffusion of heat takes place chiefly by the diffusion of molecules, each taking its energy with it, and only to a small proportion of its whole amount by the interchange of energy between molecule and molecule; whereas in the solid there is little or no diffusion of substance, and the diffusion of heat takes place entirely, or almost entirely, through the communication of energy from one molecule to another. Fourier's exquisite mathematical analysis expresses perfectly the statistics of the process of diffusion in each case, whether it be "conduction of heat," as Fourier and his followers have called it, or the diffusion of substance in fluid masses (gaseous or liquid) which Fick showed to be subject to Fourier's formulæ. Now, suppose the weapon of the ideal army to be a club, or, as it were, a molecular cricket-bat; and suppose for convenience the mass of each demon with his weapon to be several times greater than that of a molecule. Every time he strikes a molecule he is to send it away with the same energy as it had immediately before. Each demon is to keep as nearly as possible to a certain station, making only such excursions from it as the execution of his orders requires. He is to experience no forces except such as result from collisions with molecules, and mutual forces between parts of his own mass, including his weapon: thus his voluntary movements cannot influence the position of his centre of gravity, otherwise than by producing collision with molecules.

The whole interface between hot and cold is to be divided into small areas, each allotted to a single demon. The duty of each demon is to guard his allotment, turning molecules back or allowing them to pass through from either side, according to certain definite orders. First, let the orders be to allow no molecules to pass from either side. The effect will be the same as if the interface were stopped by a barrier impermeable to matter and to heat. The pressure of the gas being, by hypothesis, equal in the hot and cold parts, the resultant momentum taken by each demon from any considerable number of molecules will be zero; and therefore he may so time his strokes that he shall never move to any considerable distance from his station. Now, instead of stopping and turning all the molecules from crossing his allotted area, let each demon permit a hundred molecules chosen arbitrarily to cross it from the hot side; and the same number of molecules, chosen so as to have the same entire amount of energy and the same resultant momentum, to cross the other way from the cold side. Let this be done over and over again within certain small equal consecutive intervals of time, with care that if the specified balance of energy and momentum is not exactly fulfilled in respect to each successive hundred molecules crossing each way, the error will be carried forward, and as nearly as may be corrected, in respect to the next hundred. Thus, a certain perfectly regular diffusion of the gas both ways across the interface goes on, while the original different temperatures on the two sides of the interface are maintained without change.

Suppose, now, that in the original condition the temperature and pressure of the gas are each equal throughout the vessel, and let it be required to disequalise the temperature but to leave the pressure the same in any two portions *A* and *B* of the whole space. Station the army on the interface as previously described. Let the orders now be that each demon is to stop all molecules from crossing his area in either direction except 100 coming from *A*, arbitrarily chosen to be let pass into *B*, and a greater number, having among them less energy but equal momentum, to cross from *B* to *A*. Let this be repeated over and over again. The temperature in *A* will be continually diminished and the number of molecules in it continually increased, until there are not in *B* enough of molecules with small enough velocities to fulfil the condition with reference to permission to pass from *B* to *A*. If after that no molecule be allowed to pass the interface in either direction, the final condition will be very great condensation and very low temperature in *A*; rarefaction and very high temperature in *B*; and equal temperature in *A* and *B*. The process of disequalisation of temperature and density might be stopped at any time by changing the orders to those previously specified (2), and so permitting a certain degree of diffusion each way across the interface while maintaining a certain uniform difference of temperatures with equality of pressure on the two sides.

If no selective influence, such as that of the ideal "demon," guides individual molecules, the average result of their free motions and collisions must be to equalise the distribution of energy among them in the gross; and after a sufficiently long time from the supposed initial arrangement the difference of energy in any two equal volumes, each containing a very great number of molecules, must bear a very small proportion to the whole amount in either; or, more strictly speaking, the probability of the difference of energy exceeding any stated finite proportion of the whole energy in either is very small. Suppose now the temperature to have become thus very approximately equalised at a certain time from the beginning, and let the motion of every particle become instantaneously reversed. Each molecule will retrace its former path, and at the end of a second interval of time, equal to the former, every molecule will be in the same position, and moving with the same velocity, as at the beginning; so that the given initial unequal distribution of temperature will again be found, with only the difference that each particle is moving in the direction reverse to that of its initial motion. This difference will not prevent an instantaneous subsequent commencement of equalisation, which, with entirely different paths for the individual molecules, will go on in the average according to the same law as that which took place immediately after the system was first left to itself.

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* The definition of a "demon," according to the use of this word by Maxwell, is an intelligent being endowed with free will, and fine enough tactile and perceptive organisation to give him the faculty of observing and influencing individual molecules of matter

By merely looking on crowds of molecules, and reckoning their energy in the gross, we could not discover that in the very special case we have just considered the progress was towards a succession of states in which the distribution of energy deviates more and more from uniformity up to a certain time. The number of molecules being finite, it is clear that small finite deviations from absolute precision in the reversal we have supposed would not obviate the resulting disequalisation of the distribution of energy. But the greater the number of molecules, the shorter will be the time during which the disequalising will continue; and it is only when we regard the number of molecules as practically infinite that we can regard spontaneous disequalisation as practically impossible. And, in point of fact, if any finite number of perfectly elastic molecules, however great, be given in motion in the interior of a perfectly rigid vessel, and be left for a sufficiently long time undisturbed except by mutual impacts and collisions against the sides of the containing vessel, it must happen over and over again that (for example) something more than nine-tenths of the whole energy shall be in one half of the vessel, and less than one-tenth of the whole energy in the other half But if the number of molecules be very great, this will happen enormously less frequently than that something more than 6-10ths shall be in one half, and something less than 4-10ths in the other. Taking as unit of time the average interval of free motion between consecutive collisions, it is easily seen that the probability of there being something more than any stated percentage of excess above the half of the energy in one half of the vessel during the unit of time, from a stated instant, is smaller the greater the dimensions of the vessel and the greater the stated percentage. It is a strange but nevertheless a true conception of the old well-known law of the conduction of heat to say that it is very improbable that in the course of 1,000 years one half the bar of iron shall of itself become warmer by a degree than the other half; and that the probability of this happening before 1,000,000 years pass is 1,000 times as great as that it will happen in the course of 1,000 years, and that it certainly will happen in the course of some very long time. But let it be remembered that we have supposed the bar to be covered with an impermeable varnish. Do away with this impossible ideal, and believe the number of molecules in the universe to be infinite; then we may say one half of the bar will never become warmer than the other, except by the agency of external sources of heat or cold. This one instance suffices to explain the philosophy of the foundation on which the theory of the dissipation of energy rests.

Take however another case in which the probability may be readily calculated. Let a hermetically-sealed glass jar of air contain 2,000,000,000,000 molecules of oxygen, and 8,000,000,000,000 molecules of nitrogen. If examined any time in the infinitely distant future, what is the number of chances against one that all the molecules of oxygen and none of nitrogen shall be found in one stated part of the vessel equal in volume to 1-5th of the whole? The number expressing the answer in the Arabic notation has about 2,173,220,000,000 of places of whole numbers. On the other hand the chance against there being exactly 2-10ths of the whole number of particles of nitrogen, and at the same time exactly 2-10ths of the whole number of particles of oxygen in the first specified part of the vessel is only 4021×10^9 to 1.

[*Appendix.—Calculation of Probability respecting Diffusion of Gases.*]

For simplicity I suppose the sphere of action of each molecule to be infinitely small in comparison with its average distance from its nearest neighbour: thus, the sum of the volumes of the spheres of action of all the molecules will be infinitely small in proportion to the whole volume of the containing vessel. For brevity, space external to the sphere of action of every molecule will be called free space: and a molecule will be said to be in free space at any time when its sphere of action is wholly in free space; that is to say, when its sphere of action does not overlap the sphere of action of any other molecule. Let A, B denote any two particular portions of the whole containing vessel, and let a, b be the volumes of those portions. The chance that at any instant one individual molecule of whichever gas shall be in A is $\frac{a}{a+b}$, however many or few other molecules there may be in A at the same time; because its chances of being in any specified portions of free space are proportional to their volumes; and, according to our supposition, even if all the other molecules were in A, the volume of free space in it would not be sensibly diminished by their presence. The chance that of n molecules in the whole space there shall be i stated individuals in A, and that the other $n-i$ molecules shall be at the same time in B, is

$$\left(\frac{a}{a+b}\right)^i \left(\frac{b}{a+b}\right)^{n-i}, \text{ or } \frac{a^i b^{n-i}}{(a+b)^n}$$

Hence the probability of the number of molecules in A being exactly i, and in B exactly $n-i$, irrespectively of individuals, is a fraction having for denominator $(a+b)^n$, and for numerator the term involving $a^i b^{n-i}$ in the expansion of this binomial; that is to say it is—

$$\frac{n(n-1)\;.\;.\;.\;.\;(n-i+1)}{1.2\;\;.\;.\;.\;.\;\;i}\left(\frac{a}{a+b}\right)^i \left(\frac{b}{a+b}\right)^{n-i}$$

If we call this T_i we have

$$T_{i+1} = \frac{n-i}{i+1}\,\frac{a}{b}\;T_{i+1}$$

Hence T_i is the greatest term if i is the smallest integer which makes

$$\frac{n-i}{i+1} < \frac{b}{a}$$

this is to say, if i is the smallest integer which exceeds

$$n\frac{a}{a+b} - \frac{b}{a+b}$$

Hence if a and b are commensurable the greatest term is that for which

$$i = n\,\frac{a}{a+b}$$

To apply these results to the cases considered in the preceding article, put in the first place

$$n = 2 \times 10^{12}$$

this being the number of particles of oxygen; and let $i = n$. Thus, for the probability that all the particles of oxygen shall be in A, we find

$$\left(\frac{a}{a+b}\right)^{8 \times 10^{12}}$$

Similarly, for the probability that all the particles of nitrogen are in the space B, we find

$$\left(\frac{b}{a+b}\right)^{2 \times 10^{12}}$$

Hence the probability that all the oxygen is in A and all the nitrogen in B is

$$\left(\frac{a}{a+b}\right)^{2 \times 10^{12}} \times \left(\frac{b}{a \times b}\right)^{8 \times 10^{12}}$$

Now by hypothesis

$$\frac{a}{a+b} = \frac{2}{10}$$

and therefore

$$\frac{b}{a+b} = \frac{8}{10}$$

hence the required probability is

$$\frac{2^{26 \times 10^{12}}}{10^{10^{13}}}$$

Call this $\frac{1}{N}$, and let log denote common logarithm. We have $\log N = 10^{13} - 26 \times 10^{12} \times \log. 2 = (10 - 26 \log. 2) \times 10^{12} = 2173220 \times 10^{6}$. This is equivalent to the result stated in the text above. The logarithm of so great a number, unless given to more than thirteen significant places, cannot indicate more than the number of places of whole numbers in the answer to the proposed question, expressed according to the Arabic notation.

The calculation of T_i when i and $n-i$ are very large numbers is practicable by Stirling's Theorem, according to which we have approximately

$$1.2 \; .\;.\;.\;.\; i = i^{i+\frac{1}{2}} \epsilon^{-i} \sqrt{2\pi}$$

and therefore

$$\frac{n(n-1) \; .\;.\;.\;.\; (n-i+1)}{1.2 \;\; .\;.\;.\;. \;\; i} = \frac{n^{n+\frac{1}{2}}}{\sqrt{2\pi}\, i\,(i+\frac{1}{2})(n-i)^{n}}$$

Hence for the case

$$i = n\frac{a}{a+b}$$

which, according to the preceding formulæ, gives T_i its greatest value, we have

$$T_i = \frac{1}{\sqrt{2\pi n e f}}$$

where

$$e = \frac{a}{a+b} \text{ and } f = \frac{b}{a+b}$$

Thus, for example, let $n = 2 \times 10^{12}$;

$$e = \cdot 2,\; f = \cdot 8$$

we have

$$T_i = \frac{1}{800000\sqrt{\pi}} = \frac{1}{1418000}$$

This expresses the chance of there being 4×10^{11} molecules of oxygen in A, and 16×10^{11} in B. Just half this fraction expresses the probability that the molecules of nitrogen are distributed in exactly the same proportion between A and B, because the number of molecules of nitrogen is four times greater than of oxygen.

If n denote the molecules of one gas, and n' that of the molecules of another, the probability that each shall be distributed between A and B in the exact proportion of the volume is

$$\frac{1}{2\pi e f\sqrt{n n'}}$$

The value for the supposed case of oxygen and nitrogen is

$$\frac{1}{2\pi \times \cdot 16 \times 4 \times 10^{11}} = \frac{1}{4021 \times 10^{8}}$$

which is the result stated at the conclusion of the text above.

WILLIAM THOMSON

LIVINGSTONE'S WORK IN AFRICA

THE daily papers have published some extracts from a letter written from Lake Bangweolo, of the late Dr. Livingstone to Mr. H. M. Stanley, which have been kindly furnished by the enterprising proprietor of the *New York Herald*; we reproduce here so much of the letter as bears on the geographical work done by Livingstone.

"The Chambezi was crossed long ago by the Portuguese, who have thus the merit of its discovery in modern times. The similarity of names led to its being put down in maps as 'Zambesi' (eastern branch) and I rather stupidly took the error as having some sort of authority. Hence my first crossing it was as fruitless as that of the Portuguese. It took me twenty-two months to eliminate this error.

"The Cazembe who was lately killed was the first who gave me a hint that Chambezi was one of a chain of rivers and lakes which probably forms the Nile; but he did it in rather a bantering style that led me to go back to the head waters again and see that it was not the mere 'chaff' of a mighty potentate. There is Omar Island in the middle of Bangueldo, with 183° of sea horizon around. The natives, slowly drawing the hand around, said—'That is Chambezi flowing round all this space and forming Bangweolo before it winds round that headland and changes its name to Luapula.' That was the moment of discovery and not the mere crossing of a small river.

"The late Cazembe I found sensible and friendly. His empire has succumbed before a very small force of Arab slaves and Banyamwezi. Pereira, the first Portuguese who visited the Cazembe eighty years ago, said that he had 30,000 trained soldiers, sacrificed twenty human victims every day, and that the streets of his capital were watered daily. I thought that my late friend had 30,0(00), diminished by two 00's, and sacrificed five or six pots of pombe daily; but this may have been only a court scandal—the streets of his village were not made. So I was reminded of the famous couplet about the Scotch roads:—

> "'If you had seen these roads before they were made,
> You would lift up your hands and bless General Wade.'

"I have been the unfortunate means of demolishing two empires in Portuguese geography—the Cazembes and that of the Emperor Monomotapa. I found the last about ten days above Tette. He had too few men to make the show Cazembe did, but I learnt from some decent motherly-looking women attached to his Court Zembere (?) that he had 100 wives. I have wondered ever since and have been nearly dumfoundered with the idea of what a nuisance a man with 100 wives in England would be. It is awful to contemplate, and might be chosen as a theme for a Young Men's Debating Society. I wish someone would visit Mtesa, or Uganda, without Bombay as an interpreter. He (Bombay) is by no means a sound author. The King of Dahomey suffered eclipse after a common-sense visit, and we seldom hear any more of his atrocities. The mightiest African potentate and the most dreadful cruelties told of Africans owe a vast deal to the teller.

"You and I passed the islet Kasenge, where African mothers were said to sell their infants for a loin-cloth each. This story was made to fit into another nice little story of 'a mother bear' that refused to leave her young. A child that cuts its upper front teeth before the under is dreaded as unlucky and likely to bring death into the family. It is called an Arab child, and the first Arab who passes is asked to take it. I never saw a case, nor have the Arabs I have asked seen one either, but they have heard of its occurrence. The Kasenge story is, therefore, exactly like that of the Frenchman who asserted that the English were so fond of hanging themselves in November you might see them swinging on trees along the road. He may have seen one; I never did. English and American mothers have been guilty of deserting infants; but who would turn up the whites of his eyes and say, as our mothers at Kasenge did, these people are no better than, or not so good as, she-bears?

"This lake, so far as I have seen it, is surrounded by an extremely flat country, though all 4,000 ft. above the level of the sea. When first discovered I was without paper, but borrowed a little from an Arab, and sent a short account home. I had so much trouble from attendants that I took only the barest necessaries. Yet no sooner was the discovery announced at the coast than the official description was forthwith sent to the Bombay Government, that 'the lake is like Nyassa, Tanganyika, and the Albert Nyanza, overhung by high mountain slopes, which slope down to great plains, which, during the rainy season, become flooded, so that caravans march for days through water knee-deep seeking for higher ground on which to pass the night.'

"The only mountain slopes are ant-hills, some of them 20 ft. high. They could scarcely be called high unless thought of as being built on the top of the 4,000 ft. These statements are equally opposed to the truth, as the Cazembe town is built on the banks of the Luapula.

"People having a crochet for map making traced every step of the Portuguese slaving expeditions to Cazembe, and built the village in latitude 8° 43′ South—that is, in deep water, near the north end of Lake Moero, and over 50 miles from Luapula. I found it in latitude 9° 37′ South, and on the banks of a lagoon or loch, having no connection with Luapula, which river, however, falls six or seven miles west of the village of Moero.

"Now it is very unpleasant for me to expose any of these misstatements and so appear contradictious. But what am I to do? I was consulted by Sir Roderick Murchison as to this present expedition, and recommended the writer of the above as a leader. Sir Roderick afterwards told me that the offer was declined unless a good salary and a good position to fall back upon were added, as Speke and Grant had, on their pay and commission. He then urged the leadership on myself as soon as the work on which I was engaged should be published. My good, kind-hearted friend added, in a sort of pathetic strain, 'You will be the real discoverer of the source of the Nile.' I don't wish to boast of my good deeds, but I need not forget them. . . ."

SOUNDINGS IN THE PACIFIC

RECENT explorations in the Pacific Ocean indicate that its bed is singularly level. The soundings of the U.S. steamer *Tuscarora*, Capt. George T. Belknap, between Cape Flattery and Oonalaska, were described in Nature, vol. viii. p. 150. Upon the conclusion of that cruise, which included also soundings from Cape Flattery to San Francisco, a month was spent in the latter harbour, and on December 5 a survey was begun between that port and San Diego on the same coast, especially between depths of 100 and 1,500 fathoms. The latter depth or a greater one is reached precipitately along the entire coast of California, at distances of 20 to 70 miles from shore. Off the Golden Gate, in the latitude of San Francisco Bay, at a distance of 30 miles, there is 100 fathoms; at 55 miles' distance, there is a sudden descent from 400 fathoms to a depth of two miles; at 100 miles out, 2,548 fathoms failed to reach bottom.

Soundings between San Diego, California, and Honolulu, Sandwich Islands, show that this part of the Pacific is a basin with precipitous sides and a comparatively level bottom. The distance between these points, surveyed by the *Tuscarora*, is 2,240 miles. The work was accomplished between January 6 and February 3, favourable weather being experienced during almost the entire voyage.

In the first 100 miles west from San Diego, there appear to be two valleys and two peaks. The first valley is from 622 to 784 fathoms depth; the first peak 445 fathoms, the second valley 955 fathoms, the second peak 566 fathoms. Thence a precipitous fall takes place, giving in lat. 31° 43′ N., long. 119° 28′ W., at 115 miles from San Diego, a depth of 1,915 fathoms. After that there is a gentle slope with comparatively unimportant interruptions, at the rate of three feet to the mile, to the point of greatest depth, 3,054 fathoms, at a distance of about 400 miles east of Honolulu. The sharpest elevation is a rise about midway between the United States and the Sandwich Islands, in lat. 26° 30′ N., long. 127° 37′ W., the highest portion of which is 2,159 fathoms below the surface. At the next cast of the lead, the valley to the west of this elevation took 2,650 fathoms. The fall of the side of the basin east of Honolulu is even more remarkable than the descent off the American coast. Fifty miles from Honolulu, soundings gave 498 fathoms; at 40 miles farther east, in lat. 21° 43′ N., long. 156° 21′ W., the depth was 3,023 fathoms. Between the last-mentioned point and that of greatest depth a hill rises, on whose summit there are only 2,488 fathoms of water.

These soundings coincide very nearly with the determinations of the depth of the Pacific made on theoretical grounds by the United States Coast Survey in 1854. Those calculations were based on the movements of tidal waves occasioned by earthquakes in Asia. The wave that reached San Francisco had a length of 210 to 217 miles, an oscillation of 35 minutes, and a velocity of 6·0 to 6·2 miles per minute. This would give a depth of 2,200 to 2,500 fathoms. Similar data with regard to the wave that reached San Diego (having a length of 186 to 192 miles) were calculated as giving an average depth of 2,100 fathoms. The average depth of the present soundings is about 2,400 fathoms.

The bottom is generally a soft, yellowish-brown ooze, better suited in this respect, as well as in being more level, than the route surveyed toward Oonalaska, for a telegraphic cable. Other considerations of an economic character, such as prospects of connection with other telegraph lines, may also serve to overbalance the shortness of the more northern route, and there is much better prospect of fair weather for laying a cable and keeping it in repair in the lower latitudes.

Surface-temperatures rose from 59° F. near San Diego, to 74° F. near Honolulu; temperatures at 105 fathoms between the same places rose from 50° F. to 63° F. These, of course, indicate the equatorial current. At 300 fathoms the temperature was constant at 43° F. At bottom, the temperature was everywhere 35° F., except in a single instance where it was 1° colder. The uniformity of temperature below 1,600 fathoms was noticeable.

One wire has been used in all these soundings, which were made every 40 miles, and the apparatus still works excellently.

M. CHARLES SAINTE-CLAIRE DEVILLE'S WEATHER PROGNOSTICATIONS

THE prognostications delivered by M. Charles Sainte-Claire Deville, in his communication of March 2, before the French Institute, were wonderfully fulfilled, at least for Paris, the cold period having had its beginning on the 9th, and its end on the 13th, as was predicted. Public attention was all the more attracted because the cold was manifested by a heavy fall of snow, which was the first of the year. Having recently visited M. Ch. Sainte-Claire Deville, the learned physicist was kind enough to explain everything connected with his theories.

M. Ch. Sainte-Claire Deville has very often published similar prognostications which were always successful, but never in so striking a way. He has been a constant compiler of meteorological records for nearly twenty years; and being the Inspector-General of the French Meteorological Stations, as well as a member of the French Academy of Sciences, he has consequently at his command an immense number of trustworthy observations.

He has discovered that there is monthly a large thermometrical oscillation, which he calls dodecuple, from the

the Greek word δώδεκα, twelve; that dodecuple oscillation generally takes place in the second week of the month, but it is not equally marked every month, and besides it is not true to say that it is always exhibited by a depression of the mean temperature.

The November dodecuple oscillation decidedly exhibits a warming effect. February, March, and May have, on the contrary, a cooling effect. For centuries May and November were observed and noted as the "Saints de Glace" of the spring and Martinmas summer. But other oscillations, viz. February and March, which are generally very cold, were unnoticed.

The range of the oscillations, as well as their exact position *in time*, are different for different years, very probably because there is more than one single law in operation to produce them. Happily M. Charles Sainte-Claire Deville has discovered an indication which enables him to foresee which oscillations are to be the largest or the smallest.

Each dodecuple thermometrical oscillation is preceded by a similar dodecuple barometrical oscillation. The difference of time between both oscillations is variable, but the ordinary value is *five days*. Consequently, having noted a large barometrical dodecuple oscillation on March 2, he was certain that by the 8th the regular thermometrical dodecuple oscillation for March should appear very decidedly. The deviation of the thermometrical oscillation is uncertain, to the extent of four or five days.

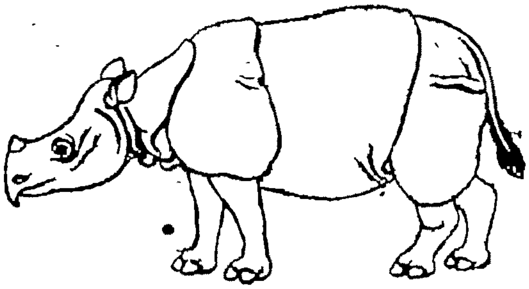
Everything is empirical in this wonderful method of announcing future oscillations of the thermometer by the careful observation of the barometer.

M. Charles Sainte-Claire Deville is of opinion that the phenomenon is owing to the presence of certain cosmical streams of meteoric bodies which may chance to be distributed in an irregular manner in the celestial space. These do not always keep just in the same place, owing to multifarious perturbations; they also vary in breadth, thickness, &c. All these assumptions are merely theoretical, but the existence of the dodecuple period in itself is based on pure observation, and cannot be questioned like the explanation offered for its origin.

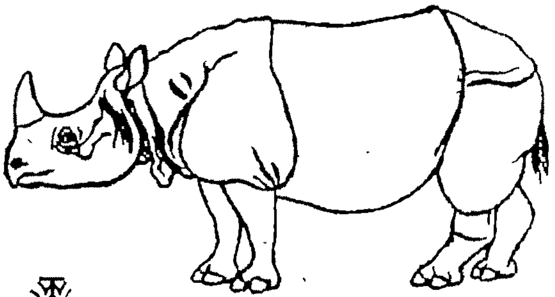
W. DE FONVIELLE

ON THE ARRANGEMENT OF THE SKIN-FOLDS IN THE ONE-HORNED RHINOCERI

IN the two accompanying woodcuts Mr. T. W. Wood has very carefully and accurately mapped out for us the manner in which the peculiar skin-folds, so conspicuous in both the Indian one-horned rhinoceri, are arranged over the surfaces of their bodies. The sketches were both taken from the specimens now living in the Zoological Gardens, the Indian animal (*Rhinoceros unicornis*) being a fully adult male, presented by Mr. A. Grote in 1864, and the Javan (*R. sondaicus*), the not quite full-grown example, of the same sex, just purchased. A fortnight ago (NATURE, vol. ix. p. 363) we mentioned some of the most important points by which the two species are distinguished, laying stress on what is rendered so much more evident by the sketches we now give, namely, the peculiar manner in which the lateral shoulder-fold—which in the Indian species does not run up the middle line of the back, but is lost over the upper part of the scapula before it reaches the post-scapular transverse fold, as it is continued longitudinally backwards—in *Rhinoceros sondaicus* is carried perpendicularly upwards along the middle of the scapular shield, quite to the back, so as to cut off an extra, independent, saddle-shaped, small, median segment, which covers the nape of the neck. The peculiar notch in the post-scapular transverse fold, and the less extent of the longitudinal fold in the gluteal shield in the Javan species, is also very apparent. Another point which is well indicated is the difference in the shape of the upper lip in the two animals, it being short and blunt in *R. indicus*, whilst it is long, pointed, and semi-prehensile in *R. sondaicus*.



R. sondaicus.



R. indicus

The head of the Javan rhinoceros is also proportionately smaller, whilst the skin-folds along the inferior surface of its neck are more symmetrical and numerous, being arranged so as to appear very like the surface of a coarse three-cord braid. Its skin, especially over the back, is covered with hair to a degree which would hardly have been expected, as in the Indian species there is but little hair to be seen. The ears are also fringed, much in the same way that they are in *Rhinoceros lasiotis* and *R. sumatranus*, the two Asiatic two-horned species.

The two sketches are made of one size to facilitate comparison, but it must be borne in mind that the Javan animal never reaches anything like the bulk of its Indian ally. It is also almost certain that its skin never becomes so coarsely tuberculated.

In rhinoceri kept in confinement there is nothing to be learnt from the shape or length of the horns, because that depends so much on the opportunities which their owners have had of rubbing them down. In the wild state the continual employment of the horn or horns in tossing and dividing comparatively yielding substances, such as loose earth and wood, causes them to become pointed, long, and polished, because they wear at the sides almost entirely. But in captivity the seasoned wood, iron, and stone of the cages only break off the tips and leave the sides comparatively unworn, or very unequally so; this is why museum specimens of horns are generally so very unlike those found on exhibited living animals.

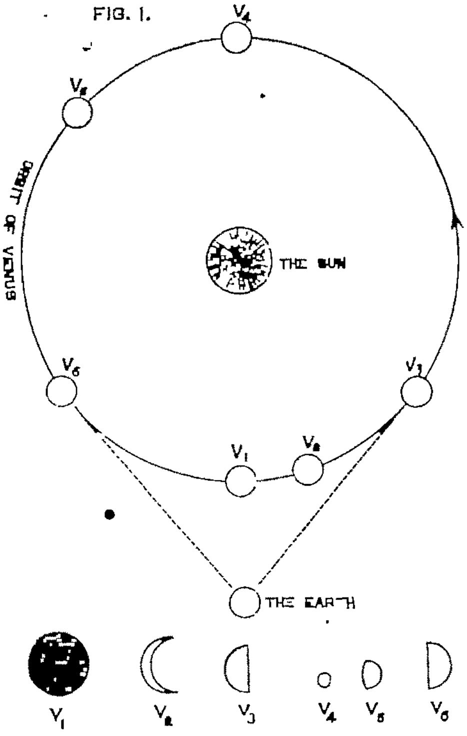
Those who noticed the illustrations we gave two months ago (NATURE, vol. ix. p. 227) of the huge *Brontotherium ingens* discovered by Prof. Marsh, will be struck, on looking at the Javan rhinoceros, with the general similarity in the proportions of the head in the two animals. The nose is undoubtedly different, but there is the same extreme shallowness of the frontal and interorbital region, combined with great zygomatic breadth. In Brontotherium the two expanded symmetrical nasal processes were probably covered with tough skin, like those on the face of the wart-hog, to replace in function the coreless but none the less well-developed horn of the rhinoceros.

THE COMING TRANSIT OF VENUS

I.

IN days of old it was supposed that the earth held the central position of the solar system, and that moon, sun, and planets moved round it, each in its own orbit. The moon was supposed to be nearest to us, then came Venus, then Mercury, after that the sun, then Mars, Jupiter, and Saturn. We now know that of all these the moon is the only one which revolves round the earth, and that all planets go round the sun in the following order:—Mercury, Venus, the earth, Mars, Jupiter, Saturn. These are all the planets which were known to the ancients. Since Mercury and Venus were formerly supposed to be lower than the sun, and all the others higher, the name of *inferior planets* was given to the former, and *superior planets* to the others. These terms are still retained by astronomers, though the ideas that gave rise to these terms are long since exploded. Fig. 1 shows the phenomena exhibited by an inferior planet in the course of its journey round the sun. V is the planet Venus in the different parts of its orbit. E is the earth, which is shown in the figure always in one position, although of course it also describes an orbit round the sun. We are naturally led by a study of the diagram to three points of interest concerning the motions of an inferior planet.

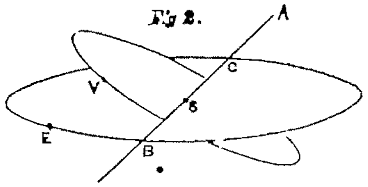
FIG. 1.



The first is that the planet can never seem to be far distant from the sun. Venus moves round the sun in the direction shown by the arrow. The earth rotates in the same direction. We are supposed to be looking down upon the solar system from some point in the northern heavens. It will be noticed that when the planet leaves the point V_1 she will seem to recede from the sun more and more, until she reaches the position V_3. She can never be farther from the sun than this, and is then said to be at her greatest eastern elongation. She then approaches the direction in which the sun is seen, until she is lost in the brightness of his rays. During all this time she is seen best in the early morning before sunrise, setting before the sun. When Venus has passed this position her distance from the sun appears to an observer upon the earth to increase until she reaches V_6, her greatest western elongation, when she again begins to approach the sun.

The next point to be noticed is that she is sometimes a great deal closer to the earth than at other times; and when she is most near to the earth she appears to be larger. At her closest approach to the earth she is only about 26,000,000 of miles away; but when farthest off her distance is 158,000,000 of miles. Her apparent size is therefore much greater in the first case than in the second. These differences are shown at the lower part of Fig. 1.

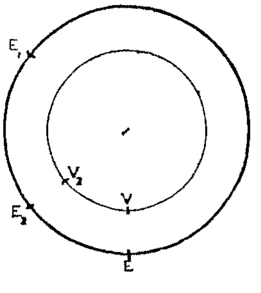
FIG. 2.



The third point to be mentioned is that she exhibits phases just as the moon does. In any position that hemisphere alone is illuminated which is directed to the sun; so that in the position V_3, when we can only see one-half of that hemisphere, she will have the appearance of a half-moon. So in the position V_2 she has a crescent form, and at V_5 a gibbous form. The apparent size and shape of the planet in its different positions are shown in the lower part of Fig. 1.

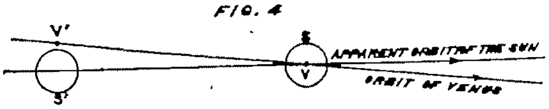
The question now arises, what will happen when Venus is between us and the sun? In the first place, since her illuminated hemisphere is turned away from us, she will appear to be black; so that we shall have no chance of seeing her, unless she be seen as a black spot upon the bright surface of the sun. We would naturally expect that this should happen every time that the planet is at its least distance from us. A simple consideration shows that this need not be the case. The orbits of Venus and the earth do not lie in the same plane. In other words, we cannot represent accurately the paths of Venus and the earth by a drawing upon a sheet of paper. The orbit of Venus would have to be tilted up above the plane of the earth's orbit. Both of these planes pass through the sun, but they are inclined to each other at a certain small angle. This will be seen by a glance at Fig. 2, where V represents the orbit of Venus, E that of the earth. The line AB, which passes through the sun S, is called the line of nodes; and it is quite clear that in order to see Venus as a black spot upon the sun both the earth and Venus must lie approximately on this line of nodes. Now it generally happens that when Venus is at her least distance from the earth, these two planets occupy some such places as V and E, so that she seems to be above the sun; and, not being illuminated, she is invisible. Only twice in a century does she reach the node, so nearly at the same time as the earth, as to be seen as a black spot upon the sun. Such a phenomenon

FIG 3



is called a transit of Venus. If it happen that Venus seems to pass across the centre of the sun she takes about eight hours to complete the passage. The earth occupies the position A always in June, and the position B in December. If there be a transit of Venus when the earth is at B, Venus is said to be at the *descending* node, because then she is descending from the northern portion of her orbit to the southern. When Venus is at C she is at her *ascending* node.

FIG. 4

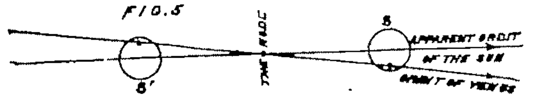


It has been said that there are, roughly speaking, two transits of Venus in a century. The following table shows all the transits of which we know anything:—

1631. Predicted by Kepler, but not observed.
1639. Predicted and observed by Horrox.
1761. Predicted by Halley; observed by many.
1769. Observed generally.
1874.
1882.

It will be noticed that the transits occur in pairs, eight years apart; the reason of this can easily be rendered clear. The earth takes 365·256 days to go round the sun; Venus takes only 224·7 days. Suppose then that at any particular date Venus and the earth are at the node simultaneously, viz. at V and E, Fig. 3, a transit of Venus over the sun's diameter will be seen. When Venus has completed a revolution the earth will have moved away to E_1, and Venus will not overtake the earth until they reach the positions V_2 and E_2. This is 583·920 days from the time when they were at V and E; but V_2 and E_2 do not lie upon the line of nodes; hence there can be no transit. After another 584 days Venus will again be in conjunction with the sun, but still not on the line of nodes. But the fifth conjunction occurs after 2919·6 days (5×583·920); and the earth completes eight revolutions in 2922·05 days. Thus it appears that, at this conjunction of Venus with the sun, the earth and Venus are very near to their old positions V and E. Hence they are almost on the line of nodes. In this case we can without difficulty examine the possibility of a transit. If we suppose the motion of the earth to be stopped, the apparent motions of the sun and Venus may be represented as in Fig. 4, where a portion of the orbit of Venus and of the ecliptic are shown near the nodes. When the sun and Venus are on the line of nodes simultaneously S represents the sun and V Venus. At the fifth conjunction the sun will not quite have reached S, but will be 2¼ days behind at S′; Venus will then be at V′. Now in this case there can be no transit visible, for here Venus is quite out of range of the sun. But if in the original transit the sun was a little past the node as at S (Fig. 5), then eight years after he will be at S′, and there will be another transit. It follows from this that there will be a pair of transits eight years apart, only when in the first one Venus does not pass close to the sun's centre. This equality of eight revolutions of the earth, with thirteen of Venus, is very interesting, because this fact was shown by the present Astronomer Royal to account for an inequality in the earth's motion due to the attracting influence of Venus. The result of a short calculation informs us that for positions of Venus and the earth near the line of nodes, Venus is at one conjunction 22′ 16″ distant from her position at the conjunction which occurred eight years previously,* this distance being measured at right angles to the ecliptic. Now the sun's diameter is 32′. This shows why, generally, there are two transits eight years apart.

FIG. 5



The first prediction of a transit of Venus was made by Kepler,† and was calculated from his Rudolphine tables. In 1631, the year predicted, astronomers of Europe were eagerly on the watch for so rare a spectacle. But the calculation was in error, so that it took place when the sun was below the horizon in Europe, and was consequently invisible.

After this no astronomers seem to have interested themselves about the possibility of such an occurrence, with one exception. Jeremiah Horrox, a curate of the village of Hoole, near Liverpool, was much devoted to astronomical pursuits.‡ He possessed some tables for calculating the places of the planets; but his observations did not agree at all with them. He had, however, before discovering the faults of Lansberg's tables, calculated from them the future positions of the planets. This work, with corrections deduced from his own observations, led him to predict a transit of Venus, visible in England, for the year 1639. He acquainted his friend Crabtree, of Manchester, with the results of his calculation, and then prepared himself for the observation. He considered the best method to be the employment of a telescope to throw an image of the sun on a white sheet of paper in a darkened room. A circle was drawn, of about 6 inches diameter, upon the paper, to make the sun's image exactly fill the circle. A plumb-line would give him the direction of the vertical, and by marking successive positions of the planet on the sun's disc, he would be able to calculate many of the elements of Venus. Such an observation is of course peculiarly well suited for determining the diameter of the planet, the inclination of its orbit, the position of the node, and the true time of passing this node. His calculations showed that the transit ought to commence on the afternoon of November 24 (old style); but to guard against disappointment, and because of discrepancies in various tables, he kept a watch from the 23rd. On returning from some clerical duties on the 24th (Sunday) he was gratified by beholding a black spot on the sheet of paper, which indicated the presence of Venus on the sun's disc. He made three observations before sunset and has left us a drawing to illustrate the observations.§

It is curious to find an astronomer supporting the opinions of the astrologers; but in his treatise we find that the chance of a clouded atmosphere caused him much anxiety, for Jupiter and Mercury were in conjunction with the sun almost at the same time as Venus. This seemed to him to forbode great severity of weather. He adds, "Mercury, whose conjunction with the sun is invariably attended with storm and tempest, was especially to be feared. In this apprehension I coincide with the opinion of the astrologers, because it is confirmed by experience; but in other respects I cannot help despising their more than puerile vanities." But we must not laugh at Horrox for his opinion. In our own day there is a considerable number of diligent astronomers who believe that the cyclones in the Indian Ocean, certain other winds, the growth of vines, and various other phenomena, are in part regulated by the positions of Venus and Jupiter with respect to the sun.*

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* For at the fifth conjunction the earth is 2·45 days distant from her place at the original conjunction. This is equivalent to 2° 24′ 59″, when viewed from the sun, from which subtract 2′ 44″ (= the retrogression of the node of Venus in eight years), and we have 2° 22′ 15″ = the angular distance of the earth from its corrected original position, as seen from the sun. The ratio of this to the angular distance of Venus from her original position as seen from the earth $= \frac{\text{dist. of Venus from earth}}{\text{dist. of earth from sun}} = \frac{277}{723}$. Multiplying 2° 22′ 15″ by 723, and dividing by 277, we have 6° 11′ 17″. Multiplying this by ·06 = tan 3° 23′ 31″, which is the inclination of the orbit of Venus, we have 22′ 16″ = the latitude of Venus at the fifth conjunction.

† "Admonitiuncula ad Curiosos rerum Coelestium," Leipsic, 1626.

‡ See NATURE, vol. viii. p. 113.

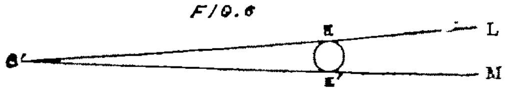
§ Venus in Sole Visa.

Mr. Horrox's observations have been of great value in perfecting the tables of Venus. He was further led by a kind of analogy, much in vogue at the time, to deduce from his observations a value of the sun's distance from the earth. It will readily be understood that if we could find out what size, in angular measure, the earth would seem to have if viewed from the sun, we should have a means of determining how much greater the distance from the earth to the sun is than the diameter of the earth. For, suppose S (Fig. 6) to be the position of an observer placed upon the sun, S L, S M the directions in which he must look to see the opposite sides of the earth, so that the inclination of these lines is known. All we have to do now is to draw a circle of any size and move it about between the lines S L, S M, until it just fills the interval, as at E E′. If now we measure with a ruler how much greater S E is than E E′ we shall know the distance from the earth to the sun, the earth's diameter being taken as the unit of measurement; and if we multiply this by the diameter of the earth measured in miles we shall know the distance from the earth to the sun, in miles. All that we require to know is the size of the angle E S E′. Horrox estimated the probable value of this angle in the following manner. From the observations of Tycho Brahé it appeared that during the transit of Venus the apparent diameter of the planet would be 12′ 18″; while Lansberg found 12′ 21″; and Kepler 6′ 51″. Horrox found from his measurements that it was only 1′ 16″. The error of ordinary observations arises from the apparent enlargement of the planet's disc through irradiation. Gassendi had in the same manner, during the transit of Mercury in 1631, reduced the apparent diameter of Mercury to scarcely 20″. From these data it can be found that the apparent diameters of Venus and Mercury as seen from the sun would be 21″ and 34″ respectively. Proceeding to the other planets he arrived at the general conclusion that each of them would, if viewed from the sun, have an apparent diameter of about 28″. Applying this to the case of the earth, he showed that the distance of the earth from the sun must be 7,500 diameters of the earth (it may be well here to state that the latest measurements show the apparent diameter of the earth as viewed from the sun to be about 18″, and the distance = 11,400 diameters). This analogy by Horrox gave a much closer approach to the truth than any previous conjectures.

Before taking leave of Horrox, we must say a few words to his memory. He died at the early age of 23. During his short career he showed a remarkable aptitude for the acquisition of knowledge, and for the striking out of new ideas. He lived at a time when the scientific spirit of the age was leading up to the theory of gravitation, and many passages in his writings show that he had even then grasped the grand idea of the theory, and that he was well fitted to become its constructor and its expounder. His researches on the lunar and planetary theories indicate the magnitude of his talents.

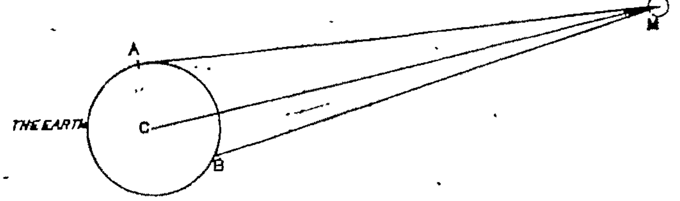
We have already mentioned some of the uses to which careful observations of a transit of Venus may be applied; viz. the correction of the elements of the planet's orbit. But the observation also leads us to a knowledge of the distance of the sun from the earth, and in a manner much more direct and logical than that employed by Horrox. There is an opinion very prevalent that a transit of Venus affords the best means of determining this distance. So far as our present knowledge goes we are hardly justified in such a statement until after the observations that shall be made in the present year.

FIG. 6



Before entering upon the method by which we measure the sun's distance, let us devote a few lines to explaining what is meant by the word *parallax*, which is continually employed in such discussions. Let a man stand in a street exactly north of a lamp-post. The lamp-post will seem to be south of him. Now let him cross over to the other side of the street. The lamp-post will now be in some other direction, such as south-west. This movement of the direction of the lamp-post is the effect of parallax. Now let us suppose, by a stretch of imagination, that a man observes the moon from the centre of the earth. He will see it in the direction C M (Fig. 7). If now he goes to A he will see it in the direction A M. The angle A M C through which the moon appears to have been moved is the parallax of the moon as observed from A.

FIG. 7.



It will be noticed that the parallax is an error introduced into the observed position of the moon, and which must be allowed for if we wish to get the position as seen from C. Moreover, the parallax at B is different from what it is at A. But at no point on the surface of the earth can the parallax be greater than at A. And if we know the parallax of the moon at A, we can deduce that at B from a knowledge of the relative positions of A, B, and C. Hence it is useful to have a distinct name for the parallax at A. Now it will be noticed that a line drawn from C to A is the vertical line at A; hence the moon M will appear to be on the horizon to an observer at A; and hence the moon has its greatest parallax when on the horizon. For this reason the parallax at A is called the moon's *horizontal parallax*. Further, since the equatorial diameter of the earth is greater than the polar, the parallax will be greater, when the moon is on the horizon, to an observer at the equator than to an observer at one of the poles. Hence the greatest parallax we can have occurs when the moon is on the horizon and the observer is at the equator; this value of the parallax is the *equatorial horizontal parallax*. In the same way the sun has an equatorial horizontal parallax, and if we knew its value we could find out the sun's distance from the earth as explained above (Fig. 6).

GEORGE FORBES

(*To be continued.*)

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* See the researches of Messrs. De la Rue, Stewart, and Loewy on the connection of sun-spot frequency with planetary positions, "Phil Trans."; also the writings of Mr Meldrum, Mr. E. J. Stone, Prof. Balfour Stewart, M. Poey, and others, on the connection between terrestrial phenomena and sun-spot frequency.

THE "CHALLENGER" EXPEDITION

III.

ST. VINCENT, CAPE DE VERDE ISLANDS

ST. VINCENT, one of the Cape de Verde Islands, noted in the old gazeteers for its wood, water, wild goats, turtles, and saltpetre, was visited by the *Challenger* in July and August last. From a record of this visit we gather the following particulars about the island itself and of the plants growing there. The island is small, not more than twelve miles by six, comparatively flat in the centre, but surrounded by higher land. This range of high land is divided by a series of deep valleys, forming ridges which are again divided into transverse valleys. Most of the hills are from 700 to 1,200 ft. high, but one in the south is over 2,200 ft. high. St. Vincent is sometimes visited by long periods of drought, extending occasionally to a year's duration, during which time the whole island has a parched, sterile appearance. The most abundant plant in the island is *Lavandula rotundifolia* Bth., which forms small thick bushes; there is also on the summits of the higher hills *Euphorbia tuckeyana*, Steud., and on the sandy plains as seen from the vessel in Porto Grande, reaching inland from the shore, were dense masses of bushes of *Tamarix gallica*. In one spot, springing up from amongst these bushes, was the well-known tamarind tree (*Tamarindus indica* L.), so valuable both in the East and West Indies, for the sake of the agreeable acid pulp contained in the pod, which when preserved in sugar forms "Tamarinds" of commerce. Side by side with the tamarind grew *Acacia albida* Del., and *Terminalia catappa* L., the first being one of the many spiny acacias found on the African continent, where it forms a large straggling branching tree, with straight, stout spines, sometimes ¾ in. long. The *Terminalia* is a native of India, but has been found in Upper Guinea, though probably not indigenous. The seeds are almond-shaped, white, and of an agreeable taste. In the plains *Tribulus cistoides* L., a spreading prostrate or decumbent plant, occurs in great abundance, as also a small grass. Nearly all the vegetation, however, had a shrivelled, dried-up appearance, with the exception of the lavender, upon which a few fresh green leaves were to be seen. The effect of rain in changing the aspect of vegetation on this island is said to be almost instantaneous, not only in bringing out the young foliage of perennial plants, but also in causing a thick carpet of seedlings to spring up. Though the hill slopes and the lower parts of the valleys are in some parts of the island covered with a thick grass, the drought causes it to become so dry that goats and cattle frequently die from sheer starvation.

On the Green Mountain, at an altitude of about 200 ft. above the level of the sea, the gardens contained pumpkins, sugar-cane, a small kind of date-palm, and maize; cotton bushes also grew in the neighbourhood. At another 500 ft. there were Euphorbias and the woody Composites. At 1,000 ft. there were *Echium stenosiphon* Webb, in flower; and at 1,300 ft. occurred patches of moss and marchantia, while at 1,700 ft. *Statice jovi-barba* Webb was abundant. The *Lavandula rotundifolia*, which is found at the very top of the mountain, has here a very different aspect from that before described, inasmuch as it is green and vigorous-looking. In the south-west of the island, at a height of 900 ft., was discovered a single plant of *Sarcostemma daltoni* Dcne, which grows on the cliffs at St. Jago, almost down to the sea. On the top of the Green Mountain the land is much cultivated with potatoes, tomatoes, pumpkins, maize, and similar plants. The position is so favourable to the growth of the tomato that it appears to have run wild. The origin of many of the introduced weeds which grow on the mountain is no doubt traceable to the imported seeds of the vegetables just mentioned.

At an elevation of about 2,000 ft. was a group of agaves planted in the form of a double circle; many of them had flowering spikes about 10 ft. high. The marked differences of aspect caused respectively by the trade-winds and the sun, at altitudes suitable for plant growth, are points of much interest, illustrations of which may be had in the fact that *Aizoon canariense* L., which grows on the windward and shady side of Bird Rock, nearly down to the sea-level, does not commence till 700 feet or 800 feet on the leeward sides of the main island; the Euphorbias and woody Composites are found at about the same elevation, while on the other side they reach nearly down to the sea. On the windward slopes of the mountains, on the southern side of the island, the vegetation commences at a higher elevation, being kept back by the wind becoming heated and dry from its passage across the hot central plain.

Sinapidendron vogelii Webb, a cruciferous undershrub, with yellow flowers, grows on the cliffs on the weather side of the island; and *Samolus valerandi* L., known to us as the brookweed or water pimpernel, an erect plant, from 8 in. to 10 in. high, growing in marshy places or near springs, was also seen, but only in a single isolated patch near a small stream. This plant is remarkable for its very wide geographical range, being found in almost every country where the soil is wet and gravelly, and though seen only in this one spot in St. Vincent, it grows abundantly in St. Jago by the stream in St. Domingo Valley.

As seen from the sea, the rocks of St. Vincent present a singular appearance, owing to the presence of a thick incrustation at water-mark of masses of calcareous algæ, which either follow the forms of the rocks, or occur in rounded masses, their delicate tints of white, light pink, or cream colour, considerably heightening the effect. These incrustations are frequently bored by *Lithodomus candigerus* and other molluscs, and small sponges and Bryozoa occupy the cavities between them and the rocks.

ST. PAUL'S ROCKS

The isolated rocks known as St. Paul's Rocks lie to the north of the equator about 1°, and in longitude 29° 15′ W., nearly midway between the South American and African coasts. They are in truth mere rocks, not more than a quarter of a mile long, and rising to a height of from 50 to 60 ft. above the sea. They are described by travellers as being quite bare of land-vegetation even to the exclusion of lichens, the only vegetation in fact being numerous species of algæ.

From an examination made of the rocks during the two days' stay of the *Challenger*, it seems that the species of algæ are by no means numerous, fourteen species being all that were found; so powerful indeed is the wash of the waves that it seems to be too much even for these marine plants to retain their positions. The water also deepens very rapidly round the rocks, so that it is not likely many species would be found there.

A similar incrustation of calcareous algæ is seen upon the rocks as at St. Vincent. It here forms a deep pinkish white band at tide mark, and is riddled through and through by a small annelid. In some places the colour of the incrustation is white, and above this is a dark red-coloured algæ, covering the rocks for several feet. At the bottom of the small bay formed by the circlet of rocks and extending out in the sea to a depth of twenty fathoms, is a thick growth of a green-coloured sea-weed (*Caulerpa clavifera* Ag.), together with another species of smaller growth. The former is loosened from the bottom by the action of the waves, and is gathered up by the noddies (*Sterna stolida*) to build their nests.

A few diatoms and oscillatoriæ occur in stagnant pools, among them being *Navicula didyma*, *Rhabdonema adriaticum* and *Biddulphia pulchella*. A careful examination of the guano found in the hollows of the rocks showed that no diatoms were present, but fossil fragments of incrustation formed of a kind of nullipore, combined with pebbles and broken shells, occur in the singular veins of conglomerate traversing the rock.

NOTES

WE are glad to hear that Government have consented, though tardily, to give effect to the wishes of the country, by offering to defray the expenses of the funeral of Dr. Livingstone in Westminster Abbey The *Times* states that a merchant in the city of London, in view of the inability of Dr Livingstone's family to bear the expense of the ceremonial, had already volunteered to be at the charges of this melancholy tribute of respect, but it obviously would have been unbecoming the dignity of the nation which has been honoured by the achievements of the illustrious traveller for the last honours to have been rendered him at the cost of any private person. The Southampton Town Council has resolved to receive, with fitting honours, the remains of Dr. Livingstone, which are expected to arrive at that port very shortly. The body, it is said, is to lie in state for four days in the Geographical Society's building, Saville Row.

AT an influential meeting held at Edinburgh on Monday, it was resolved to extend on a large scale the University buildings and to remodel those already existing, so as to suit them to modern requirements. We are glad to notice that the importance of Science in University teaching was insisted on at the meeting, and we hope that in the extended and remodelled University scientific teaching and research will be accorded a prominent place. Of the 100,000*l.* required, 55,000*l.* have been already subscribed. The Right Hon. Lyon Playfair, M.P., gave an able address, in which he said,—A student's life should not be that of a bookworm. Discovery as well as instruction in Science forms the glory of a Professor, and infuses a new life into the pupil who comes under its influence. Our efforts in the extended organisation which we now ask for this University should be to provide means for investigation in Science, as well as its mere teaching. Of all things in the world nothing is so dismal or so useless as to connect Science with mere verbalism, and to try to teach it out of a book as you would a page of Livy or Homer.

THE French Government have only done an act of justice in conferring a pension of 12,000 francs upon M. Pasteur in consideration of his services to science and industry.

COLONEL STUART WORTLEY has been appointed by the Commissioners of Patents to the Curatorship of the Patent Museum at South Kensington, vacant by the death of Sir Francis Pettit Smith.

SIGNOR AUGUSTO RIGHI has been appointed Professor of Natural Philosophy in the Instituto Technica Reale of Bologna.

THE Council of the Society of Arts has decided to offer a prize, consisting of a gold medal or 20 guineas, for the best essay "On the Cultivation and Manufacture of Indian Teas."

A MEETING of the local general committee of the British Association was held in the rooms of the Chamber of Commerce Bradford Exchange, on March 31, to receive a report as to the reception fund. It appeared that the total receipts were 3,248*l.* 16*s.* 8*d.*, and the expenditure 3,097*l.* 7*s.* 2*d.*, leaving a balance of 155*l.* 9*s.* 6*d.* in the bank, subject to the expenses connected with the winding up of the committee's duties. It was agreed that the balance remain in the hands of the local executive committee until all expenses are paid, and that the amount which may remain be given to the Bradford Philosophical Society, an institution having an object kindred to that of the British Association.

M. LEVERRIER has been appointed president of the section of Science at the meeting of the Delegates of Learned Societies, which will be held at Sorbonne.

FROM the sixth quarterly report on the Sub-Wealden Exploration we learn that during the last two months a depth of 359 ft. has been bored, making a total of 671 ft. The borings are still in the Kimmeridge clay. In this deposit indications of petroleum have been noticed, and at depths of from 600 ft. to 650 ft. it was particularly observable. Occasional veins of carbonate of lime have been met with crossing the cores obliquely, but the report states that all the beds yet passed through are horizontal. One of the most important results of the exploration has been the discovery of gypsum and other beds which are likely to prove commercially productive. Attached to the report is a list of the fossils which have been found. The committee report that they have sufficient funds to continue the work to a depth of 1,000 ft., and should it be deemed desirable to go beyond that depth, a conference will be held to consider the question before soliciting further contributions. The present balance in hand is 576*l.* 4*s.* 4*d.*

IT is said MM. Croce Spinelli and Sivel will be awarded a prize for their last aeronautical ascent, in which they took with them oxygenised air.

ANOTHER aeronautical ascent took place on Thursday week from Lavillette gasworks in a new balloon called "Michel le Brave," which is to be sent to Roumania. The measurement is 1,500 cubic metres. It was sent up with six persons and descended at Vic-sur-Aisne in a regular storm; large trees were uprooted, but no bodily harm was received by the balloonists. It is intended to have several other ascents next spring.

THE greatest alarm has been caused in North Carolina and Tennessee by the appearance of what seems to be volcanic phenomena in the former of these states. The scene of the reported disturbance is Bald Mountain, in the south-western part of M'Dowell County. Rumblings were heard during several days, apparently coming from the interior of the mountain, and one letter, dated March 20, states that near the summit of the peak an area of nearly an acre was agitated by subterranean upheavals, and from which smoke and vapour issued. The people of the surrounding district are reported as being in the greatest consternation, ceasing from work and living in common, and evidently quite expecting that the final catastrophe is impending.

SEISMIC commotions of some magnitude have been felt in Algeria, at Algiers, and surrounding places. The centre of commotion seems to have been somewhere in the vicinity of Cherchel, where the barracks have suffered much. The first shock was felt on March 28 at 11.10 A.M.

AN Alpine club has been established in Paris under the presidency of M. de Billy. The rules will be similar to those of English, Swiss, Italian, and Austrian Alpine clubs. It is intended to issue a periodical containing the papers read before the Association. More than 100 members have been enrolled.

THE sittings of the Bureau des Longitudes are now being held at the Collége de France.

THE Mexican Axolotls, which have for some time been exhibited in one of the handsome vases in the entrance-hall of the Brighton Aquarium, spawned about a month ago. As the parents showed some disposition to devour their eggs, the latter were removed to one of the troughs of the salmon-hatching apparatus, where the young axolotls may now be seen, having just been hatched after a period of 29 days in the egg.

A SUPPLEMENTARY credit of 4,000*l*. has been voted by the Versailles National Assembly for paying a part of the expenses incurred by the observation of the Transit of Venus. Six members belonging to the ultra-clerical party have given a negative vote on a division. It is said they are not believers in the Copernican theory, and have no faith in the astronomical observations.

THE Rev Henry Moule, after a series of experiments extending over twenty years, has devised a process of manufacturing an illuminating gas from Kimmeridge clay.

THE educational means of Harvard University have recently been increased by the addition of an institution which will make that University one of the most complete in the United States. This Institution is known as the "Bussey Institution," after Mr. Benjamin Bussey of Roxbury, Mass., who about thirty years ago left to the University a magnificent sum of money and a small estate for the purpose of promoting the scientific study of agriculture and horticulture. The money was allowed to accumulate for many years, and has since been increased by 100,000 dollars left by Mr. James Arnold, Merchant, of New Bedford. The estate has been to some extent laid out for the purpose intended, and several suitable buildings, including a laboratory, have been erected, and the Harvard authorities have devised a course of instruction and investigation on a broad and thoroughly scientific basis. The appointments already made include:—An instructor in Farming, a professor of Agricultural Chemistry, a professor of Horticulture, a professor of Applied Zoology, an instructor in Entomology, a director of the Arnold Arboretum, and a librarian and curator of Collections. The institution is intended both for instruction and investigation, though we are glad to see that students' fees are not necessary to the support of the institution. The permanent funds provided by Mr. Bussey will enable the President and Fellows to maintain the Institution as a scientific station, like the Astronomical Observatory or the Museum of Comparative Zoology at Harvard College, until the time shall come when there shall be a demand for its privileges as a school. The experiments and investigations made at the Bussey Institution will be published from time to time in a Bulletin, the first number of which is before us. It contains four papers by F. H. Storer, professor of Agricultural Chemistry in Harvard University, one containing analyses of some commercial fertilisers, another of American "shorts" and "middlings," a third On the Agricultural Value of the Ashes of Anthracite, and a fourth containing a Record of Trials of various Fertilisers on the ground of the Bussey Institution. The only other paper is a useful one by Dr. Slade, professor of Applied Zoology, On the Humane Destruction of Animals.

THE French Society of Geography, we learn from *La Nature*, has just received news of a French expedition which has been exploring Terra del Fuego. On December 7, last year, the expedition landed on the coast of that island, and proceeded into the interior. The explorers found a large lake of 25 kilomètres in circumference, surrounded by luxuriant vegetation, and literally covered by an army of wild fowl, among which the most abundant were ducks and geese. These regions are inhabited by rude but hospitable tribes; the women especially are very affable and obliging. One of them, in exchange for some pieces of sugar and common handkerchiefs, gave the leader of the expedition an object to which she attached an immense value, and which she preserved as a relic,—the lid of a sardine box.

WITH a view of properly exhibiting the geological and metallurgical resources of America at the forthcoming exposition at Philadelphia, an association has been organised, embracing such names as those of Prof. Leslie, Prof. Genth, Prof. Raymond, Prof. Wyman, Prof. T. Sterry Hunt, George H. Cook, and others, to whom is to be intrusted by the Board of Centennial Commissioners the duty of collecting whatever will best answer the purpose in question.

THE Paris Jardin d'Acclimatation has succeeded in "breaking-in," some zebras so far as to induce them quietly to draw a carriage, and one permits children to ride round the gardens on its back.

DR. HAYDEN, the head of the Geological and Geographical Survey of the U.S. Territories, has commenced the publication of a bulletin to communicate such announcements of new facts made by any member of his party as it is desirable to bring promptly to the notice of the scientific community in advance of their publication in his reports. The first number of this Bulletin, bearing date January 21, is occupied by a list of the members and collaborators of the survey for 1873, and a list of the publications, from which we learn that six volumes of the reports have appeared from 1867 to 1873, and that seven volumes of miscellaneous publications will be published in octavo form, the most elaborate being the hand-book of the Ornithology of the North-western Territories, by Dr. Coues. Several quarto volumes will also be sent out, of which there have been actually published one by Prof. Leidy, on the extinct vertebrata of the Western formations, and one on the Acridida of North America, by Prof. Cyrus Thomas. This quarto series, it is expected, will include ten volumes, among them memoirs on the vertebrata of the cretaceous and Tertiary formations, by Prof. Cope; one each, on the fossil plants, by Prof. Newberry and Prof. Lesquereux; on the fossil invertebrates, by Prof. Meek; and the volume on general geology, by Prof. Hayden. Thirteen maps have been published for the survey, those of the Yellowstone region being especially valuable. The body of the bulletin is occupied by a report on the stratigraphy and Pliocene vertebrate palæontology of Northern Colorado, by Prof. Cope, in which he presents the parallelism of the formations recently investigated by him with those earlier known and in other parts of the West. He concludes that, although these formations have generally been considered as Tertiary, the geological evidence shows them to be strictly mesozoic, as in the great lignite formations on the Missouri River. During the past season twenty-one new species of vertebrates were obtained in the Pliocene sandstone at the head of the watershed between the South Platte River and the Lodge-pole Creek.

THE *Belgique Horticole*, for February, publishes a complete list of botanical gardens throughout the world, with the names of their curators and of the professors of botany at the different towns.

THE Science and Art Department has issued a catalogue of apparatus for instruction in geology, mineralogy, animal physiology, elementary botany, general biology, principles of mining, and physical geography.

THE *Brisbane Courier* of December 30, 1873, publishes the following official telegram from Mr. Walter Hill, the Government botanist, dated from Cardwell on the 27th and received by the Queensland Secretary for Lands:—"Since November 20 we have examined the banks of the Mulgrave, Russell, Mossman, Daintree, and Hull Rivers, and have been more or less successful in finding suitable land for sugar and other tropical and semi-tropical productions. The ascent of the summit of Bellenden Kerr was successfully made by Johnstone, Hill, and eight troopers. At 2,500 ft. in height we observed an undescribed tree with crimson flowers, which excels the *Poinciana regia*, *Colvillia racemosa*, *Lagerstrœmia regia*, and the *Jacaranda mimosifolia*. At 4,400 ft. a tree-fern, which will excel in grandeur all others of the Alboreous class. A palm-tree at the same height which will rival any of the British-Indian species in gracefulness. On the banks of the Daintree we saw a palm-tree cocoa, which far exceeds the unique specimens in the garden of the same genera from Brazil in grandeur and gracefulness. While cutting a given line on the banks of the river Johnstone for the purpose of examining the land, an enormous fig-tree stood in the way, far exceeding in stoutness and grandeur the renowned forest giants of California and Victoria. Three feet from the ground it measured 150 ft. in circumference; at 55 ft., where it sent forth giant branches, the stem was nearly 80 ft. in circumference. The River Johnstone, within a limited distance of the coast, offers the first and best inducements to sugar cultivation."

WE are glad to observe signs of life in Dundee, says the *Scottish Naturalist*. That town, long noted for its commercial enterprise, has had nearly an equal, but not enviable, celebrity for its poverty and deadness in regard to the study of natural science. But now we trust that that reproach will soon be wiped away, and that the members of the recently founded Dundee Naturalists' Society, a copy of whose constitution is before us, will do good work, and show their fellow-citizens that there are other and more valuable *dona Dei* in the fields, woods, and mountains of the interesting county of Forfar, than that wealth for which the inhabitants of the town of the *donum Dei* are deservedly remarkable. The Society has already upwards of forty members, which number will probably soon be considerably increased. We recommend to the Society the formation of a good local museum of the natural productions of Forfarshire.

IN the forty-first volume of the *Journal of the Asiatic Society of Bengal*, Mr. G. E. Dobson has drawn attention to a particularly interesting feature in the osteology of the Rhinolophine Bats. In the genera *Phyllorhina*, *Trianops* and *Cœlops*, he finds that in the innominate bone the ilium sends forward a process from its upper part, which meets and anchyloses with an extension of the ileo-pectineal spine to form a second foramen above that around which the obturator muscles arise. This peculiarity has not been observed in any other mammal.

THE additions to the Zoological Society's Gardens during the last week include a St. John's Monkey (*Macacus sancti-johannis*) from China, and a Java-Chevrotain (*Tragulus javanicus*) presented by Captain Nutsford; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. W. Webster; three Passenger Pigeons (*Ectopistes migratorius*) from North America, purchased; an Egyptian Monitor (*Monitor niloticus*) six feet long; and a Tuberculated Lizard (*Iguana tuberculata*) from the West Indies, deposited.

SCIENTIFIC SERIALS

American Journal of Science and Arts, March.—This number commences with an interesting paper, by Prof. Leconte, On the Great Lava Flood of the West, and on the Structure and Age of the Cascade Mountains. The flood, commencing in Middle California in separate streams, became in Northern Oregon and Washington absolutely universal; the whole country, mountain and valley, being buried several thousand feet. Its extent cannot be less than 200,000 to 300,000 square miles; its average thickness is probably 2,000 ft., and extreme thickness 3,700 ft. From the structure of the Cascade Range (which extended throughout the entire region of the flood) and palæontological evidence, the author thinks the flood began to occur during or after the Miocene; and the process of flooding probably continued, by successive fissure-flows of lava, chiefly in the Cascade and Blue Mountain Ranges, until the Post-Tertiary; the liquid matter having been squeezed out by horizontal and vertical pressure, while water, percolating through the hot mass, generated volcanoes that continued the up-building process.—Dr. Blake of San Francisco has a paper On the Connection between Isomorphism, Molecular Weight, and Physiological Action. One of the conclusions arrived at is, that among compounds of the more purely metallic elements, the quantity of substances in the same isomorphous group required to produce analogous changes in living matter, is less as the atomic weight of the electro-positive element increases.—Mr. Carey Lea describes some experiments made to determine whether it is a general law that when a metallic compound reducible by light is placed in contact with an oxidisable body (or one capable of uniting with Cl, Br, or I, as the case may be), the capacity of reduction of the compound by any particular part of the spectrum is influenced by the *colour* of the body placed in contact with it. But he did not succeed in thus generalising Vogel's results; which, however, he does not regard as contradicted or disproved.—Some experiments by Prof. Wright on the oxidation of alcohol and ether by ozone, seem to indicate that the vinegar process might be materially accelerated by passing ozonised air through the apparatus.—Prof. Marsh communicates a notice (bearing on the genealogy of the modern horse) of new equine mammals from the Tertiary formation; and we further note papers On Recent Dredging Operations in the Gulf of St. Lawrence (Mr. Whiteaves); On Fossils figured in the Illinois State Geological Report (Mr. Meek); On Dissociation of certain Compounds at very low Temperatures (Mr. Leeds), &c.

Der Naturforscher, February.—We may first note, in this number, an account of some valuable researches by MM. Pettenkofer and Voit, as to the significance of the carbohydrates in nutrition. The authors conclude that carbohydrates, in the animal system, always pass entirely into carbonic acid and water, and do not produce fat; but they save (*ersparen*) the fat produced from albumen, and this in proportion to the quantities of the albumen-fat and the carbohydrate. There is also, in the biological department, a succinct statement of Prof. Haeckel's "Gastraea" theory.—In geology, some observations by M. G. Laube appear to indicate that the transport of *débris* and stones by ice in East and West Greenland is by no means a common thing; and a note by M. Albert Heim describes and explains the formation of certain huge cauldron-like cavities in solid rock in the Gletschergarten at Lucerne.—From an examination of plant-remains found in amber, Prof Caspary has inferred that Prussia, in the Amber period, must have been much warmer than now; certain Arctic Ericaceæ, supposed to be of the period, probably flourished on lofty mountains.—M. Merget's recent observations on thermo-diffusion of gas in leaves, and those of M. Reinke on the function of leaf-teeth, are also given; while MM. Fliche and Grandeau study the relation between chemical composition of the ground and vegetation of *Pinus pinaster*. This plant, while a flint-loving species, yet absorbs a considerable quantity of lime; and in soils with much lime, the increased absorption of this salt is accompanied with a decrease in the other ash constituents, especially potash (this being probably the cause of the bad condition of the tree in such soil).—In the department of physics, we have several notes from English sources: On the Elements present in the Sun (Lockyer); On the Affinities of the Magnetic Metals, and On Molecular Phenomena in Glowing Iron (Barrett); On Propagation of Sound in Fog (Reynolds), &c. And in chemistry, there is a note by M. Thomsen, treating of the influence of temperature on chemical phenomena of heat; also a popular summary of M. Ebermayer's researches as to the presence of ozone in the air.—Astronomy is represented by papers on the star shower of November last, and on the direction of the large axes of cometary orbits.

Bulletin de l'Academie Royale de Belgique. No 1, 1874.—In this number M. de Wilde makes some contributions to the theory of bleaching of vegetable fibres which contain incrusting and other matters. He considers that there is substitution of chlorine for hydrogen in the alkaline liquid, which has served to dissolve the incrusting matter, and that chlorine acts, besides, in decomposition of the water, formation of hydrochloric acid, and fixation of oxygen in the organic matter.—The same author communicates notes on the preparation of acetylene, the action of hydrogen on acetylene and ethylene under the influence of platinum black, and the action of the electric effluvia on some gases and gas-mixtures. In the last he confirms MM. Thenard's observations; and acetylene, he finds, is condensed by the effluvia into a liquid which solidifies rapidly, becoming yellow; the solid detonates under the action of heat. Sulphurous anhydride and oxygen combine directly to form sulphuric anhydride.—Continuing his researches on glyceric derivatives, M. Henry describes an octobromide obtained by action of bromine on tetrabromide of dipropargyle; and a paper by M. Spring, describing new syntheses of hyposulphurous acid and of trithionic acid, is of theoretical importance as showing the relations between the sulphates and hyposulphates, and between the latter and trithionates.—

M. Gosselet furnishes a detailed account of the southern band of Devonian limestones in the district Entre-Sambre-et-Meuse; and M. Selys de Longchamps makes some additions to a synopsis of the Cordulina.—A programme of five questions for medal competition is announced, the subjects being briefly these: disturbing causes in determination of the electromotive force and inferior resistance of a battery element; relations of heat to the phenomena (especially periodic) of vegetation; embryonal development of Tunicata; composition and mutual relations of albuminoid substances; coal system of the Liège valley.

Archives des Sciences Physiques et Naturelles, Feb. 15, 1874.—In this number M. Dufour gives a detailed account of his researches on the variation of temperature which accompanies diffusion of gases through a porous partition. After describing the apparatus (in which a porous vessel, with thermometer and other tubes inserted in its gutta-percha stopper, was enclosed in a cylindrical glass vessel, and this, enveloped in loose cotton, in a larger earthen vessel), the author studies first the influence of the dry or humid state of gases coming into contact with the porous wall, without diffusion; next, variation of temperature where there is no change of pressure; and third, variation where there is such change. With constant pressure, there is fall of temperature on the side where the denser gas is; and rise on the other side. Each current seems to have a heating effect where it enters the porous wall and a cooling one where it issues. With change of pressure, where this rises within the vessel, through endosmose of a lighter gas, the temperature slightly increases, sinking again as the pressure tends to equilibrium. Where exosmose of a lighter gas causes diminution of pressure in the vessel, the reverse occurs.—From observations of the partial solar eclipse of May 26 last, at three Italian stations, D'Aoste, Moncalieri, and Florence, Prof. Denza finds no sensible influence on the declination needle, either as regards its regular diurnal movement, or the absolute value of its displacement. He is confirmed in the conclusion, previously formed (on data of former eclipses), that no connection has hitherto been demonstrated between the two orders of cosmic facts, eclipses and phenomena of terrestrial magnetism.—M. Charles Lory communicates a note on some facts of structure in the central chains of the Alps.—The *Bulletin Scientifique* gives, as usual, a valuable series of notes on recent progress in Physics, Geology, Zoology, and other branches.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 26.—On the Motions of some of the Nebulæ towards or from the Earth, by William Huggins, D.C.L., LL.D., F.R.S.

The observations on the motions of some of the stars towards and from the earth, which I had the honour to present to the Royal Society in 1872, appeared to show, from the position in the heavens of the approaching and receding stars, as well as from the relative velocities of their approach and recession, that the sun's motion in space could not be regarded as the sole cause of these motions. "There can be little doubt but that in the observed stellar movements we have to do with two other independent motions—namely, a movement common to certain groups of stars and also a motion peculiar to each star." [*]

It then presented itself to me as a matter of some importance to endeavour to extend this inquiry to the nebulæ, as it seemed possible that some light might be thrown on the cosmical relations of the gaseous nebulæ to the stars and to our stellar system by observations of their motions of recession and approach.

Since the date of the paper to which I have referred, I have availed myself of the nights sufficiently fine (unusually few even for our unfavourable climate) to make observations on this point. The inquiry was found to be one of great difficulty, from the faintness of the objects and the very minute alteration in position in the spectrum which had to be observed.

At first the inquiry appeared hopeless, from the circumstance that the brightest line in the nebular spectrum is not sufficiently coincident in character and position with the brightest line in the spectrum of nitrogen to permit this line to be used as a fiducial line of comparison. The line in the spectrum of the nebulæ is narrow and defined, while the line of nitrogen is double, and each component is nebulous and broader than the line of the nebulæ. The nebular line is apparently coincident with the middle of the less refrangible line of the double line of nitrogen.[*]

The third and fourth lines of the nebular spectrum are undoubtedly those of hydrogen, but their great faintness makes it impossible to use them as lines of comparison under the necessary conditions of great dispersive power, except in the case of the brightest nebulæ.

The second line, as I showed in the paper to which I have referred, is sensibly coincident with an iron line, wave-length 495·7; but this line is inconveniently faint, except in the brightest nebulæ.

In the course of some other experiments my attention was directed to a line in the spectrum of lead which falls upon the less refrangible of the components of the double line of nitrogen. This line appeared to meet the requirements of the case, as it is narrow, of a width corresponding to the slit, defined at both edges, and in the position in the spectrum of the brightest of the lines of the nebulæ.

In December 1872 I compared this line directly with the first line in the spectrum of the Great Nebulæ in Orion. I was delighted to find this line sufficiently coincident in position to serve as a fiducial line of comparison.

I am not prepared to say that the coincidence is perfect; on the contrary, I believe that if greater prism power could be brought to bear upon the nebulæ, the line in the lead spectrum would be found to be in a small degree more refrangible than the line in the nebulæ.

The spectroscope employed in these observations contains two compound prisms, each giving a dispersion of 9° 6′ from A to H. A magnifying-power of 16 diameters was used.

In the simultaneous observation of the two lines it was found that if the lead line was made rather less bright than the nebular line, the small excess of apparent breadth of this latter line, from its greater brightness, appeared to overlap the lead line to a very small amount on its less refrangible side, so that the more refrangible side of the two lines appeared to be in a straight line across the spectrum. This line could be therefore conveniently employed as a fiducial line in the observations I had in view.

In my own map of the spectrum of lead this line is not given. In Thalén's map (1868) the line is represented by a short line to show that, under the conditions of spark under which Thalén observed, this line was emitted by those portions only of the vapour of lead which are close to the electrodes.

I find that by alterations of the character of the spark this line becomes long and reaches from electrode to electrode. As some of those conditions (such as the absence of the Leyden jars, or the close approximation of the electrodes when the Leyden jars are in circuit) are those in which the lines of nitrogen of the air in which the spark is taken are faint or absent, the circumstance of the line becoming bright and long, or faint and short inversely, as the line of nitrogen suggested to me the possibility that the line might be due not to the vapour of lead but to some combination of nitrogen under the presence of lead vapour. As, however, this line is bright under similar conditions when the spark is taken in a current of hydrogen, this supposition cannot be correct.

A condition of the spark may be obtained in which the strongest lines of the ordinary lead spectrum are scarcely visible, and the line under consideration becomes the strongest in the spectrum, with the exception of the bright line in the extreme violet.

I need scarcely remark that the circumstance of making use of this line for the purpose of a standard line of comparison is not to be taken as affording any evidence in favour of the existence of lead in the nebulæ.

Each nebula was observed on several nights, so that the whole observing time of the past year was devoted to this inquiry. In no instance was any change of relative position of the nebular line and the lead line detected.

It follows that none of the nebulæ observed show a motion of translation so great as 25 miles per second, including the earth's motion at the time. This motion must be considered in the results to be drawn from the observations; for if the earth's motion be, say, 10 miles per second from the nebulæ, then the nebula would not be receding with a velocity greater than 15 per second; but the nebula might be approaching with velocity as great as 35 miles per second, because 10 miles of this velocity would be destroyed by the earth's motion in the contrary direction.

The observations seem to show that the gaseous nebulæ as a class of bodies have not proper motions so great as many of the bright stars. It may be remarked that two other kinds of motion may exist in the nebulæ, and if sufficiently rapid, may be detected by the spectroscope. 1. A motion of rotation in the planetary nebulæ which might be discovered by placing the slit of the instrument on opposite limbs of the nebulæ. 2. A motion of translation in the visual direction of some portions of the nebulous matter within the nebula, which might be found by comparing the different parts of a large and bright nebula.

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[*] Proceedings of the Royal Society, vol. xx. p. 392.

[*] Proceedings of the Royal Society, vol. xx. p. 380.

Sir William Herschel states that "nebulæ were generally detected in certain directions rather than in others, and the spaces preceding them were generally quite deprived of stars; that the nebulæ appeared some time after among stars of a certain considerable size, and but seldom among very small stars; that when I came to one nebula I found several more in the same neighbourhood, and afterwards a considerable time passed before I came to another parcel."*

Since the existence of real nebulæ has been established by the use of the spectroscope, Mr. Proctor† and Prof. D'Arrest‡ have called attention to the relation of position which the gaseous nebulæ hold to the Milky Way and the sidereal system.

It was with the hope of adding to our information on this point that these observations of the motions of the nebulæ were undertaken.

In the following list the numbers are taken from Sir J. Herschel's "General Catalogue of Nebulæ." The earth's motion given is the mean of the motions of the different days of observation.

No	h.	H	Others.	Earth's motion from Nebulæ
1179	360	—	M. 42	7 miles per second.
4234	1970	—	Σ. 5	12 " "
4373	—	IV. 37.	—	1 " "
4390	2000	—	Σ. 6	2 " "
4447	2023	—	M. 57	3 " "
4510	2047	IV. 51.	—	14 " "
4964	2241	IV. 18.	—	13 " "

Chemical Society, April 2.—Prof. Odling, F.R.S., in the chair.—A paper on Sulpho-cyanide of Ammonium and Sulphocyanogen, by Dr. T. L. Phipson, and a note On a Reaction of Gallic Acid, by H. R. Procter, were read by the Secretary. Mr. Procter finds that a mixture of gallic acid and potassium arsenate, when exposed to the air, acquires a beautiful green colour.—Mr. W. Noel Hartley then read a memoir On Cobalt Bromides and Iodides, in which he described the method of preparation and properties of these compounds; they closely resemble the corresponding chlorides. Fine specimens of the different salts were exhibited by the author.—Mr. E. Neison read a paper On the Distillation of Sodium Ricinoleate, and Mr. C. H. Piesse a note On the Solubility of Plumbic Chloride in Glycerin.—Mr. Kingzett read a voluminous communication On Ozone as a Concomitant of the Oxidation of the Essential Oils, Part I., and from his experiments he infers that the compound produced during the oxidation of oil of turpentine is neither ozone nor hydrogen peroxide, but a hydrated oxide of turpentine. The last paper was On the Action of Chloride of Benzyl on Camphor, Part II., by Dr. D. Tommasi.

Royal Microscopical Society, April 1.—F. H. Wenham, vice-president, in the chair.—A paper On the Structure of the Lepisma Scale, by Dr. Anthony, was read to the meeting, in which the author showed that the two sets of markings were upon opposite sides of the scale, the ribs being upon the under side.—Mr. Wenham gave a demonstration of his method of measuring the angular apertures of objectives, and explained his mode of stopping out the extraneous rays which were so frequently a cause of error.—Mr. S. J. McIntire read a paper describing the proboscis of a moth (believed to be a South African species) which was furnished with a means of perforating the nectaries of flowers. A mounted specimen was exhibited under one of the Society's instruments in the room, and drawings in illustration of the paper were placed upon the table.

Linnean Society, April 2.—Mr. J. Gwyn Jeffreys, F.R.S., in the chair.—On the Morphology of the Skulls in the Woodpeckers (*Picidæ*) and the Wrynecks (*Yungidæ*), by Mr. W. Kitchen Parker, F.R.S. The present paper is one of a series in hand in which the writer has endeavoured to work out thoroughly the facial characters of certain types of birds in harmony with the views given by Prof. Huxley in his well-known paper on the Classification of the Feathered Types (Zool. Proc. April 11, 1867). His own mode of research is much more like that followed by the distinguished author of that paper than that pursued by ornithologists proper. Without undervaluing their excellent labours, there are many things which are seen first and first understood by the embryologist and not by the zoologist as such. Prof. Huxley, in the paper just referred to, separated the forms now under consideration into his group Coleomorphæ, and gives (p. 467) a very valuable summary of their characters. It was sought in that paper to bring into more or less zoological contiguity such birds as have a similar structure of the facial, and especially of the palatal, bones. Those group-terms, the Schizognathæ (p. 426), the Dromæognathæ (p. 425), &c., are very important, although some of them are of very wide application. It was the first thought of the author of this paper that the woodpeckers would easily find a place amongst the *non-passerine* aerial birds; but examination of their palatal structures soon dispelled this opinion. They are more allied to the Passerinæ than most of the Zygodactyles; but it is to the *embryos* of that type, and not to the adult, that they are related. The Passerinæ themselves are well termed ægithognathous (p. 450); this huge group is under hand at present. Most of the non-passerine birds that seem to come nearest to the woodpeckers have a very solid palate; they are desmognathous; others, as the humming-birds and goatsuckers (*Caprimulgus*), are schizognathous; whilst the swift (*Cypselus*) is as perfectly ægithognathous as the swallows. But the woodpeckers retain that non-coalescent condition of the palatal structure which we see in the lizards, very unlike that great fusion of parts towards the mid-line which occurs in most of the higher birds. They also have an unusually arrested condition of the palatal part of the upper jaw-bone (maxillary), which is characteristic of the lizard and unlike the bird-class generally, and bones superadded to the palate, vomers, septomaxillaries, &c.; these are persistently in paired groups, more in number, and altogether more evidently embryonic and Lacertian, than the homologous parts of other birds. The writer therefore seeks to introduce a new morphological term for these birds as a group, having relation to their face, namely, the term Saurognathæ; for none of Prof. Huxley's terms are appropriate for this type of palate. The writer has worked out these parts in the nestling of *Yunx torquilla*, in four stages of *Gecinus viridis*, in the young of *Picus minor*, and in the adult of *P. major*, *P. analis*, *Hemilophus fulvus*, and *Picumnus minutus*.

MANCHESTER

Literary and Philosophical Society, March 3.—*Physical and Mathematical Section.*—Alfred Brothers, F.R.A.S., president of the Section, in the chair.—Results of Rain-Gauge Observations at Eccles, Manchester, during the year 1873, by Thomas Mackereth, F.R.A.S.

March 10.—Ordinary Meeting.—E. W. Binney, F.R.S., vice-president, in the chair.—The chairman said that at a meeting of the Society on January 9, 1872, in presenting to the notice of the members specimens of fossil woods from the lower coal measures of Lancashire, he stated "that from some examples in his cabinet he was led to believe that Cotta's *Medullosa elegans* was merely the rachis of a fern or a plant allied to one." Prof. Renault, of Paris, to whom we owe so much for his researches in fossil botany, read a memoir before the French Academy on January the 26th last, which has since been printed in the *Comptes Rendus*, that completely confirms this opinion.—Further Observations and Experiments on the Influence of Acids on Iron and Steel, by William H. Johnson, B.Sc. At the last meeting of the Society Prof. Reynolds, in an interesting paper On the Effect of Acid on the Interior of Iron Wire, appears to think that Mr. Johnson did not attribute to hydrogen any portion of the remarkable change produced in iron and steel by immersion in acid. That immersion in acid is the primary cause no one, Mr. Johnson thinks, will dispute; but that hydrogen plays an important part in producing these changes and is the cause of the bubbles, the author showed in a paper read before the Society, March 4, 1873. The supposition that the absorption of hydrogen is the sole cause of the change in the breaking strain, diminution in toughness, &c., attendant on the immersion of iron in hydrochloric or sulphuric acids, and that there is no absorption of these acids into the interior of the iron, does not account for a number of phenomena that have been observed so often and so carefully as to leave no doubt of their invariable recurrence if the conditions of experiment be only properly observed. It seems to the author that the only satisfactory way of explaining all the phenomena is to suppose that when a piece of iron is immersed in acid two actions go on, viz.: an absorption of the nascent hydrogen into the interior of the iron, which hydrogen may subsequently be given off by gentle heat or immersion in a liquid, &c. Secondly, an absorption of the acid itself, possibly in a very concentrated form, by the interstices between the fibres or crystals of the metal. It will however be said, the acid must act on the walls of the cavity and form a salt of iron with liberation of hydrogen. This may go on to a small extent, but in opposition to this view we may bring the experiments of Prof. Bequerel on solutions separated by a cracked tube (*Comptes Rendus*, lxxvi.), where he shows that no precipitate is formed on placing a cracked tube filled with nitrate of lead in a solution of potassium sulphate within the crack, thus making it probable that chemical interchanges do not take place in very minute spaces. By this theory we may easily explain the decrease in toughness after immersion in acid. For toughness implies a certain ease of mobility of the particles. When a piece of iron is bent the particles of one side are compressed, thus diminishing the minute cavities between the fibres, while those of the other side are stretched, and the minute cavities elongated. If we fill these cavities with a liquid this mobility of the particles is prevented, for the cavities cannot now be diminished in size and the compression of the one side cannot now take place, consequently the piece tears or breaks off just like a piece of frozen rope. It will also explain the acid reaction of the moistened fracture, and further, as hydrochloric acid is much more volatile and of less specific gravity than sulphuric acid, it is only natural to expect that the effect of immersion in hydrochloric acid will pass off more rapidly than of immersion in sulphuric. This experience fully confirms. The author then gives details of a number of experiments and their results bearing on the point under discussion.—Results of certain Magnetic Observations made at Manchester during the year 1873, by Prof. Balfour Stewart, F.R.S.

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* Philosophical Transactions, 1784, p. 448.

† "Other Worlds than Ours," pp. 280–290

‡ "Astronomische Nachrichten," No. 1908, p. 190.

GLASGOW

Geological Society, March 12.—A paper was read On some Polyzoa from the carboniferous limestone shales near Glasgow, by Prof. Young, F.G.S., and Mr. John Young, vice-president. The authors described a new genus which they had established under the name of *Rhabdomeson*, and which includes at least two species hitherto referred to *Ceriopora*, namely, *C. gracilis* and *C. rhombifera*. The authors also described and exhibited specimens of other two species of polyzoa, the one having the habit of a *Fenestella*, the other of a *Glauconome*, but both showing the remarkable peculiarity of a series of eight denticles projecting horizontally over the cell aperture. For the fenestrated species, they proposed to constitute a new genus—*Stellipora*. The other they retained, meantime, in the genus *Glauconome*.—Mr. Robert Craig read a paper, the first of a series, On the Fossils found in the carboniferous beds around Beith and Dalry, with special reference to the position of their first appearance in the beds. These beds, he remarked, are highly fossiliferous, and occur in the following general order:—(1) Lower limestone, resting upon volcanic ash, 17 fathoms; (2) coal and ironstone measures, resting upon the lower limestone, above 100 fathoms; (3) upper limestone, taking the Swindridge or Highfield "post" as the lower stratum, about 65 fathoms. Mr. James Dairon read a paper on a new species of Retiolites (*Retiolites fibratus*) found by him last summer in the Moffat shales of the Lower Silurian system of the South of Scotland.

PARIS

Academy of Sciences, March 30.—M. Bertrand in the chair. M. de Quatrefages presented to the Academy the second part of his work (written in conjunction with M. Hamy, assistant naturalist in the Museum) entitled "*Crania ethnica*. The skulls of the human races." The author made additional remarks on fossil human races, calling attention in particular to the race of Cro-Magnon. The characters of this race are well exemplified in the male and female remains discovered at Cro-Magnon in 1868. The male skull is remarkable for its capacity, gauging, according to M. Broca, not less than 1,590 cent. cubes, a number sensibly above the mean of all European populations. With the Cro-Magnon remains the authors class several other specimens of human fossils from the same valley of Vézère, from Bruniquel (caves of Lafaye and Forges), from the south towards the Pyrenees (the cave of Aurignac), and from the cave of Gourdon near Montréjean. The same race is traced in the Menton skeleton and beyond the Alps in the Cantalupo skulls and in that from Isola del-Liri. In France again male skulls of the same race have been excavated in Mâconnais and Grenelle, while Liège has furnished the celebrated Engis skull. During the quaternary epoch it appears therefore that the Cro-Magnon race had its head-quarters in the south-west of France, particularly in the valley of Vézère, where the intellectual development can be traced from station to station, possibly to the confines of civilisation. The authors think it probable that the earliest representatives of the race will be found in Africa.—M. Pasteur made some verbal observations on M. A. Guerin's recent communication on the pathogenetic rôle of ferments in surgical maladies.—On an apparatus invented by M. Moncoq for the operation of transfusion of blood, by M. Bouley.—On the hydrometric service of the basin of the Seine, by M. Belgrand.—MM. Daubrée and Brongniart presented a report on M. Renault's memoir, entitled "Study of the Genus *Myelopteris*." The reporters consider the conclusions arrived at of sufficient importance to warrant the publication of the memoir in the collections from foreign *savants*.—On the integration of equations to the partial derivatives of the second order, by M. A. Picart.—On the artificial production of the phenomena of gaseous thermo-diffusion of leaves by means of moist porous and pulverulent bodies, by M. Merget. The author concluded by observing that the dynamical utilisation of thermo-diffusive forces would resolve in a simple manner the problem of the direct transformation of solar heat (energy ?) into mechanical work.—On some general facts which arise from comparative androgenesis, by M. A. Chatin.—Observations on the disposition of the fibro-vascular bundles in leaves, by M. J. L. de Lanessan.—On a method of photographic enlargement for astronomical observations, by M. C. Zenger. The method proposed is likely to be of service in photographing the forthcoming transit of Venus. The author uses a mirror of long focus instead of a lens to produce the sun's image, and to prevent errors of irradiation and inflexion, proposes to photograph the planet at its moment of passage across a point *h* of a particular meridian of the sun. The enlarging process suggested corrects aberration in the original photograph.—On an electro-automatic whistle for locomotives, by MM. Lartigue and Forest.—On the employment of luminous signals in geodesic operations, by M. Laussedat.—The analysis (mathematical) of an armed and closed electro-magnetic circuit proves that electric induction does not traverse conducting masses, by M. P. Volpicelli.—On the movement of air in pipes, by M. C. Bontemps.—On the action of ammonia on acetone, by MM. Œchsner and Pabst. The authors believe that the reaction gives rise to Staedeler's *acetonine*.—On Egyptian blue, by M. H. de Fontenay.—Experimental researches on the influences which changes of barometrical pressure exert on the phenomena of life, 13th note, by M. P. Bert.

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THURSDAY, APRIL 16, 1874

THE ADAPTATION OF OUR UNIVERSITIES TO THE WANTS OF THE AGE

IT has given us special pleasure during the last few years to record the efforts made in several of our British Universities and Colleges to adapt their teaching and their appliances for teaching to the present state of knowledge. We have seen what has been done by means of a fraction of the splendid revenues of Oxford, what the princely munificence of her Chancellor is providing for Cambridge, and what public subscriptions aided by judicious liberality on the part of Government have enabled Glasgow to achieve. Let us see what is now being attempted by a University which, though for its years rich in usefulness and fame, even relatively to those just mentioned, is, so far as funds are concerned, in a state approaching to indigence.

A short paragraph in our last number called the attention of our readers to the important step which has just been taken by the University of Edinburgh, with the view of thoroughly adapting its lecture-rooms and laboratories at once to the enormously increased numbers of its teachers and students, and to the ever-growing demands of physical and biological science.

Increase of numbers of teachers and taught would of itself demand a proportionate increase of space, which in Edinburgh must be considerably more than two to one as regards lecture-rooms alone. But when we consider the improvements which have been introduced into the modes of teaching, the imperative necessity for *practical* instruction in addition to lectures and demonstrations; nay more, the desirability of enabling professors not merely to teach what is known, but also, by original research conducted by themselves and their more promising students, to endeavour to extend the boundaries of Science: we see how immense are the issues involved in the step which Edinburgh has just taken.

That it will be successful, no one who knows Scotland and Scotsmen can for a moment doubt. But Scotland is a comparatively poor country—receiving back from the Treasury a much smaller fraction of her taxation than more favoured portions of the empire—and Edinburgh University is, relatively to the number of her students, by far the poorest of even the poor Scottish Universities. It is to be hoped, therefore, that Government aid will be forthcoming, as in the recent case of Glasgow, to eke out the efforts of those who, with as good a cause as could be wished for, and hearty desire to advance it, yet cannot entirely rely on the results of their unaided exertions. All former Edinburgh *alumni*, scattered as they are broadcast over the world, especially in England and in India, must be prepared to acknowledge, by such contributions as they can afford to make, the value of the instruction they have received. Let no one abstain from giving because of the smallness of the sum he can afford; every mite is of importance—let him rather rejoice that he has the opportunity, which appears to occur but once in a century, of contributing to so noble an object.

The story of her last successful endeavour to meet wants to a certain extent akin to those now felt is well told in the following extract from the Programme of the Acting Committee:—

"One hundred years ago, an Appeal was made to the public on behalf of the University of Edinburgh. The number of students was then stated as 'betwixt six and seven hundred,' the inadequacy of the collegiate buildings to the size and importance of the University was pointed out, and it was declared that while in Edinburgh great improvements were going forward on all hands, 'the University fabric alone' remained 'in such a neglected state, as to be generally counted a dishonour to the City of Edinburgh, and to this part of the kingdom.'

"The result of that Appeal was a liberal public subscription, opened in March 1768, which, with the aid of Government, provided the handsome edifice now existing. That building for a long period amply sufficed for all the teaching purposes of the University. But the lapse of a century has produced great changes. During that period the population of the metropolis has been more than trebled; increased facilities for travelling have brought the University within easy reach of all parts of the country; the advantages of a University education have become much more appreciated; the advancement of Science has widely extended the field of academic teaching; and the renown alike of teachers and graduates, whose names will ever be associated with the University of Edinburgh, has increased its fame and reputation throughout the world.

"Thus, the buildings of the University again prove to be wholly inadequate to its necessities. This inadequacy is felt in various ways.

"The number of students attending the University in 1768 was 'betwixt six and seven hundred,' and the number of Professors was 21. In the present Session (1873-4) the number of students is between 1,900 and 2,000, and that of the Professors is 35. The Class-room accommodation has thus become wholly insufficient. The students at present attending the Chemistry, Anatomy, and Natural History Classes number about 300 in each case. The Lecture-rooms are consequently much overcrowded, and great personal discomfort is thus occasioned to both the teachers and the taught.

"But apart from the present buildings being insufficient as regards the students in attendance, the nature of the modern system of teaching in several branches has rendered the existing accommodation altogether unsuitable.

"Since the present University buildings were erected, the whole subject of Practical Chemistry has been added to the course of study. Within the last ten years large and commodious laboratories have been provided in connection with many of the European Universities, and it would be most unfortunate if the University of Edinburgh, which was the first British school to introduce practical instruction in Chemistry into the medical curriculum, were not enabled to carry on satisfactorily this important branch of medical and scientific training.

"Again, the instruction formerly given in Anatomy consisted almost entirely of lectures and demonstrations delivered in the class-room. The changes in Medical education during the last thirty years render it necessary that each student should now pursue for himself the study of Practical Anatomy. The rooms at present in use were not constructed for that purpose, and are lamentably inadequate for the work to be done in them.

"But besides the departments of Chemistry and Anatomy, increased accommodation, in the form of Laboratories, and rooms suited for microscopic and other practical investigation and instruction, is required for the Chairs of Materia Medica, the Institutes of Medicine, Natural History, and Pathology. Nor is it less urgent that much additional accommodation for the apparatus and the Physical Laboratory of the Chair of Natural Philosophy should be provided."

Take these in turn—the last-mentioned first. The Physical Laboratory has been but six years in existence; simply because it was impossible sooner to find any accommodation for it. One small room was obtained capable of holding a dozen students (at very high pressure). The success of the first year was so great that in the next session more than half of the applicants had to be refused admission; and as the demand grew, the working time allowed each student per day had to be further and further restricted, till, in the session just concluded, the lowest admissible limit (*one hour per day*) had at first to be adopted, and yet several applications for admission had to be refused. In spite of these drawbacks, much sound work has been done, and many of the Laboratory students have already obtained excellent posts connected with Astronomy, Telegraphy, Engineering, Sugar-refining, &c., mainly on account of the training they have received. The good thus done is to be measured, not by the mere fact of the success of these men in life, but by the fact that their success introduces into practical observatories, workshops, &c., men who have learned the reasons for the manipulations they employ, and who can therefore meet an emergency in ways which no rule-of-thumb teaching could possibly have suggested.

In Anatomy and Chemistry, practical teaching has long been established, and is afforded to every medical student and to such others as study these subjects as parts of a general scientific training. But it is necessary that a great deal more should be done in this direction, especially in the way of affording to advanced students opportunities of cultivating their own powers, and furthering Science by original research. The present arrangements render this possible only to a very limited extent.

Although practical instruction in Physiology, Pathology, and Pharmacology have not formed for so long a period as in Anatomy and Chemistry an integral part of a medical curriculum, yet the University authorities have recognised its importance and have introduced it as far as the meagre space at their disposal would admit. But the increasing demand for a practical training has overcrowded these rooms and made it imperative that additional accommodation should be provided, not only for tuition but for self-training and discovery.

Thus all the practical departments in both the physical and biological sciences urgently demand additional houseroom.

In conclusion, we would again call attention to the fact that one of the great reasons for the present appeal is to be found in the immense success of the University; its mere numerical growth has far exceeded the accommodation provided. But we would also specially note the fact that, although the scheme has just been launched, the contributions already received or promised amount to the very handsome, though of course utterly inadequate, sum of 60,000*l.* At least 40,000*l.* more, with the equivalent which may reasonably be expected from Government, are required to give us yet another University, furnished at least with buildings which will enable it to preserve for another century its well-deserved but hardly-won fame.

But it must not be forgotten that buildings alone, however perfect, are not sufficient for the work desired. The further extension of the teaching staff must inevitably follow. But questions of this nature, as well as the annual supply of funds for the purchase of apparatus and materials, will, we hope, be effectively treated by the Royal Commission on Science, whose Report on the Scottish Universities, and whose proposals for their adequate endowment, are, in the North at least, anxiously expected.

SCHORLEMMER'S "CHEMISTRY OF THE CARBON COMPOUNDS"[*]

A Manual of the Chemistry of the Carbon Compounds; or, Organic Chemistry. By C. Schorlemmer, F.R.S., Lecturer on Organic Chemistry in the Owens College, Manchester. (London: Macmillan and Co, 1874.)

JUDGING from the rapidity with which text-books on Organic Chemistry have made their appearance of late, it might reasonably be inferred that a good treatise on that subject is much wanted. The student who turns eagerly to the present manual in the hope that the eminent author will help him out of some of his difficulties, and that he will find the subject treated in a novel manner, will however, we fear, feel somewhat disappointed.

The classification adopted by the author deals first with the compounds of carbon with oxygen, sulphur, and nitrogen; compounds which form the connecting link between inorganic and organic chemistry. He considers, justly, that a knowledge of the compound radicals into which these elements enter is essential to a proper understanding of a large number of other carbon compounds. He then describes the large group of fatty substances, subdivided again according to the quantivalence of their radicals, as well as the carbohydrates, terpenes, and camphors. The next division comprises compounds richer in carbon than the fatty substances, and which are not converted into such by the addition of hydrogen. These are again subdivided into several groups, including that of the aromatic compounds, which has been most fully investigated, and the group of compounds containing two or more aromatic nuclei linked together by carbon, and the glucosides. Lastly, we have a division of artificial and natural bases (alkaloids), of colouring and bitter principles, of compounds contained in bile and other secretions of the animal body, and of albumenoids and proteids.

It will be seen from this brief synopsis that the author deviates for the most part from the arrangement which has found favour with many modern writers on Organic Chemistry. Rather than treat of well-defined families of organic bodies, such as hydrocarbons, alcohols, ethers, aldehydes, ketones, acids, &c., he prefers to retain groups of homologous series, together with their derivatives. To the student this arrangement has the decided drawback that it involves much repetition in examining chemical changes, and, what is more important, it does not enable him to take in at a glance in what consists the similarity or dissimilarity between classes of bodies of analogous structure, and derived from a homologous parent stock; nor is it so easy to see where one or several links in the various homologous series are missing. The reward of grappling with the intricacies of organic bodies to which each worker is entitled who adduces new facts—no matter to how limited an extent—consists just in the intellectual treat of supplying perhaps some of these many missing links. There can be no question which system of classification will assist him to do this most speedily.

A student's perplexity will not be diminished on following our author into the vexed question of formulæ. We take it for granted that chemical formulæ have been devised to express the phenomena produced by the action of chemical force. That they express at present the final results rather than the agencies and forces which have been at work to produce them may likewise be taken as correct. They are, at the very best, poor representations only of the chemical changes which we witness daily. Whether it will ever be possible to prove that the atoms or groups of atoms of which chemical compounds are supposed to consist really stand to each other in certain definite relations, because they exhibit certain analogies under the influence of the chemical force which holds them together or loosens them, may well remain matter for speculation. As long as no differences of opinion respecting chemical facts are involved, views may differ on the mode of expressing them by formulæ, provided always that the choice between two different modes of expression falls upon the one which recalls the greatest number of analogies in the most simple and rational manner, and can therefore become more fruitful in new discoveries. Several rational formulæ are possible for one and the same compound according as different relations are to be expressed. A knowledge of the order or position of the atoms is out of the reach of experimental demonstration. If the so-called position-theory of the carbon atoms in certain organic compounds can assist us, however, in elucidating certain relations and analogies, and if this theory is leading rapidly to numerous new discoveries, then, by all means, let us avail ourselves of it. It seems that constitutional, or, as they are sometimes called, structural formulæ excite the ire of some of our critics of chemical literature inversely to the understanding they display of them, and the very name constitutional formula, or the sight of a graphic representation thereof, inflames their fury as violently as a red cloak excites an infuriated bull. However much ridicule may attach in the eyes of some people to constitutional or structural representation, it is pleasant to observe that the chemists who now dispense with it altogether form the exception rather than the rule.

Mr. Schorlemmer makes an especially liberal use of structural formulæ, showing the relative position and units of combining capacity of carbon atoms, with the view mainly of explaining isomeric bodies. Every student of organic chemistry will thank him for this, for, on perusing the journal of the Berlin Chemical Society, for instance, one cannot help being struck with the remarkable impulse which the conception of structural representation of the chemical composition of bodies has given to the study of organic chemistry in Germany. Chemists find it, no doubt, difficult to disengage themselves entirely from some of the various theories that have held sway during the last twenty years, and hence we look with leniency upon the want of uniformity of formulæ and chemical nomenclature displayed in this book. On pp. 3, 4, and 5, for instance, the brace is used in a double sense, showing the formation of molecular bodies by the direct combination of element with element as in $\left.\begin{matrix}H\\H\end{matrix}\right\}$, $\left.\begin{matrix}H\\Cl\end{matrix}\right\}$, $\left.\begin{matrix}K\\I\end{matrix}\right\}$; and again by uniting two or more elements with a polyvalent element, as in $\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N$, without any connection existing between the monad elements themselves other than through the polyad element. Such names as Tin chloride, $SnCl_4$, Platinum chloride, $PtCl_4$, and others, must create the impression that these are the only compounds which tin, &c. forms with chlorine. The different atomic groups in structural formulæ are sometimes separated by points, sometimes by lines (forks, prongs, or whiskers, as some fastidious critics have called them). In the absence of these various graphic representations free use is made of molecular formulæ; indeed it strikes us that the author has been often over-cautious, and has not attempted constitutional formulæ where such representation would appear of particular interest to the student, as, for instance, in the case of the isomers of aldehyde, of aldines, &c.

Certain groups of atoms contained in a great number of organic bodies, such as nitroxyl, NO_2, sulphuryl, SO_2, phosphoryl, PO, *may* be viewed otherwise than as monad, dyad, or triad compound radicals. Why, for instance, should PO in phosphorous acid be a triad radical when one of the hydrogen atoms is not replaceable, but "remains together" with the PO group? or in hypophosphorous acid, where two atoms of hydrogen remain linked to PO? Considering the liberality displayed by the author regarding formulæ, we shall be pleased to see him shake off the trammels which still encumber his inorganic compounds. We cannot see why NO_2 should combine with OH to form nitric acid, and with H to form nitrous acid, or why SO_2 should form the compound radical in $SO_2\left\{\begin{matrix}Cl\\Cl\end{matrix}\right.$ and in sulphuric acid $SO_2\left\{\begin{matrix}OH\\OH\end{matrix}\right.$, and also in sulphurous acid $SO_2\left\{\begin{matrix}H\\OH\end{matrix}\right.$. Our author writes ethyl nitrite $C_2H_5O.NO$, and not $C_2H_5NO_2$, "because in the former the N is linked to the ethyl by means of an atom of oxygen, whilst in the isomeric nitro ethane $C_2H_5N\left\{\begin{matrix}O\\O\end{matrix}\right.>$ the nitrogen is linked directly, and the oxygen atoms satisfy each other." We need scarcely say that this latter compound may also be viewed differently. We might greatly extend the list of similar incongruities, taking our examples especially from the organic silicon and boron compounds, which, more than anything else, show that the same idea of grouping elements that is now so freely admitted to prevail for carbon compounds must logically hold good also for inorganic bodies.

The nomenclature first proposed by Hofmann and adopted by our author of designating the different parallel hydro-carbon series by the terminations, ane, ene, ine, one, une has some inconveniences which perhaps are less apparent to the teacher than to the student of organic chemistry, who must be sorely puzzled to distinguish, for instance, between ethine and phosphine, stibine, oxytetraldine, &c.; and it is with regret we see some authors go even further, and substitute, for instance, iodethane for the familiar ethyl iodide, &c.

As constitutional or structural formulæ are intended to assist the student in the study of organic chemistry, we should have preferred if the well-understood abbreviations for compound radicals, advocated by some of our most eminent chemists, such as Et. for ethyl, Ay. for amyl, had been used. We observe, on p. 53, that Cfy. and Cfdy. are used for the compound cyanogen radicals; why should not constitutional formulæ generally be simplified by the use of abbreviations? The task of deciphering certain complex organic formulæ is already heavy enough, and some such shorthand expressions as the above will soon become all but indispensable. We do not for a moment blame the author alone for these sins of omission and commission. Our nomenclature and terminology are in such a state of confusion that a bold reformer should be welcomed rather than discouraged by every lover of our science.

We notice a few slips: On p. 7 “$C_n H_{2+2}$” should be $C_n H_{2n+2}$; on p. 13 “monad and triad radicals cannot be isolated;” but at top of p. 14 we are told that “methyl combines with methyl, and we obtain ethane or ethyl hydride.” Why methyl, &c., should not exist in a free or molecular state as much as hydrogen we are unable to see. On p. 67, the oxygen in the formula for guanidine should be omitted. On p. 72, “methyl iodide $2CH_3$” should be $2CH_3I$. On p. 111, “The vapour of ether is 2·557 times heavier than water.” On p. 112 “triacetyl chloride” should be trichloracetyl chloride. On p. 135, “$C_6 H_{12} N$” is given in the equation, instead of $C_6 H_{12} N_4$. On p. 309, “$C_2 H_3 NH_2$” should be $C_2 H_5 NH_2$; and others which we will not mention.

We freely admit many commendable features in Mr. Schorlemmer's new book, which will render it extremely useful, especially to the student engaged in tracing the various isomerides, but we cannot help thinking that in some respects it does not come up to some works on organic chemistry which we already possess.

OUR BOOK SHELF

Dahomey as it Is: being a Narrative of Eight Months' residence in that country, with a full account of the notorious Annual Customs and the Social and Religious Institutions of the Ffons; also an Appendix on Ashantee, and a Glossary of Dahoman Words and Titles. By J. A. Skertchly. (London: Chapman and Hall, 1874).

MR. SKERTCHLY left England in 1871 for the purpose of making zoological collections on the West Coast of Africa. On his arrival at Whydah he was induced to go up to Abomey, the capital of Dahomey, for the purpose of instructing the king, Gelelé, in the use of some guns that had arrived, on the promise that he would be back at Whydah in eight days. The king, however, detained Mr. Skertchly as an unwilling guest for eight months, treating him with the greatest consideration and kindness, and creating him a prince of the country. The greater part of Mr. Skertchly's work is occupied with a description of the protracted annual “customs,” as they are called, of Dahomey, which consist of elaborate and harmless trivial ceremonies, mixed up with much that is revolting and cruel; the details of these Mr. Skertchly describes in minute and often tiresome detail. We do not think there was any need for Mr. Skertchly making so large a book on what he saw, especially as the Dahomans and their “customs” are pretty well known through previous travellers. He often questions the accuracy of Burton, who is quite able to defend himself if he feels aggrieved at Mr. Skertchly's criticisms. The author succeeded, during his stay at Abomey, in doing but little in the way of collecting, and in this work there is scarcely any details as to the natural history of the country. He has evidently a considerable admiration both for the Dahomans and Ashantees, especially for the former, whom he considers not nearly so cruel as the latter, though both equally brave and remarkably well-disciplined as soldiers. In a short Appendix on the Ashantees, he prophecies that our recent expedition to the Gold Coast would find them formidable enemies, which prophecy can hardly be said to have been fulfilled. He defends the Dahomans from the charge of intentional cruelty in the barbarously performed human sacrifices which form so important a part of their customs, and we think he succeeds; the victims, who are all either criminals, or prisoners of war, are sent as messengers to deceased kings. The work is illustrated with a number of gorgeously coloured plates, which no doubt show faithfully the dresses and manners of the people, though some of the pictures which exhibit the method of sacrificing the human victims are simply revolting, and ought to have been confined to the author's portfolio.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Fertilisation of the Fumariaceæ

I BEG permission to make a few remarks on Mr. J. Traherne Moggridge's statement (NATURE, vol. ix. p. 423) that the flowers of *Fumaria capreolata* are at first pale or nearly white, and only attain their brightest colouring, becoming even crimson, after the ovaries are set. He then adds:—“If the reverse had been the case there is little doubt that we should have regarded the bright colouring as specially adapted to attract insects.” But does Mr. Moggridge know that these flowers are visited chiefly by diurnal insects? It has often been observed that flowers which are visited by moths are commonly white or very pale; but if they are odoriferous, they may be of any tint, even very dark or green. If therefore the flowers of the above Fumaria are visited by moths, it would be an injury to the plant had the flowers been from the first of a fine crimson. I have often seen bees sucking the flowers of the fumariaceous genera, *Corydalis*, *Dielytra*, and *Adlumia*; but many years ago I watched perseveringly the flowers of *Fumaria officinalis* and *parviflora*, and never saw them visited by a single insect; and I concluded from reasons which I will not here give (as I cannot find my original notes), that they were frequented during the night by small moths. Insects are not necessary for the fertilisation of *Fumaria officinalis*; for I covered up a plant, and it produced as many seeds as an uncovered one which grew near. On the other hand, with some species of *Corydalis*, the aid of insects is indispensable. With respect to the flowers of *F. capreolata* becoming brighter coloured as they grow old, we see the same thing in some hawthorns, and with the double rocket in our gardens. But is it surprising that this should sometimes occur with flowers, seeing that the leaves of a multitude of plants assume, as they become oxygenised, the most splendid tints during the autumn?

Down, Beckenham, Kent, April 6 CHARLES DARWIN

IN the vegetable kingdom we meet very commonly with gaily-coloured chemical products, essentially connected with the normal processes of development (the chlorophyll of most non-parasitic plants, the splendid rose pigments of Florideæ, the many lively-coloured pigments of lichens and fungi), and originating from venomous infection by insects (red-coloured galls of oak-leaves) or from decomposition (red pigments in autumnal leaves). In all these cases these colours appear to us to be merely an accidental quality of the chemical products, and we do not feel induced to start the question of what use any particular colour may be to the plant producing it. But it is quite otherwise with the gay colours of flowers. Bright colours in flowers which especially attract our attention and admiration are in most cases beneficial to the plant itself which produces them, by attracting in like manner also the attention of insects, which, visiting the flowers for their own profit, at the same time unconsciously bring to the plant the great advantage of cross-fertilisation. Hence we understand that bright-flowered varieties, whenever produced by any cause, might be preserved by natural selection, and at last remain the only survivors among all the concurrents of the same species. Thus, the occasional appearance of gaily-coloured varieties granted as a matter of fact, and the peculiarities of colour supposed to be hereditable, we are enabled by Darwin's theory to explain the variety of colours met with in flowers. But we should always bear in mind that we are at present quite ignorant of the chemical processes by which certain colours are produced in the flowers, and of the physical or organic causes by which these chemical processes were effected when they first appeared and are effected in every subsequent generation. Reflecting on the first origin of the adaptation of flowers to the cross-fertilisation by insects, and considering that the oldest and most primitive phanerogamous plants which still exist, the Gymnospermæ, are exclusively fertilised by the wind (are *anemophilous*), whilst the enormous majority of Angiospermæ is provided with flowers adapted to cross-fertilisation by insects (*entomophilous*), we cannot doubt that the original manner of fertilisation of phanerogamous plants was fertilisation by the wind, and that the first plants which adapted their flowers to cross-fertilisation by insects were anemophilous ones, either Gymnospermæ or the next descendants of them. Nevertheless the flowers of many Gymnospermæ (Abietineæ) present a beautiful colour, which attains its culmination during the disseminating of the pollen.[*] This beautiful colour is apparently neither of any use to these plants, which are regularly cross-fertilised by the wind, nor can have been inherited from ancestors to which it was useful. We may therefore also in this case, without hesitation, regard the colour as a merely accidental phenomenon, which, secondarily produced by the more active chemical processes during the time of flowering, disappears again in the same degree as the intensity of development decreases in the cones. Probably the gaily-coloured perianths of the entomophilous Angiospermæ have originated in a similar manner.

Independently of possible physical effects, natural selection is evidently without any influence as to colours, unless animals are attracted or repelled by them. Consequently not only the first origin of bright-coloured flowers, but also the change of colour in the flowers after the ovaries are set, is altogether foreign to the effects of natural selection. It is as indifferent to an entomophilous plant whether its flowers, after having been fertilised, grow paler or darker, as it is to an anemophilous plant whether its flowers are attractive to insects or not. In most cases, indeed, flowers change while fading into paler and less conspicuous colours, but often also their colour remains unaltered or even grows more conspicuous. Old flowers of *Melampyrum pratense*, for instance, which, not having been cross-fertilised by insects, regularly fertilise themselves, are always reddish-yellow, whilst younger ones are yellow.

As to *Fumaria capreolata*, alluded to in Mr. Moggridge's letter (NATURE, vol. ix. p. 423) I have never had the opportunity of observing its flowers myself, but from Hildebrand's account ("Jahrb. f. wissensch. Bot." vii. p. 452) I believe that it is restricted to regular self-fertilisation, cross-fertilisation by insects not, indeed, being impossible, but taking place very exceptionally; for it has lost, probably from permanent disuse, the elasticity of the cap formed by the inner petals, which in other fumitories secures cross-fertilisation in case of the repeated visits of insects. If this presumption of mine be right, it would the more explain Mr. Moggridge's observation; for in this case the colour of the flowers of this fumitory, inherited from ancestors to which it was quite useful, would be almost useless to this degenerated descendant, and therefore almost withdrawn from the influence of natural selection.

HERMANN MULLER

Lippstadt, April 4

Conference for Maritime Meteorology

SOME of your readers may have noticed in the Report of the Proceedings of the Meteorological Congress at Vienna that it was decided to be advisable to convene a fresh Conference for maritime meteorology, in order to reconsider the decisions of the Brussels Conference in 1853.

The matter was handed over to the Permanent Committee, and by them delegated to a sub-committee composed of the following members:—

Prof. Buys Ballot (Holland)
Prof. Mohn (Norway)
Capt. E. Mouchez (France)
Dr. G. Neumayer (Germany)

with myself.

The sub-committee have nearly decided on a form of programme for the proceedings, and there are hopes that the Conference will meet in London in the month of August or so. Endeavours will probably be made to induce H.M.'s Government to issue the invitations, and thereby to give an official character to the Conference.

ROBERT H. SCOTT

Herbert Spencer and *à priori* Truths

ABSENCE from town has delayed what further remarks I have to make respecting the disputed origin of physical axioms.

The particular physical axiom in connection with which the general question was raised, was the Second Law of Motion. It stands in the *Principia* as follows:—

"*The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.*

"If any force generates a motion, a double force will generate double the motion, a triple force triple the motion, whether that force be impressed altogether and at once, or gradually and successively. And this motion (being always directed the same way with the generating force), if the body moved before, is added to or subducted from the former motion, according as they directly conspire with or are directly contrary to each other; or obliquely joined, when they are oblique, so as to produce a new motion compounded from the determination of both."

As this, like each of the other laws of motion, is called an axiom;[*] as the paragraph appended to it is simply an amplification, or re-statement in a more concrete form; as there are no facts named as bases of induction, nor any justifying experiment; and as Newton proceeds forthwith to draw deductions, it was a legitimate inference that he regarded this truth as *à priori*. My statement to this effect was based on the contents of the *Principia* itself; and I think I was warranted in assuming that the nature of the laws of motion, as conceived by Newton, was to be thence inferred.

The passages quoted by the *British Quarterly Reviewer* from Newton's correspondence, which were unknown to me, show that this was not Newton's conception of them. Thus far, then, my opponent has the best of the argument. Several qualifying considerations have to be set down, however.

(1) Clearly, the statements contained in the *Principia* do not convey Newton's conception; otherwise there would have been no need for his explanations. The passages quoted prove that he wished to exclude these cardinal truths from the class of hypotheses, which he said he did not make; and to do this he had to define them.

(2) By calling them axioms, and by yet describing them as principles "*deduced* from phenomena," he makes it manifest that he gives the word axiom a sense widely unlike the sense in which it is usually accepted.

(3) Further, the quotations fail to warrant the statement that the laws of motion are proved true by the truth of the *Principia*. For if the fulfilment of astronomical predictions made in pursuance of the *Principia* is held to be the evidence "on which they chiefly rest to this day," then, until thus justified, they are unquestionably hypotheses. Yet Newton says they are not hypotheses.

Newton's view may be found without seeking for it in his letters: it is contained in the *Principia* itself. The scholium to Corollary VI. begins thus:—

"Hitherto I have laid down such principles as have been received by mathematicians, and are *confirmed* by abundance of experiments. By the two first Laws and the two first Corollaries, Galileo discovered that the descent of bodies observed the duplicate ratio of the time, and that the motion of projectiles was in the curve of a parabola; experience *agreeing* with both," &c.

Now as this passage precedes the deductions constituting the *Principia*, it shows conclusively, in the first place, that Newton did not think "the whole of the *Principia* was the proof" of the laws of motion, though the Reviewer asserts that it is. Further, by the words I have italicised, Newton implicitly describes Galileo as having asserted these laws of motion, if not as gratuitous hypotheses (which he says they are not), then as *à priori* intuitions. For a proposition which is *confirmed* by experiment, and which is said to *agree* with experience, must have been entertained before the alleged verifications could be reached. And as before he made his experiments on falling bodies and projectiles, Galileo had no facts serving as an inductive basis for the Second Law of Motion, the law could not have been arrived at by induction.

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[*] See Strasburger's memoir in "Yenaische Zeitschrift," vi. band, 2 heft, pp. 249-261.

[*] It is true that in Newton's time, "axiom" had not the same rigorously defined meaning as now; but it suffices for my argument that, standing unproved as a basis for physical deductions, it bears just the same relation to them that a mathematical axiom does to mathematical deductions.

Let me end what I have to say on this vexed question by adding a further reason to those I have already given, for saying that physical axioms cannot be established experimentally. The belief in their experimental establishment rests on the tacit assumption that experiments can be made, and conclusions drawn from them, without any truths being postulated. It is forgotten that there is a foundation of preconceptions without which the perceptions and inferences of the physicist cannot stand—*preconceptions which are the products of simpler experiences than those yielded by consciously-made experiments.* Passing over the many which do not immediately concern us, I will name only that which does,—the exact quantitative relation between cause and effect. It is taken by the chemist as a truth needing no proof, that if two volumes of hydrogen unite with one volume of oxygen to form a certain quantity of water, four volumes of hydrogen uniting with two volumes of oxygen will form double the quantity of water. If a cubic foot of ice at 32° is liquefied by a specified quantity of heat, it is taken to be unquestionable that three times the quantity of heat will liquefy three cubic feet. And similarly with mechanical forces, the unhesitating assumption is that if one unit of force acting in a given direction produces a certain result, two units will produce twice the result. Every process of measurement in a physical experiment takes this for granted; as we see in one of the simplest of them—the process of weighing. If a measured quantity of metal, gravitating towards the earth, counterbalances a quantity of some other substance, the truth postulated in every act of weighing is, that any multiple of such weight will counterbalance an equi-multiple of such substance. That is to say, each unit of force is assumed to work its equivalent of effect in the direction in which it acts. Now this is nothing else than the assumption which the Second Law of Motion expresses in respect to effects of another kind. "If any force generates a motion, a double force will generate a double motion," &c., &c.; and when carried on to the composition of motions, the law is, similarly, the assertion that any other force, acting in any other direction, will similarly produce in that direction a proportionate motion. So that the law simply asserts the exact equivalence of causes and effects of this particular class, while all physical experiments *assume* this exact equivalence among causes and effects of all classes. Hence, the proposal to prove the laws of motion experimentally, is the proposal to make a wider assumption for the purpose of justifying one of the narrower assumptions included in it.

Reduced to its briefest form the argument is this:—If definite quantitative relations between causes and effects be assumed *à priori*, then, the Second Law of Motion is an immediate corollary. If there are not definite quantitative relations between causes and effects, all the conclusions drawn from physical experiments are invalid. And further, in the absence of this *à priori* assumption of equivalence, the quantified conclusion from any experiment may be denied, and any other quantification of the conclusion asserted. HERBERT SPENCER

MR. SPENCER's letter in NATURE, vol. ix. p. 420, is likely to give to such of your readers as have not followed the controversy in which he is engaged a false notion of the issues therein. Mr. Spencer writes as though the views of the nature of physical truth that were objected to by Prof. Tait and myself amounted to the ascription of our knowledge of sundry physical laws to organised ancestral instead of individual experiences. In one portion of his reply to me he intimates the same, as, for instance, where he says of me:—

"His argument proceeds throughout on the assumption that I understand *à priori* truths after the ancient manner as truths independent of experience; and he shows this more than tacitly where he 'trusts' that he is attacking one of the last attempts to deduce the laws of nature from our inner consciousness. Manifestly a leading thesis of one of the works he professes to review is entirely unknown to him—the thesis that forms of thought, and consequently those intuitions which those forms of thought involve, result entirely from the effects of experiences organised and inherited" ("Replies to Criticisms," p. 332).

But, in his "First Principles," Mr. Spencer expresses himself far too clearly for him to be able to assign the above as his views at that time on these so-called *à priori* truths. Speaking of the indestructibility of matter, one of the three truths in question, he says:—

"The annihilation of matter is unthinkable for the same reason that the creation of matter is unthinkable—and its indestructibility thus becomes an *à priori* cognition of the highest order—*not one that results from a long continued registry of experiences gradually organised into an irreversible mode of thought;* but one that is given in the form of all experiences whatever."

For the second of the truths he claims a similar authority; while for the third—the Persistence of Force—he claims a yet higher warrant:—

"Deeper than demonstration—deeper even than definite cognition—deep as the very nature of mind is the postulate at which we have arrived (*i.e.* the Persistence of Force). *Its authority transcends all other whatever; for not only is it given in the constitution of our own consciousness*, but it is impossible to imagine a consciousness so constituted as not to give it." ("First Principles," p. 192).

Had Mr. Spencer confined himself to defending such an *à priori* origin of physical truths as he now seems inclined to put forward, I should never have compared his theories to those of the Ptolemaists. But I can leave it with confidence to the readers of NATURE to decide between us as to whether the above passages do not show that at the time when they were written Mr. Spencer understood *à priori*, as there applied, in a manner very like the "ancient manner," and whether he did not maintain that these *à priori* truths were indeed "truths independent of experience."

THE AUTHOR OF THE ARTICLE ON HERBERT SPENCER
IN THE BRITISH QUARTERLY REVIEW

[The Editor, very properly wishing, I doubt not, to end the controversy, has sent to me the foregoing letter in proof. My comment on it is very brief.

Had the reviewer read the "Principles of Psychology," placed at the head of his article apparently for form's sake only, he would, not, I think, have made the above rejoinder.

That view of the *à priori* origin of physical truths which the Reviewer now seems to think defensible is the view implied in "First Principles" and the view set forth in the "Principles of Psychology," published years before. Tacitly throughout that work, and explicitly near the end, in a chapter on "Reason," the doctrine is that the "forms of thought" themselves are the products of experience. If the nervous system as a whole and in all its structures has been evolved by converse between the organism and the environment, the fundamental principles of its action, the very "forms of all experiences" have been evolved. Experience itself grew into definiteness gradually. And if the very form of our thought, the very frame-work of our consciousness, has been thus moulded, the inability to conceive a mode of thinking fundamentally different, is simply the result of inability to invert the fundamental action of the structures by which we think.—H. S.]

On the Word "Axiom"

IN reference to the controversy between Mr. Spencer and his reviewer about Sir I. Newton's calling his laws of motion "axioms," it is to be observed that there is a certain ambiguity in the word. "Axiom" is from ἀξιόω (I demand), and would thus signify a first principle to be taken for granted. It does not, of course, carry with it the meaning of a necessary judgment which cannot be contradicted. Whatever may be considered the ground of Euclid's "axioms" so called, Euclid himself did not apply that name to them; but the first nine he called "common notions," and the last three (which are peculiar to geometry) he placed among the postulates (ὁμολογήματα), and heads them with "let it be granted." Now it is clear, from Newton's own words, that in calling his Leges motûs "axioms," he does not imply that they are *necessary* judgments, but that he requires them first of all to be granted (however established) in order to the following reasoning. In other words, they are postulates, like Euclid's last three "axioms." In our modern use of the words "axiom," "axiomatic," there is always implied the *ground* why a proposition is demanded as granted, viz., because its necessity is self evident; but this wider use is not required by etymology, or (I think) in interpreting all ancient writings. F.M.S.

A Beech pierced by a Thorn Plant

THE word *pierced* makes the difference between an impossibility and a fact which is not uncommon in nature. The thorn mentioned in your last impression by Mr. Murphy has grown between two beech stems, which were so close that from their annual increase they grew together, and in so doing they enclosed the thorn, which could no more have *pierced* the beech than it could have pierced a block of marble. If young trees are twisted together they will grow together. Years ago I placed a bar of iron in an interstice between two stems so twisted, in another interstice below it I placed a part of the drag-chain of a waggon. According to Mr. Murphy the two iron appendages "have grown right through the middle of the trunks of the two beeches." They are at least as firmly fixed as if they had done so.

The tree with the iron branches is close to the lodge on Brookwood Hill. Should any of your readers consider it to be worth inspection, the lodge-keeper will show it to them.

April 11 GEORGE GREENWOOD

Mars

I BEG to offer my thanks to Mr. Knobel for his obliging correction (vol. ix. p. 396) with regard to the contrasted tint of the snow-poles of Mars. His observations had quite escaped my recollection.

I have also to mention a correction with which I have been favoured by the Earl of Rosse It appears that an erroneous hour had been affixed to the drawing of Mars made at Parsonstown on September 14, 1862, and engraved in Mem. R.A.S., vol. xxxii., pl. v., and that an explanation is thus offered of one of the discrepancies commented on by Prof. Kaiser.

Cheltenham, April 9 T. W. WEBB

Bright Shooting-star

A SHOOTING-STAR, equal in apparent brightness to the planet Jupiter, was seen here by me this evening at $9^h 18^m$. It traversed a path of 24° in two seconds, beginning at R.A. 242°, $D + 47^\circ$, and ending at R.A. 278°, $D + 50^\circ$. No perceptible train remained after the disappearance of the nucleus, which, however, emitted numerous sparks when in motion. The radiant point of this meteor was probably near β Bootis, and identical with No. 36 in Mr. R. P. Greg's table of radiant positions in the "Monthly Notices R.A.S.," vol. xxxii. p. 350. This is given at R.A. 223°, $D + 40^\circ$ by Greg and Herschel, and at R.A. 224°, $D + 38^\circ$ by Schiaparelli and Zezioli. The meteor described above was not therefore a member of the well-marked meteoric streams of April 18–20. At stations eastward it was probably a much brighter object than observed here, and these brief details may be useful, taken in conjunction with others, in determining its height and velocity.

Cotham Park, Bristol, April 11 WILLIAM F. DENNING

THE LATE DR. LIVINGSTONE

OUR readers are no doubt familiar through the daily press with all that has transpired during the past week in reference to the all-absorbing topic of the late Dr. Livingstone and the home-bringing of his remains. The coffin containing these arrived at Southampton yesterday morning, and was received by the Corporation, Livingstone's family and friends, the President and fellows of the Royal Geographical Society, and many others, with all solemnity and with every mark of genuine respect. The body of the great explorer was accompanied to the station by a long and distinguished procession, and was conveyed in a special train to London, to be buried in Westminster Abbey on Saturday at 1 P.M.

The proposed position of the grave in the Abbey is near that of Major Rennel, the father of English geography, and the friend and adviser of Mungo Park. There was some hesitation between this position and the one near the grave of Sir John Chardin, the Persian traveller.

The President of the French Geographical Society, Vice-Admiral Baron de la Ronciere le Noury, is coming over from Paris, for the express purpose of being present at the funeral.

The Government grants a sum which Sir Bartle Frere "trusts will be sufficient for all purposes." Still we are glad to have Sir Bartle Frere's assurance that in the end there will be "no shortcoming on the part of the Government."

Dr. Livingstone's vocation was not a money-making one; he did not even live to hear that the world ranked him among its greatest men; the end of all his labours was a sad one. This country, all civilised countries we may say, will attend to the appeal which has been made on behalf of his family.

As was to be expected, Scotsmen have taken the initiative in raising a monument to one of the greatest of their fellow-countrymen; at a meeting held at Edinburgh, on Tuesday, it was resolved, in recognition of the "heroic services rendered to science and civilisation by the late Dr. Livingstone," that a national statue be erected to his memory in the capital of his native country. This is right and it is honourable to his fellow-countrymen, though the memory of Livingstone will need no "labour of an age in piled stones" to render it immortal. Indeed a true idea of the full height of his greatness is only as yet beginning to dawn gradually upon us, and it will be some time ere we are able adequately to estimate it. No doubt, therefore, the thought contained in Tennyson's sad strain must have occurred to many a one during the last few weeks—

> "I would that my tongue could utter
> The thoughts that arise in me;"

and perhaps with still greater force those others—

> "Oh for the touch of a vanished hand
> And the sound of a voice that is still."

What honours would we have heaped upon his head had he only lived to reach his native shore!

NATIONAL MUSEUMS IN BRAZIL

THE working of the National Museums in Brazil seems to be conducted on similar principles to those recently advocated for the management of the Government Museums in this country. From a thick volume of 388 pp. explanatory of the topography, constitution, and resources of Brazil, issued in connection with the Brazilian Department of the late Vienna Exhibition, we gather that the most important Natural History Museum in South America, is that at Rio de Janeiro, which was founded in 1817. It is divided into four sections:—the first includes Comparative Anatomy, Physiology, and Zoology; the second Botany, Agriculture, and the Mechanical Arts; the third Mineralogy, Geology, and the Physical Sciences; and the fourth Numismatics, Archæology, &c. Each section has its separate director, who has assistants, and the whole Museum is presided over by a Director-in-Chief. "The Museum has, besides, several corresponding members in the National and Foreign Scientific Societies, and there are two naturalists travelling through the Empire, for the purpose of making collections.

"The principal object of the National Museum is, to collect and study all the natural products of the country, and to deliver public lectures on the science of its province, spreading among the people theoretical and practical knowledge, in a simple style, adapted to their comprehension.

"The Museum," it is stated, "now keeps up a correspondence with European establishments of the same description, and willingly exchanges duplicates of its collections for those of foreign museums.

"The Government intends to create in the provinces several museums independent of that in the capital of the empire, that they may exchange among one another the respective products of each one, receiving at the same time from the central one, not only the necessary instructions for the classification and study of the collections, but its superabundant duplicates."

POLARISATION OF LIGHT*

VII.

AMONG the phenomena of polarised light which may be observed either with a Nicol's prism or even with the naked eye, one of the most curious, and perhaps not yet fully explained, is that of Haidinger's brushes. If the eye receives a beam of polarised light a pale yellow patch in the form of an hour-glass, the axis of which is perpendicular to the plane of vibration, is perceived. On either side of the neck of the figure two protuberances of a violet tint are also seen to extend. After a little practice these figures or "brushes" may readily be observed. If the day be cloudy a Nicol must be used and directed to a tolerably bright cloud. The brushes are better defined in one position than in others; but if the Nicol be turned round, the brushes will be seen to revolve with it. If on a clear day we look in a direction at 90° from that of the sun, where the skylight is most completely polarised, the brushes may be seen with a naked eye. Jamin has suggested in explanation of this phenomena that the substances of the eye act like a pile of glass plates, or rather spheres, which affect in different degrees (1) the rays of the same colour whose vibrations are differently inclined to the plane of incidence, and (2) the rays of different colours whose vibrations are similarly inclined. This will cause one colour to predominate in a general direction parallel, and its complementary to predominate in a plane perpendicular, to that of vibration. Helmholtz, however, connects the phenomenon with some double refraction due to the yellow spot in the eye, with the area of which that of the brushes is coincident.

It was explained above that in Iceland spar there is a particular direction, viz. that of the line joining the two opposite obtuse angles of the natural crystal, in which there is no double refraction, and in which all rays travel with the same velocity. This direction (that is to say, this line and all lines parallel to it) bears the name of the optic axis. There are many other crystals having the same property in one and only one direction, in other words having a single optic axis. There is, moreover, another class of crystals having two such axes. Crystals of the first class or uni-axal crystals are again divided into two groups, viz. positive, in which the extraordinary ray is more refracted than the ordinary, and negative, in which the ordinary ray is the more refracted. It will be remembered that the ray which travels slowest is the most refracted. Among the former may be mentioned

UNI-AXAL CRYSTALS.

Positive.

Apophyllite.
Boracite.
Ditopaz.
Hydrate of magnesia.
Hyposulphate of lead.
Ice.
Quartz.
Red Silver,
Stannite.
Superacetate of copper and lime.
Sulphate of potash and iron.
Tungstate of zinc.
Zircon.

Negative.

Apatite.
Arseniate of copper.
Arseniate of lead.
Arseniate of potash.
Beryl.
Carbonate of lime and magnesia.
Carbonate of lime and iron.
Chloride of calcium.
Chloride of strontium.
Cinnabar.
Corundum.
Emerald.
Honey stone.
Idocrase.
Mellite.
Mica.
Molybdate of lead.
Nepheline.
Octaedrite.
Phosphate of lime.
Phosphate of lead.
Rubellite.
Ruby.
Sapphire.

Crystals are usually divided into six systems, in each of which there is a fundamental and a variety of derived forms. The fundamental form of each system is based upon the number, magnitude, and inclination of the crystallographic axes or lines drawn through a point in the interior of the crystal, and terminating in its angles. The optic axes do not of necessity coincide with any of these.

(1.) The regular system, which is based upon a system of three equal rectangular axes. Any form derived from this will be perfectly symmetrical with reference to the three axes, and will present no distinguishing feature in relation to any of them. Crystals belonging to this system have no optic axis, nor any doubly refracting property.

(2.) The quadratic system, based upon a system of three rectangular axes, whereof two are equal, but the third greater or less than the other two. Crystals belonging to this system have one optic axis coinciding with the last-mentioned crystallographic axis.

(3.) The hexagonal system, having three equal axes lying in one plane inclined at 60° to one another, and a fourth axis at right angles to the other three. Crystals of this system have one optic axis coinciding with the fourth crystallographic axis. Iceland spar belongs to one of the derived forms of this system.

(4.) The rhombic system, having three rectangular but unequal axes.

(5.) The monoclinic system, which differs from the rhombic in this, that one of the three axes is oblique to the other two, which are still rectangular to one another.

(6.) The triclinic system in which all the axes are oblique.

All crystals belonging to the last three systems have two optic axes. In the rhombic system they lie in a plane containing two of the three crystallographic axes; in the monoclinic, they lie either in the plane containing the oblique axes, or in a plane at right angles thereto. In the triclinic no assignable relation between the optic and the crystallographic axes has been determined.

The phenomena of colours and their variations hitherto described have been produced by a beam of light, all the rays of which were parallel in their passage through the crystal or other substance under examination. There is, however, another class of phenomena due to the transmission of a convergent or divergent beam of polarised light, which we now proceed to consider.

It was shown above that the retardation due to any doubly refracting crystal, and consequently the colour produced by it is dependent on the thickness; and that with a crystal of constantly increasing thickness, the colours go through a complete cycle, and then begin again. Suppose then a divergent beam to fall perpendicularly upon a uni-axal crystal plate cut at right angles to the optic axis; the central rays will fall perpendicularly to the surface; but the rays which form conical shells about that central ray will fall obliquely. The rays forming each shell will fall with the same degree of obliquity on different sides of the central ray, those forming the outer shells having greater obliquity than the inner. Now the more obliquely any ray falls upon the surface the greater will be the thickness of the crystal which it traverses; and this will still be the case even though it suffers refraction, or bending towards the perpendicular on entering the crystal. Each incident cone of rays will consequently still form a cone when refracted within the crystal, although less divergent than at incidence, in its passage through the plate; and the successive refracted cones will be more and more oblique, as were the incident cones, but in a less degree, as we pass from the more central to the more external members of the assemblage forming the beam of light.

Let A B C D (Fig. 21) represent the crystal plate, O P the direction of the optic axis and of the central ray, O n, O n′ those of any two other rays. The ray O P will not be divided; but O n will be separated by the double refraction of the plate into two, n s, n r, the one ordinary, the other extraordinary; and these will emerge parallel to one another, and may be represented by the lines s t, r v. Similarly the effect of double refraction on O n′ may be represented by n′ s′, n′ r′, s′ t′, r′ v′. Suppose now that the process were reversed, and that two monochromatic rays, one ordinary, the other extraordinary, reach the plate at s and r in the directions t s, v r, respectively; these would meet at n and travel together to O. Suppose, further, that the difference in length of s n and r n is equal to one wave-length, then, since one of them is an ordinary and the other an extraordinary ray, their vibrations will be perpendicular to one another, and if the polariser and analyser be crossed, the point n viewed from O will appear dark. Similarly, if two rays arrive in the directions t′ s′, v′ r′, at the points s′ r′ respectively, they will meet at n′ and proceed together to O; and if the difference of the paths s′ n′, r′ n′ be two wave-lengths, the point n′ will also appear dark. A pair of rays reaching the crystal at points between the pairs before-mentioned, will emerge at a point n″ between n and n′, and will present a difference of phase equal to a half wave-length. On principles explained in an earlier part of these lectures, such a point n″ will appear bright. On either side of n″, that is towards n and n′, the light will gradually fade. The same alternations of light and darkness will recur at intervals as we proceed along any straight line drawn outwards from the central ray. And inasmuch as the obliquity of the ray is the same for every point equidistant from the centre O, it follows that the phenomena of light and darkness will be the same throughout each circle drawn about the centre O. In other words, the centre will be surrounded by rings alternately bright and dark. The diameters of the ring depend, as was seen above, on the wave-length of the particular light used, and will consequently be different for different coloured rays. If, therefore, white light be used, the different coloured rings would not coincide, but would be disposed in recurring series as we proceed outwards from the centre.

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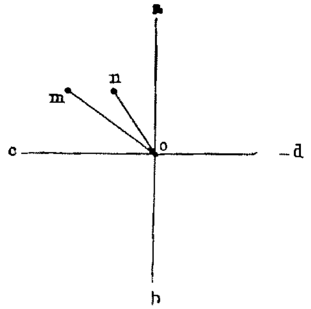
* Continued from p 386.

Another effect would, however, also be produced. Suppose the polariser and analyser to be so placed that, the field being regarded as a map, the vibrations in the one being E. and W., those in the other N. and S.; then of the two rays emerging at the most northern or the most southern point of any ring the vibrations of one would be towards the axis, or N. and S.; those of the other would be across it, or E. and W. And of these one would be extinguished by the polariser, the other by the analyser; and the same will be the case for every ring. Hence, throughout a N. and S. line crossing the entire field the light will be extinguished; and a similar effect will obviously occur along an E and a W. line. Hence, when the polariser and analyser are crossed, the entire system of rings will be intersected by a black cross, two of whose arms are parallel to the plane of vibration of the polariser and two to that of the analyser, and the rings in the quadrants on each side of an arm are of complementary tints. When the analyser is turned round through a right angle from its former position, only one set of vibrations (say those executed in a direction E. and W.) will be extinguished, and consequently along one pair of arms of the cross the ordinary rays will pass undisturbed, along the other the extraordinary; that is to say, the cross will be white. When the polariser and analyser occupy any other position than those noticed above, there are two crosses inclined at an angle equal to that between the planes of vibration, each arm of which separates complementary rings.

Various forms of polariscopes have been devised for showing the crystal rings. The simplest of these is the tourmalin forceps, which consists of two plates of tourmalin fixed in cork discs; the latter are encircled in wire in such a way that they may be turned round in their own planes. The wire after encircling one disc is bent round so as to form a handle; it then encircles the other; and the elasticity of the wire allows the pair of discs to be opened and shut like a pair of pincers. If a crystal plate be inserted between the two, and the whole held close to the eye, the rays from parts of the field at different distances from the centre will reach the eye, having traversed the crystal with different degrees of obliquity; and a system of rings and brushes will be formed.

Another method consists in applying to Norremberg's polariscope a pair of lenses, one below the crystal with the crystal in the focus, the other above it. The first ensures that the rays shall traverse the crystal with different degrees of obliquity; the second brings within the range of vision rays which would otherwise fail to reach the eye, and at the same time converging them into a cone with a smaller vertical range, renders the ring smaller than when seen with the simple tourmalins. An additional lens of greater focal length, *i.e.* of less power, is often added in order to adjust the whole to individual eyesight.



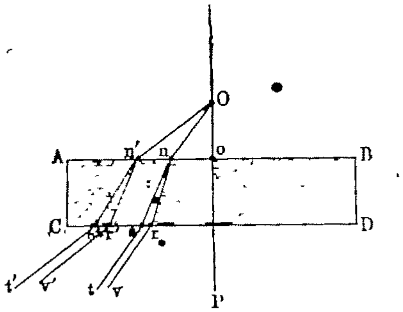


Fig. 21.

Fig. 22 gives the general appearance of the addition to the apparatus of Norremberg described above, and Fig. 23 the course of a system of rays brought to a focus on the lens a b, and again converged by a second lens c d.

But by far the most successful arrangement for enlarging the field of view so as to comprise the complete system of rings even with bi-axal crystals having widely inclined optic axes, is the system of lenses due in the first instance to Norremberg. The disposition of the parts is shown in Fig. 24; and the general appearance of the instrument as constructed by Hofmann of Paris, and called by him the "Polarimicroscope," is also given, Fig. 25. In this instrument the lenses which converge the rays upon the crystal plate can be taken out, and replaced by others giving parallel light; it can then be used as an ordinary polariscope.

Mention has been made above of the effect of the circular polarisation of quartz in the colours produced by a beam of parallel rays of polarised light. It remains for us to examine the modification which the rings and brushes undergo from the same cause. It has been explained that a ray of plane-polarised light in traversing a crystal of quartz in the direction of its axis is divided into two, the vibrations of which are circular, one right-handed, the other left. If the ray traverses the crystal in a direction perpendicular to the axis, and if the original vibrations are neither parallel nor perpendicular to the axis it is also divided into two, whereof the vibrations are not circular but rectilinear. It was suggested, first by Sir G. Airy, that these circular and rectilinear vibrations are limiting cases of elliptical; and both theory and experiment tend to confirm the suggestion, by showing that if the ray be incident on the crystal in any direction oblique to the axis, it is divided into two, the vibrations of which are similar ellipses having the longer diameter of the disc coincident with the shorter of the other, and the motion in the two oppositely directed. The longer diameters of the ellipses coincide with the directions of vibration of the ordinary and extraordinary rays in the case of an ordinary positives crystal; and are consequently directed, the one toward the centre of the figure, the other in a direction at right angles to the first.

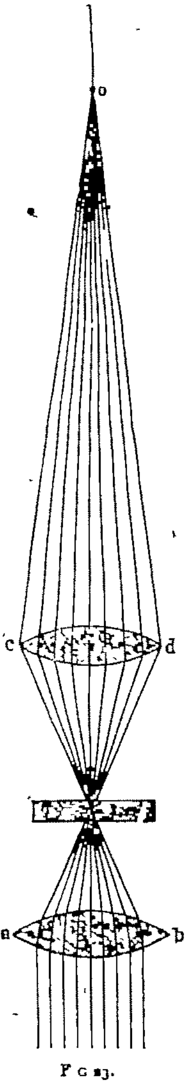


Fig. 23.

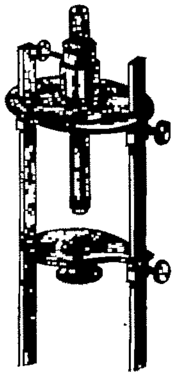


Fig. 22.

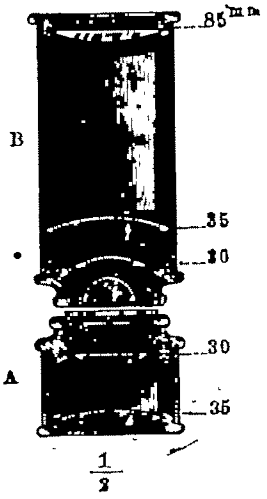


Fig. 24.

The exact, or even appproximate determination of the figures produced is a complicated question, and requires mathematical analysis for its solution, but a general idea of their nature may nevertheless be easily formed. When the polariser and analyser are either parallel or crossed, circular rings are formed, and towards the outer parts of the field traces of the black cross are seen, which grow stronger as we proceed outwards from the centre, that is, towards the parts where the rays are more oblique, and where the polarisation more nearly approaches to rectilinear. But in the centre, and near to it, where the polarisation is circular, or nearly so, the effects will resemble those produced by parallel rays, viz. the rays of different colours will emerge plane-polarised in different planes, and will be variously affected by the angle between polariser and analyser. In no position can they all be extinguished, and consequently in the centre all traces of the black cross will disappear.

When the planes of vibration of the polariser and the analyser are inclined at any other angle than 0° or 90°, the arms of the cross are less strongly marked, and the curvature of the rings becomes less uniform, increasing in the four points where they are crossed by the arms, and diminishing in the intermediate quadrants. When the angle between the planes of vibration is 45°, the rings assume a nearly square form, the corners of the square lying upon the lines which bisect internally and externally the angles between the planes. If the figures are produced with the analyser at 45° by two quartz plates of equal thicknesses, one right-handed, the other left, it will be found that the diagonals of the squares are at right angles to one another, the remains of the black cross occupying the same position in the field in both cases.

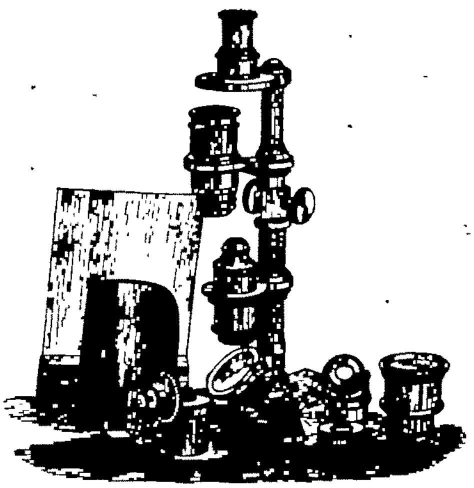


Fig. 25.

If two plates of the same thickness, the one right-handed and the other left, are placed one over the other, a beautiful effect, called from their discoverer Airy's spirals, is produced. In the centre of the field the rotatory powers of the plates neutralise one another, and a black cross commences. As we proceed outwards, the arms of the cross cease to be black, and become tinged with red on one side, and with blue on the other. At the same time they are bent round in a spiral form, in the direction of the hands of a watch if the first plate be right-handed, and in the opposite direction if the first plate be left-handed. These spirals intersect at intervals the circular rings; the points of intersection lie in four rectangular directions, which terminate towards the outer margin of the field in four arms of a shadowy cross. The colours of the rings and spirals are more brilliant and better defined than in most other phenomena of chromatic polarisation.

W. Spottiswoode

(To be continued.)

REPORT OF PROF. PARKER'S HUNTERIAN LECTURES "ON THE STRUCTURE AND DEVELOPMENT OF THE VERTEBRATE SKULL" *

II.

IT is well known that the eggs of sharks and rays, when deposited, are enclosed in a strong horny capsule or "purse" formed as a secretion from the oviduct. In both groups these curious appendages have the form of a pillow-case, the corners being pointed in the rays, and produced into long tendril-like processes in the shark and dog-fish. The embryo remains enclosed in the purse until about six months after oviposition, and it is during this period that all the most important metamorphoses are gone through.

The youngest embryo described was nearly an inch long, an extremely active little creature, attached to a yolk-sac about ¾ of an inch in diameter. In this stage the head and branchial region are large and conspicuous, the body slender, and tapering off to a long thread-like tail. The dorsal laminæ have completely united, leaving, however, a very thin covering to the hinder division of the brain, which consists of the three primary cerebral vesicles (Fig. 3, C 1—3), bent over the end of the notochord in such a way that the second, or middle division, forms the anterior termination of the head; the "cerebral flexure" is, therefore, complete. The future hemispheres (Hm) have already appeared as small buds from the fore-brain (C 1). The nasal, visual, and auditory organs are in an extremely rudimentary condition. On the under surface of the head is the large square mouth, bounded above by the fronto-nasal process (fnp), a shield-shaped elevation of the integument between the nasal sacs, found in the embryos of even the highest vertebrata, but persistent in the sharks and rays. Beneath the eye, and communicating by a slit running below the inferior boundary of the fronto-nasal process, is a cleft (Lch) answering to the lachrymal passage of the higher vertebrata, and formed by the shutting off of a portion of the original first visceral cleft by the growth of the pterygo-palatine arcade. This cleft, persistent in the higher animals, is a transient structure in the Elamobanchs. One of the most noticeable features in the embryo at this stage is the presence of a number of long filamentous external gills, each containing a single capillary loop; of these about ten spring from the hyoid and each of the branchial arches, while four much shorter ones project from the future spiracle, and are attached to the mandibular arch. The internal branchiæ are at present functionless, and form mere cog-like projections on the arches.

Fig 3

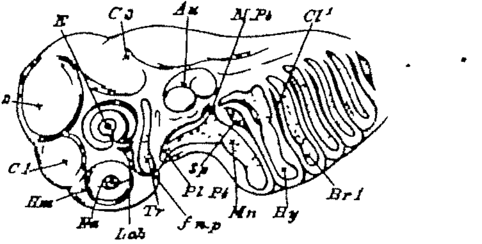


FIG. 3—Head of Embryo Dog-fish, 11 lines long. Lch, lachrymal cleft; C 1, 2, 3, Cerebral vesicles, Hm, Cerebral hemispheres, fnp, fronto-nasal process; Sp, Spiracle. Other references as before. The visceral arches are dotted for distinction's sake

Fig. 4

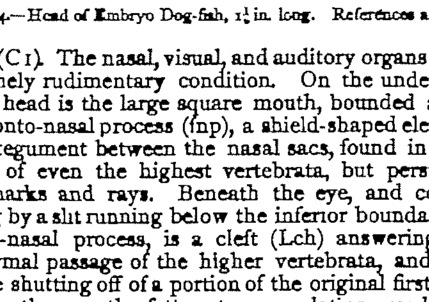


FIG. 4—Head of Embryo Dog-fish, 1¼ in. long. References as before.

The embryo at this age is so transparent, that the visceral arches can be seen with sufficient distinctness through the skin without any dissection whatever. Even at such an early period, the anterior face-bars already begin to show signs of segmentation, there being constrictions in the mandibular and hyoid arches, where division will afterwards take place. The upper part of these arches has assumed the pedate form which is taken on at a later period by the branchials. The pterygo-palatine arcade is already as large proportionally as in the adult, the true apex of the mandibular arch being reduced by its outgrowth to a mere tubercle (M Pt).

Fig. 5.

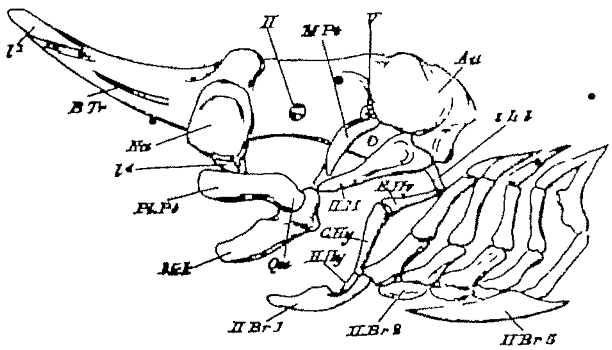


FIG. 5—Skull of Ray. M Pt, Spiracular cartilage; ihl, inter-hyal ligament

Granular subcutaneous thickenings have already appeared in relation with the face-bars; these are the extra-viscerals. In the same unchondrified condition are the parachordal and paraneural elements of the skull.

Embryos of 1⅛ and 1¼ in. in length possess external gills two or three times as long as in the preceding stage those on the mandibular arch having already shrunk and begun to form the pseudo-branchia; the internal gills are still functionless. The eye is completely formed. The investing mass and the nose and ear capsules are chrondified, but the two halves of the former are still separate, and the roof and walls of the cranium membranous. To make out clearly the relations of the facial arches, it is now necessary to dissect away the skin (see Fig. 4); it is then seen that the process of segmentation has advanced greatly, the arches behind the mouth being split up as in the adult, and differing from those of the full-grown shark only in form. The trabeculæ have become flat-

Fig. 6

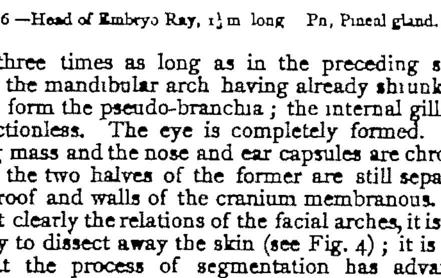


FIG. 6—Head of Embryo Ray, 1¼ in long. Pn, Pineal gland.

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* Continued from p. 446.

tened out from above downwards, but are still separate, and the lower-jaw arch, besides being divided into paleto-quadrate and Meckel's cartilage, has approached still more nearly to the adult character by the conversion into fibrous tissue of its apex (Figs. 2 and 3, M Pt).

The third stage described was a ripe embryo, about two inches long, having nearly the form of the adult. In this condition the external gills have entirely disappeared, the internal gills now performing the whole function of respiration. The skull has assumed all the characters of the adult, except as regards a few minor details, chondrification and segmentation being perfect. The investing mass has not only completely enclosed the notochord, but has formed an arch, like that of a vertebra, over the hinder part of the brain: in this way the occipital region of the skull is formed. The roof and walls of the brain-case, membranous in the last stage, have now become cartilaginous, and are fused with the nasal sacs in front, the trabeculæ below, and the auditory capsules and occipital region behind. The trabeculæ form the whole of the flat skull-floor in front of the ear capsules, where their original apices (pharyngo-trabeculars) are still to be seen as small rounded processes; they have completely coalesced behind, but are merely in apposition between the nasal sacs. The basi-trabecular (Fig. 2, B Tr) is small, and the first pair of labials (l^1), which together with it form the "cutwater," are flat, and widely separated; the snout is consequently much blunter than in the adult. The hypo-trabeculars (cornua trabeculæ of Rathke) occur as two inturned S-shaped filaments of cartilage on either side of the basi-trabecular: no trace of them is to be found in the adult.

III.—*Skull of the Ray or Thornback* (Raia clavata).—On the whole the skull of the ray resembles very closely that of the shark; in some respects, however, it approaches more nearly to the higher fishes, and in others, again, retains a lower or more embryonic character.

One of the chief points of difference between this type and the preceding is the much greater elongation of the skull, chiefly owing to the immense development of the basi-trabecular (Fig. 5, B Tr), which is produced to form the long, stout rostrum, the apex of which is strengthened by the first pair of labial cartilages (l^1). The region between the orbits is much pinched in, while the nasal and auditory regions are extremely broad, the nose capsules, especially, being as far apart as in the embryo shark. On the upper surface, the prominences (epiotic, pterotic, sphenotic) already described in the shark, are seen; but instead of one, there are two fontanelles, an oval one between the nasal sacs, and a second of an oblong shape, in the more normal position, between the orbits. On the under surface of the nasal capsules are seen the second, third, and fourth labials, forming a valvular apparatus for the nostrils; the fifth labial and the extra-branchials are absent.

The upper and lower jaws or dentigerous arches closely resemble those of the shark; the opening of the mouth is, however more completely on the under surface of the head, as in the embryo Squaloid. In the front wall of the spiracle a semi-lunar cartilage (M Pt) is found, connected by ligament with the auditory capsule above, and with the angle of the lower jaw below, and having the same relations to the fifth and seventh nerves as the metapterygoid ligament of the shark (Fig. 2, M Pt), or the bone of the same name in the osseous fish; this, therefore, is the true apex of the mandibular or first post-oral arch.

There is no mistaking the hyo-mandibular (H M) a cartilage having precisely the same connection and relation to the hinder division of the *portio dura* as the part similarly named in the shark, but much more slender, pointed below, and inclined forwards. The remainder of the hyoid arch, however, has taken on an entirely new character, and shows a marked advance towards the Teleostian type, being attached, not to the lower part of the hyo-mandibular, but to its upper posterior angle, by means of a band of ligamentous fibre, answering to the small styliform bone (stylo-hyal of Cuvier) which in the osseous fish connects the free portion of the hyoid with the suspensory apparatus. The gill-bearing part of the hyoid is slenderer than in the shark, and more highly segmented, being divisible into epi- cerato-, and hypo-hyals (E Hy, C Hy, H Hy); the basi-hyal or keystone-piece is absent.

The branchial arches differ from those of the shark chiefly in the great development of the inferior segment or hypo-branchial. The first of these (H Br. 1) is much extended, and, uniting with its fellow of the opposite side, forms a transverse bar behind Meckel's cartilage. The second, third, and fourth hypo-branchials are broad adze-shaped plates, while the fifth is coalesced with its fellow in its hinder half, and extended forwards, so as nearly to meet the corresponding piece of the first arch.

The youngest ray described was an embryo of *R. maculata*, 1½ inch long, taken from the purse at about the seventh week from oviposition. The body proper is not larger than that of the first stage of the dog-fish, the greater length being due to the immense development of the tail. The pectoral fins, which by their expansion and union in front with the head, give to the adult ray its peculiar depressed form, are at this age small semi-elliptical lobes, one on each side of the umbilicus or point of attachment of the large yolk-sac. Six or seven long branchial filaments, expanded or spatulate at the end, are attached to the hyoid and branchial arches, but none are apparent externally on the mandibular.

The facial arches are visible in a side view with perfect distinctness, and have already advanced considerably in segmentation, the apex of the mandibular being on the point of separation so as to form the spiracular cartilage, and the proximal end of the hyoid being cleft vertically, thus separating the hyo-mandibular from the epi-hyal.

Three months after oviposition, although the yolk-sac is still as large as a small walnut, the embryo has completely taken on the adult form, the pectoral fins having enlarged greatly, and brought the gill-slits to the ventral surface; from these the external branchiæ still project, being now in the form of long threads, almost like the hyphæ of a fungus; the first cleft behind the mouth (tympano-eustachian) remaining on the upper surface has taken on the form of the spiracular opening.

In the skull very few embryonic characteristics are left, the chief being that the brain-case is rounder, the rostrum shorter, and the two first labials separated from it by a slight interval, instead of being in perfect apposition.

METEOROLOGY OF THE WEST INDIES

THE hurricane season, here reckoned from July 25 to October 25, went by without damage so far as the Windward and Virgin Islands are concerned, though not without disastrous examples of the phenomenon from which it derives its name elsewhere. Two cyclones of the ordinary kind have in fact visited these seas during the above period; and although neither of them included the island of St. Thomas in its range, yet they passed sufficiently near to make us aware of their existence, and to create considerable alarm among the inhabitants.

The first of these appears to have originated about lat. 10° N. long. 55° W., on or near August 10. Taking a north-westerly direction it passed parallel with, but at a considerable distance from the Windward islands, where from August 11 to 13 the weather showed signs of great disturbance with violent squalls, that shifted to every part of the compass; while at Martinique in particular, where the most threatening signs appeared, the mercurial barometer sank to 29·60. On the 14th similar indications showed themselves at St. Thomas—rain, squalls, and thunder; while on the night of the 15th the barometer suddenly fell to 29·70, and a violent gust of wind from the north caused many of the natives to barricade their windows in anticipation of the worst. This state of things lasted till next day at noon, when the mercury rose and the sky cleared.

But by this time the cyclone, now only about a hundred miles to the east of the islands, had fully formed itself; and henceforth its course was only too clearly marked by the damage it caused among the shipping. From August 17 to 24 it passed north, with a westerly inclination, till it fell in with the course of the Gulf Stream, above Florida, and then followed that line, but gradually nearing the coast, up to Nova Scotia, where the ravage was tremendous, upwards of a hundred vessels having been either dismasted, or rendered total wrecks. Further north the cyclone seems to have expanded into an ordinary storm and disappeared.

These particulars I gathered from the captains of the injured vessels, some of which took refuge in this port. They are illiterate men, and not capable of furnishing exact details; but all agreed in describing the wind-current as having been from north to south by west, and so back by east to north; the lowest barometric indication I heard speak of was 29°.

The second cyclone originated in the Carribæan Sea itself, to the west of Barbadoes. Telegrams of threatening weather dated September 25 reached us at St. Thomas; and on the 26th the aspect of matters here was gloomy; the sky murky, especially to the south, with continued flashes of lightning, and a very heavy sea. But the hurricane did not touch Santa Cruz; its first long-shore visit having been made on the 28th at Haiti, where several small ships were lost, and much mischief done. Jamaica escaped; but on the 30th the whole southern coast of Cuba was ravaged from east to west, and many lives lost by sea and shore. From Cuba the cyclone continued to pass west till it reached the Mexican coast, which it skirted, then turned east, touching Havanna on October 4, Florida on the 7th, and then, following the Gulf Stream, was lost in mid-ocean. Its greatest fury was in the Gulf of Mexico itself, where the injury done to the shipping almost equalled that caused off Halifax by the gale of August 24.

Some disquietude has also been caused here at St. Thomas by the frequency of slight earthquake shocks, of which I counted five within a period of forty days. The two strongest occurred on July 22 and August 12; in both instances the movement appeared to pass from east to west; it was accompanied by a distinct rumbling sound. The shock of August 12 occurred at 8.15 P.M., and was within a few seconds followed by another, but slighter. In two instances, June 16 and August 27, a slight shock at this island had been preceded, about an hour and a half before, by a stronger one at Jamaica: so that the general direction of the movement must have been contrary to the apparent surface vibration from west to east.

I may add that the whole of this island is manifestly undergoing a gradual upheaval, as appears by the wave-worn rocks of recent date, but already two or three feet above water mark; sea-shells and corals left dry, and similar indications. Hence the artificial channel opened, for purposes of cleansing by means of the current thus established, between the south-western extremity of the harbour and the outer sea, and which is in itself a clean cut, twenty feet wide, through a narrow band of rock, has, since 1870, when it first was made, lost so much of its depth as scarcely to allow of boat-passage; and threatens at no distant period to become absolutely useless.

W. G. PALGRAVE

NOTES

THE well-known German serial, Poggendorff's *Annalen der Physik und Chemie* has now been in existence fifty years, and we are glad to see that practical recognition has just been taken of the striking fact that it has, during this long period, been under the sole editorial direction of Prof. Poggendorff, while printed and published by the same house in Leipzic. It was agreed, a short time since, by a number of friends of the learned professor, that they should relieve him of his editorial duties for one volume, and that this should be presented to him in honour of the occasion, as a "Jubelband," or Jubilee volume. The importance of the work done by M. Poggendorff and his *collaborateurs*, during half a century, through the *Annalen*, is sufficiently obvious to any who have taken an interest in the progress of physical science in recent years. The serial well reflects that enterprise, plodding industry, and philosophical insight, which mark original research in Germany; and the 156 volumes that have appeared (six of these supplementary) constitute an invaluable storehouse for any one desiring to prosecute new lines of investigation in the wide field of physics. We learn from the preface to the Jubilee volume (which we hope to notice at greater length) that the entire number of papers published in the *Annalen* hitherto is 8,850; and among the 2,167 authors who have contributed to its pages, we find the eminent names of Liebig, Magnus, Berzelius, Rammelsberg, Rose, Faraday, Brewster, Becquerel, Regnault, and many others. A work of this kind, as is truly remarked, unites those engaged in similar researches all over the world, into one large brotherhood of mutual assistance and regard. We congratulate the learned editor on the completion of such a long term of arduous and honourable service to Science, and heartily join in the wish that this Jubilee volume may be followed by many others edited by the "Jubilar" himself.

WE are given to understand that preliminary negotiations are on foot for the establishment of a central establishment for Ocean Meteorology in Germany. We may hope that when such a step is in contemplation, the work done by Herr von Freeden, who has for the last seven years conducted the Norddeutsche, lately called the Deutsche Seewarte, at Hamburg, will meet with its full measure of recognition. The establishment in question has been maintained at the sole expense of the town of Hamburg, and has risen steadily from a small beginning to its present state of thorough efficiency, thanks, in great measure, to the energy of its director.

IN the review of Belt's "Naturalist in Nicaragua" by Mr. Wallace, p. 221, reference is made to the theory of the origin of cyclones propounded by the author in terms which might lead our readers to think that his views have been entirely unnoticed. Such is however not the case, and our attention has been drawn to Prof. Reye's work "Die Wirbelsturme, &c., in der Erd-Atmosphare, mit Berucksichtigung der Sturme in der Sonnen-Atmosphare" (Hanover 1872), in which Mr. Belt's views are discussed at some length, with an expression of regret that so little attention has been attracted to them.

THE Turners' Company of the City of London have acknowledged the debt which their Art owes to pure scientific research, by presenting the freedom and livery of that Company to Dr. John Phillips, F.R.S., Professor of Geology in the University of Oxford, in recognition of his introduction into architecture of the various materials which constitute the rocks of England. Mr. John Jones, a member of the livery, said the City of London was always willing to bestow its honours on successful generals, conspicuous statesmen, and devoted patriots—and the tributes were honourable to both sides—but that was the very first occasion on which they had offered the like distinction to a patient, observant, and reflecting student of nature. In doing so they, at the same time, offered their thanks to the University of Oxford for having included physical science in its curriculum of education, and they celebrated, so to speak, the alliance in that city of art and science.

THE recent appearance of a French translation of the last edition of Prof. Haeckel's world-known "Natürliche Schopfungs-Geschichte;" or, "History of the Creation of Organised Beings," will give a larger number of students the opportunity of mastering its valuable contents. How is it that no translation has yet appeared in our own language?

MEN of Science will be glad to learn that the Premier has recommended to the Queen for a pension of 100*l.* on the Civil List Mrs. C. L. Basevi, the mother of Captain Basevi, who lost his life on the Thibet frontier of India, whilst engaged in exploring the mountain passes, and pursuing other scientific inquiries.

DR. JOHN ANDERSON, Director of the India Museum of Calcutta, is at present in this country on two years' leave of absence. He is, however, devoting his holiday to working out the results of the Yunan Expedition, to which he was attached as naturalist three years ago. The Linnean Society has undertaken their publication, which will embrace a full description of the anatomy of the fresh-water Dolphin of the Ganges (*Platanista gangetica*), as also of the still rarer fresh-water Cetacean of the Irawady *Orcella fluminalis*.

ON Friday, April 10, M. Leverrier gave a *soirée* at the National Observatory of Paris, in honour of the delegates of the French Scientific Societies. The weather was unfavourable, but the exhibition of scientific instruments was very successful indeed. Many new electrical apparatus were exhibited for the first time. No gaslight was used; an electrical lamp by Perrin being the only lighting medium. A lecture was delivered by M. Wolf, on phenomena of polarisation, illustrated by optical experiments by Duboscq.

THE Commissioners for the Construction of a Universal Metre are at present superintending the fabrication of the definitive standards, which will be distributed amongst the several Government delegates to the International Congress. The operations are executed at Paris, in the laboratory of the School of Mines.

M. ALLUARD, the originator and director of the Puy de Dome Observatory, has announced that its inauguration will take place next September. Invitations will be sent to England as well as throughout France. The cost of the building will be something more than 4,000*l.*, and the work is fast advancing. The Puy de Dome Observatory will be connected by an electric telegraph with another observatory built at Clermont, and which is already in operation; the difference of level between the two stations will be more than 4,000 ft. When excavating on the top of the mountain it was discovered that a large building about 80 yards in breadth had existed on the spot. It is supposed to have been a Roman fortress and temple; a number of Roman medals were discovered.

M. COLLADON, the Geneva physicist, has published an essay on the subject of turning poplars into lightning conductors. He proposes to insert in the lower part of the trunk a metallic rod, which he connects with the earth by a chain, so that the fluid cannot leave the tree to dart at any object placed within a short distance, which at present is very often the case.

M. SAINTE-CLAIRE DEVILLE, appears to have been again successful in his weather predictions. We shall give details in our next on the important question.

FRENCH officials are organising a fusion between the postal and telegraphic officials.

A REMARKABLE instance of the rapid spread of a new pest is furnished by the history of *Puccinia Malvacearum*, a fungus parasitic on various plants belonging to the natural order Malvaceæ. Its native country is probably Chili, where it was discovered by Bertero on *Althæa officinalis*. Its first appearance in Europe was in April 1873, on *Malva sylvestris*, in the neighbourhood of Bordeaux, and in August it had extended to several other plants of the same order in the botanic gardens of that town, but, singularly enough, was not found on *Althæa officinalis*, several other nearly allied genera being also exempt from its attacks. In Germany it was first discovered in October, while in this country it was detected in the summer of 1873, nearly simultaneously in many widely-dispersed localities, as Exeter, Salisbury, Chichester, Shere in Surrey, the neighbourhood of London, Eastbourne, Pevensey, Sandown in the Isle of Wight, and Lynn, and threatens to be exceedingly destructive to the hollyhocks.

TWO remarkable instances of protective mimicry have lately been described by Prof. Gerstaecker, both among parasitic Hymenoptera, and having apparently for their object to facilitate the access of the parasite to the nest of the host for the purpose of laying its eggs. *Crypturus argiolus* differs altogether in colour and marking from the allied species of Ichneumonidæ, and assumes even in the minutest details those of the wasp *Polistes gallica*, on which it is parasitic. To so great an extent is the mimicry carried out that even variations characteristic of particular districts are reproduced, the area of distribution of both insects being very wide. In the second case it is the colour and markings of a wasp, *Vespa germanica*, that are imitated by its parasite *Conops diadematus*.

AT the east end of the Palm House at Kew there is a fine specimen of the rare *Agave univittata* coming into flower. Near it stand the still stately flowering stems of two allied plants, *Agave jacquiniana* and *Fourcroya gigantea*.

THE French conscription enables us to study annually the composition of the growing generation. Last year from about 300,000 young men passing the examination for recruiting the army, 16,000 were discharged on account of having lost their brothers or having had them wounded in the war, 11,400 as being sons or grandsons of widows, or of people above seventy years of age, 1,600 elder brothers of orphans, 16,000 as not being strong enough, 10,000 for lameness, 8,000 not tall enough, 4,000 from having defects of the bowels, 3,000 of the eyes, 2,700 organs of generation, 2,000 bad teeth, 1,000 as being mute, &c., 1,200 epileptics, 600 deaf, 80 blind, 1,000 phthisic, &c.; in all, 89,000 were left out of the contingent.

IN the report furnished us by the Secretary of the Anthropological Institute (vol. ix. p. 345) of Mr. Distant's paper On the Mental Differences between the Sexes, the author is said to have referred to the "now moderately well established fact that in primitive races the *hair* of women approximates more closely to that of man than obtains in a higher state of civilisation." Mr. Distant in his paper referred to the *brain*, not to the *hair*.

A QUICKSILVER mine is said to have been discovered at Exeter.

M. GASTON TISSANDIER is publishing at Hachette's an excellent popular work on "Photography," with numerous illustrations. One of them shows Charles, the inventor of the gas balloon, photographing the silhouette of one of his pupils on a paper sensitised with one of the salts of silver, more than twenty years before the discovery of Daguerre.

IN his recent paper On the Placentation of the Sloths, published in the Transactions of the Royal Society of Edinburgh, Prof. Turner has done much to diminish the value of placental characters in the classification of the higher mammalia. In *Cholœpus hoffmanni* the placenta is dome-like, multilobate and genuinely deciduate, more like that found in the Primates and Man than in any other order, so much so that the author remarks "from the point of view of the descent hypothesis, it is possible that between the Sloths and the Lemurs genealogical relations may exist," and "now that I have called attention to the evidence of affinity with these higher mammals it is not improbable that other features of resemblance may in time be recognised."

BESIDES the ornithological papers which the late Mr. H. D. Graham contributed to the *Naturalist*, he left in the form of manuscript notes the larger and more interesting portion of the ornithological work which he had undertaken in the islands of Iona and Mull during the latter part of his life. These, together with the papers referred to, are being prepared for publication by Mr. R. Graham, to whom the whole of Mr. Graham's ornithological correspondence was originally addressed.

WE learn from the *Times* that M. Giard, Professor of Natural History at Lille, has been making an interesting inquiry into the zoology of the French shore of the Straits of Dover. Many uncommon species of crustaceans, ascidians, and mollusca have been obtained, which will be fully described before the Scientific Congress which is to be held at Lille during the ensuing summer.

WE are glad to see that Government have at last begun to carry out their agreement with the Trustees of the Bethnal Green Museum, by laying out the vacant space around the Museum in gardens for the recreation of the people.

IN the House of Commons, on Tuesday, Mr. Cowper-Temple obtained leave to bring in a Bill to remove doubts as to the powers of the University of Scotland to admit women as students, and to grant degrees to women.

THE month of April is a famous one in the annals of the French Academy for centenary anniversaries. M. Biot was born in April 1774, almost the same day when Louis XIV. died. M. Biot was a member of the Academy of Sciences and Academy of Inscriptions. It was also in this month that Maupertuis published the first French mathematical essay in which the Newtonian theory of attraction was accepted. Lavoisier was engaged in making observations on solar heat with an immense lens at the cost of one of the richest financiers of the time.

THE last (received) number of *Annales Hydrographiques*, contains details of the navigation of the Magellan Straits by the corvette *L'Atalante*, with tabulated meteorological observations made during the 13 days of the passage.

THE continuation of Adolph Schaubach's "Deutschen Alpen" (pp. 641 to 850) is brought up to the end of the Trias. The writer is Dr. H. Emmrich.

THE last number of the *Annales des Sciences Géologiques* contains a continuation of M. Oustalet's researches on the fossil insects of the Tertiaries of France. This second instalment of 112 pages is devoted to Aix-en-Provence.

THE recently published Report of the Department of Mines of Nova Scotia shows that the total produce during the year from collieries was 1,051,467 tons. Of these 264,000 tons were sold to the United States, 6,000 to Great Britain, 214,000 were used in Nova Scotia, and the rest were sold to Quebec, New Brunswick, Newfoundland, Prince Edward Island, West Indies, and South America.

A STAINED glass window has just been placed in the parish church of Folkestone to the memory of Dr. William Harvey, the discoverer of the circulation of the blood, who was born in the town in 1578. It is the gift of the medical profession, more than 3,000 of whom have contributed towards the cost.

IN view of the great economical value of the fur seals of Alaska, and of the importance of a thorough knowledge of their habits and movements, with reference to the command of the market of the world, it is proposed by the United States Treasury Department to send some one to the North Pacific Ocean for the purpose of obtaining materials for an exhaustive report on the subject. It will be remembered that these seals, almost to the number of millions, visit the St. George and St. Paul islands of the Pribylov group every summer season for the purpose of bringing forth their young, and that on this occasion a company chartered by the United States is allowed to capture 100,000 annually. What becomes of these seals after they leave the islands is entirely unknown, although congregated there in such numbers for several months. A few are taken in the spring and fall as they pass along the coast of British Columbia and Washington Territory, but whether these are related to the Pribylov army or not is uncertain. The same species is found to a limited extent on the Asiatic side of the ocean, but no very extensive captures are made. Should this commission be appointed, it is to be hoped that some of these problems may be solved, and that we may not remain longer in ignorance of the general natural history of so important an animal, which furnishes a revenue to the United States of about 300,000 dols. a year, while a profit of almost millions is made by the company which has charge of the interest.

THE additions to the Zoological Society's Gardens during the last week include a Vigne's Sheep (*Ovis vignei*), from Asia, presented by Capt. Archibald; a Sambur Deer (*Cervus aristotelis*) and an Axis Deer (*Cervus axis*), from India, presented by the Hon. Justice Jackson; two Cut-throat Finches (*Amadina fasciatus*) and two Paradise Whydah Birds (*Vidua paradisea*) from West Africa, presented by Lieut. J. H. Hearne, R.N.; a Rufous-necked Weaver Bird (*Hyphantornis textor*) from West Africa, presented by Mr. Hincks; two Negro Tamarins (*Midas ursulus*) from Rio de Janeiro; a Common Rhea (*Rhea americana*) from Buenos Ayres; a Brazilian Teal (*Querquedula brasiliensis*), and a Bahama Duck (*Pœcilonetta bahamensis*) from South America, purchased.

THE PHYSICAL HISTORY OF THE RHINE*

THE attempt to unravel geological history, as far as the stratified rocks are concerned, and all the igneous rocks connected with them, simply resolves itself into this:—an effort to realise the physical geography of different geological epochs, to make out the relations of the sea and of the land with its plains and mountains during these periods, the history of the rivers and lakes of the time, and to know as much as may be known of all the creatures and of all the vegetation which inhabited the water and the land.

I am now going to attempt to explain the history of that great historical river the Rhine. Every river has a definite history, if we could clearly make it out. Every river has had a beginning, and it is quite possible—if we have the skill—to find out when, by special changes in physical geography, such and such a river began to flow, and why it flows in such and such a direction.

In various publications I have attempted to show what is the history of some of the rivers of England; as, for example, the history of the Severn and of the Thames, and I think I have been able to prove that the Severn is a much older river than the Thames; and, on similar principles, I now propose to attempt to reveal to you the history of the Rhine and its valley from early times to the present day. For many years I had an ambition to work out the history of the Rhine. I have known it now for more than twenty years; going often up and down the river, and sometimes for weeks—once for months—living on its banks. For the last thirteen years, unfortunately, I never was able to return to it, but the problem I had marked out for myself remained in my mind, and last year I went, and worked it out, with the result which is now to be explained.

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* A Lecture delivered at the Royal Institution on Friday evening, March 27, by Prof. A. C. Ramsay, LL.D., V.P.R.S., Director-General of the Geological Survey of the United Kingdom, &c.

First, with regard to the great main features of the Rhine valley; it has its sources, as every one knows, in the mountain regions of Switzerland, one of which is in the valley of the Vorder Rhine, and the other in that of the Hinter Rhine, both glacier regions. The ground where it rises is from 7,000 to 8,000 feet above the level of the sea. From thence it passes to the Lake of Constance, 1,305 feet above the sea; and beyond, in a westerly direction, by Schaffhausen to Basel, where, at the bridge, the level of the water has an average height of about 803 feet above the sea. Thence it flows down the great upper plain of the Rhine northerly between the Schwartzwald and the Vosges to Mainz, where the height of the river is about 531 feet above the level of the sea, thus showing a fall between Basel and Mainz of about 272 feet, which gives an average slope for the running of the river of about 3 ft. 1¼ in. per mile. Beyond that, proceeding to the north, we come to the deep gorge of the Rhine, passing between high cliffy banks, which begin at Bingen and continue down to Rheineck in the neighbourhood of the Siebengebirge, for a distance of from 60 to 70 miles, according as you take into account all the windings or omit them. Beyond the Siebengebirge there is a plain, partly formed of the delta of the river, which gradually merges into the great flats that extend all the way from Calais to the Elbe.

Now the main question I have to bring before you is, first, what is the origin of the great upper plain that lies between Basel and Mainz? and, secondly, what is the origin of the gorge between Mainz and Rheineck? Why are they there, and by what means have this plain and this gorge assumed their present forms?

When you stand above Bingen, or, better still, if you ascend the Taunus and look southerly, and consider the narrowness of the gorge and the great hilly barrier of rock that must once have extended at Bingen across the lower end of the plain, the impression is irresistibly conveyed to the mind that before that gorge was opened a vast lake must have reached all the way from that barrier to where Basel now stands, covering the great plain that lies between the mountains of the Vosges and those of the Schwartzwald. And so thoroughly has this idea taken possession of the popular mind, at least of those who have at all considered the subject, that we find this statement made in some of the Guide Books of the time, and notably by Baedeker, where it is stated that a lake must have covered the whole of that vast plain, 170 miles in length, at a comparatively recent period. It is a very obvious theory and has much to recommend it, for it seems so clear that, before the gorge was opened, all that plain must have, been covered with a sheet of water, and it is hard to realise that such has not been the fact. When I first entered on the subject I was impressed with this idea, and I began to cast about and endeavour to find a reason for the scooping out of the gorge, and for the consequent drainage of the supposed lake.

Having years before written a paper on the origin of the lake basins of Switzerland, North America, and other parts of the world, and having attributed the formation of many of these, but by no means all of them, to the action of glacier-ice during the glacial period, my first impression was that ice might have had at least something to do with the scooping out of the great valley that lies between the north flanks of the Jura, the Schwartzwald, the Vosges, and the Taunus. But while slowly passing up the river, and searching for proofs which might either confirm or contradict this view, I was soon obliged to give up the idea that glacier-ice had anything to do with scooping out the great hollow. For on one side—that of the mountains of the Schwartzwald—I found that none of the glaciers of that region (and there are proofs that glaciers once existed there) even extended well down into the valley of the Rhine. And on the opposite side of the Rhine Valley, that of the old glacier region of the Vosges, I found no proof that they ever extended down so far as the plain. There is also no proof that the glaciers of the great glacial epoch of Switzerland ever extended as far north as Basel. Neither are there any signs of erratic blocks or other kinds of moraine matter on the plains or hill-slopes about Bingen, which one might expect to find there had the whole of the great plain of the Upper Rhine been once filled with glacier-ice. Therefore this theory, which I had not definitely formed, but which I surmised might possibly have had something to do with the subject, entirely melted away, and other hypothetical views along with them, and I was obliged to begin anew.

Accordingly I went to Switzerland, and with the help of friendly Swiss geologists, examined part of the Miocene or Middle Tertiary rocks between the Oberland and the Jura.

To make the rest of the subject clear, I must now say a few words about the origin of mountain chains. Most people are familiar with the outlines of the nebular hypothesis. The whole solar system was once in a nebulous state, and as this nebulous mass revolved in space, portions of it were thrown off, and one of these consisted of the matter which, by and by, resolved itself into the present earth. This nebulous fluid, in virtue of gravity, by degrees condensed more and more, and, passing through what we may call the molten state, in the course of time began to assume a solid form, and a hard outer crust was at length produced which enclosed a highly-heated fluid mass within. This crust, which continued to thicken in consequence of radiation of heat, because of the law of gravitation, was ever drawn towards the centre of the earth. By this process the circumference of the earth necessarily became less, and that consolidated rocky sphere which formed the outer shell of the earth was forced to readjust itself so as to occupy a diminishing area. Thus it happened that while some parts sank, other parts of the crust were crumpled, and relatively raised higher than other portions of the crust that still retained their original curves as part of a sphere.

This hypothesis, which, as far as I know, was first propounded by Elie de Beaumont, may be looked upon as the origin of mountain chains. What began in the earlier and prehistoric times of geological history, seems to have been going on steadily down to the present day, and thus it happens, that geologists can prove mountain chains to be of very different ages, and that, of whatever age they may chance to be, the strata that compose them are found to be bent and contorted. This contortion of strata took place simply from that shrinking of the earth's crust which was the natural result of radiation of heat into outer space. Portions of the crust more or less gave way to lateral pressure, while other parts of the great spheroidal curve more or less retained what we call horizontal or nearly horizontal position.

In this way it happened that at a certain period of geological history which preceded the formation of the Miocene rocks in the region now occupied by the Alps, a disturbance of the earth's crust took place, due to shrinkage of the general mass, of such a nature that the Alpine strata were thrown into highly-contorted forms, and a great mountain range of pre-Miocene age was the result. On the north of these mountains the Miocene strata began to accumulate in great lakes. But these lakes lay so near the level of the sea, that every now and then, by depression of the land, they sometimes sank a little below the sea, and the sea invaded the area formerly occupied by fresh water. The result was that in Switzerland, between the Oberland and the Jura, and much farther north, the Miocene strata which are hundreds and sometimes thousands of feet thick, are now found to consist of interstratifications of marine, brackish, and of fresh-water beds. At that time the Jura had no existence. It is the result of a later disturbance of the crust of the earth, and thus it happened that all the Miocene waters in which were deposited the strata that now lie between the Oberland and the Jura originally spread northwards far across the area now occupied by the Jura, and into the district of the present plain of the Rhine between Basle and Bingen.

It is hard to realise the scenery of that time; but partly by an effort of imagination, and partly by special knowledge of the fossils contained in the rocks, it is possible to form some conception of the appearance of the country.

On the east and west of the great valley were mountainous ranges now called the Schwartzwald and the Vosges, while far to the south rose the high mountains of the pre-Miocene Alps, more or less covered with a forest vegetation. On the banks of the lakes there grew in an early stage of the Miocene epoch vast numbers of forest-trees and evergreen shrubs, of genera such as are now characteristic of tropical and sub tropical countries; figs and vines, many species of Proteaceæ analogous to those that still grow in the Australian continent, together with cypress, sequoia, cinnamon, fan palms, and palmettoes, ferns, hornbeams, and buckthorns, all of genera still familiar, but mostly if not altogether of extinct species. At a later date this vegetation partly died out, and was replaced by plane-trees, poplars, elms, willows, and maples; while cinnamons, figs, vines, laurels, and Proteaceæ still continued to flourish. In the woods, on the meadows, and in the waters respectively, the *Mastodon angustidens*, the rhinoceros, Chæropotamus, Dichobune, deer, Dinotherium, hippopotamus, crocodiles, salamanders, fish, and numerous other creatures roamed at pleasure, while the air and the land were tenanted by dragon-flies, ants, beetles, and other insects, of which more than 800 species have been distinguished.

I now come to the chief part of this lecture, which is to account for the origin of the Rhine: for at that earlier time the Rhine had no existence in this valley, and indeed there is proof that instead of the main drainage of the area, flowing from south to north as it does now through this valley, the waters drained from north to south; and the pebbles of the Schwartzwald, instead of being carried north as they are now, were carried southward by minor rivers, and found their way into Switzerland, thus helping to form some of the conglomerate rocks of which the Miocene strata of Switzerland to a great extent consist.

Not only had the Rhine no existence then, but the romantic gorge of the river, with which so many are familiar, had no existence either. It has been customary sometimes to attribute the formation of that gorge to violent disturbance and fracture of the strata, by which the waters were allowed to escape from south to north. I have no belief in such violent disturbances having any place in the modern economy of the world, nor yet in such cataclysmal action having ever affected the ancient world, as far as it is in the power of geologists to trace back events from the present day to the oldest known geological periods.

After the Miocene epoch had lasted for a long period of time, there occurred another disturbance of the European region, and of much of the rest of the world besides, though it is only the Alpine region and the countries north of the Alps that we have now to deal with. This second disturbance of the Alps produced a great upheaval of the Miocene strata. All the Miocene lakes that occupied the old lowlands of Switzerland and extended far east into what is now the Austrian dominions and into Asia itself,—all that area, as far as the Alps are concerned, was gradually heaved up high above the level of the sea, and those beds of conglomerates, sandstones, and marls that form the lowlands of Switzerland, and all across what is now the Jura, were disturbed to such an extent that the strata now forming the Righi and Rosberg and other sub-Alpine hills were partly raised to a height of at least 5,800 ft. above the level of the sea, and probably much more. The lower parts of Central Switzerland, about the Lake of Geneva, the Lake of Constance, and Neufchatel, still stand at heights of from 1,200 ft. to 1,300 ft. above that level. Then the range of the Jura first rose up to form a mountain-chain, and this is the proof of these disturbances. First we know that the Miocene rocks originally lay all the way from the Alps to the Taunus in flat-lying strata. During that period a vast quantity of Miocene pebbles were carried into the lakes, which were by and by consolidated into an exceedingly coarse conglomerate. Anyone who has ascended the Righi will remember that nearly the whole of it is formed of this coarse conglomerate, proving the prodigious amount of waste that the Alps underwent during the Miocene period. When we consider the amount of this waste, even though the waters of the Miocene period lay but little above the level of the sea, still in my opinion it is probable that the Alps themselves were then quite as high, if not higher, than they are now. For the prodigious amount of waste proved by the conglomerate, indicates the removal of an enormous amount of material from the pre-Miocene Alps.

After the disturbance which raised the Jura and the Miocene strata of the lowlands of Switzerland, this is what took place. Along with the contorted secondary strata of the Jura, the Miocene beds that previously covered them were thrown into a number of anticlinal and synclinal curves, and the greater part of the Miocene material over that area having since been removed by denudation, only a few outlying fragments of these strata remain, left in those wonderful upland basin-shaped hollows of the Jura, which still attest the original continuity of the Middle Tertiary deposits all the way from the base of the pre-Miocene Alps to the northern base of the Taunus.

When the post-Miocene disturbance of the whole of this area took place the general effect was, that much of the Swiss Miocene area was contorted and raised high into the air, while between the Jura and the Taunus, the equivalent strata were simply heaved up and tilted so as to form a long inclined plain sloping northerly and lying between the Vosges and the Schwartzwald, and the surface of which may have been about 1,200 or 1,300 ft. above the present level of the sea where Basel now stands, and about 1,000 to 1,100 ft. high where the opening of the gorge now begins near Bingen.

Before this wide-spreading disturbance took place, the Rhine had no existence, for up to that time such small rivers as occasionally ran in the more ancient Miocene valley flowed partly south. But when the inclined plain was fairly completed, the result in the long run was that for the first time the great general drainage of the area began to run from south to north, and the Rhine was established flowing at a height which we may roughly speak of as having been 500 ft. higher than now, because at that time all the great valley between Basle and Bingen was filled to that height with Miocene strata. We know this to be a fact by an examination of the valley on the right hand and the left, from Bingen towards Basle, for every here and there, we find table-shaped hills formed of flat-lying Miocene strata, which border the present alluvial plain of the Rhine and abut upon the more ancient mountains on either side. The history revealed by this fact is plain to anyone accustomed to reason on geological phenomena. The strata forming scarped slopes on opposite sides of the valley were once united, and their early continuity has been destroyed, simply by long-continued watery waste and denudation. They are indeed only the relics of an older phase of the physical geography of the district, when the surface of the plain stood about 500 ft. higher than it does at present.

Now when the Rhine first began to flow, the river then passed through a high upland valley with gently sloping sides that lay between the Taunus and the Hundsruck, and which in no manner resembled the precipitous cliffs that now bound the Rhine in the gorge below Bingen. The bottom of part of this old upland valley still forms a narrow terraced plain, immediately above and beyond the edge of the cliffy gorge of the Rhine. It is not always continuous on both sides of the gorge, but enough of it remains to attest its original continuity at heights of from 400 to 500 ft. above the present level of the river. Now what I wish to persuade you of is this, that the Rhine flowing in this valley by degrees began to cut out its own gorge, and that it was not produced by fracture. Every running river is busy eroding its channel, especially where the ground is at all steep. That is one of the main functions of running waters. They are constantly deepening their channels and carrying the sediments so formed from higher to lower levels, till in the course of time they find their way into lakes or the sea.

When we first enter the gorge of the Rhine, going southward, one feature that strikes the geological observer is the constant recurrence of this old terrace backed by the hilly country beyond. On the left bank, overlooking Bingen, the flat-topped spur of the Rochus-berg, about the same height as the tops of the neighbouring Miocene tabular hills, first strikes the eye. When fairly within the gorge below Niederheimbach, beyond its upper edge the old river plain is seen gently sloping to the north, while the sides of the gorge itself is seamed by numerous gullies worn by occasional torrents since the great ravine—a kind of canon—has been cut down to its present level.

At Welmich, below Niederheimbach, looking down the river, the edge of the terraced plain is seen receding northward in long perspective, and at Salzig, still further down, the features so well shown near Niederheimbach are again reproduced. The same outline occurs again and again all down the river between Bingen and Coblenz, and equally below Andernach, as for instance at Rheineck. Finally, above the Siebengebirge, just about the mouth of the gorge, looking up the river, the long eastern hills sloping to the river end in a terrace corresponding in general height and outline to those already mentioned. The general conclusion to be drawn from these observations is that at heights of from 450 to 500 ft. above the present river this ancient river terrace has a persistent gentle slope from south to north which approximately corresponds to that of the existing river.

The inference is plain: that formerly throughout the length of what is now the gorge the river flowed at that high terraced level, at a time when the plain above the gorge was so deeply filled with Miocene strata that the level of the river, where Mainz and Bingen now stand, was as high as the upland terrace that crowns the gorge between Bingen and Rolandseck. By degrees the river began to excavate the gorge, and slowly cutting deeper and deeper, and at the same time winding and ever changing its channel through the great plain between the Jura and the Taunus, by slow gradation it wasted away the surface of that plain more and more, and the matter won from that surface was carried down through the gorge to be added to the old delta of the river. At last the major part of the Miocene rocks that partly occupied the plain were worn away and the plain has been reduced to its present temporary level; while the terraced hills on either bank still remain to attest the amount of watery degradation that the area has undergone.

So much for the scooping out of the valley. But there is another point which I would like to impress upon you. On each side of the Rhine there are important tributary rivers. Thus, for example, above the gorge we have the Maine, the Neckar, the Kinzig, the Eli, and other streams, flowing through deep steep-sided valleys; and these rivers have from a very early period been tributaries of the Rhine. It follows, then, that when the level of the Rhine was 400 or 500 ft. higher than at present the levels of the bottoms of these rivers must also have been 400 or 500 ft. higher than at present; and therefore, just in proportion as the great valley of the plain of the Rhine was being cut down and lowered, so in proportion must the valleys in which these rivers run have gradually been deepened. When we come to the gorge the same kind of argument applies to the Moselle and other tributaries of the Rhine.

I have elsewhere attempted to show that at one time the Moselle ran as high as the top of the table-land that now bounds it on each side. Everyone who knows that river is aware that, though it looks so hilly when we go up the stream in a steamboat, as soon as we reach the edges of the slopes on either side we are on the top of a great table-land intersected by numerous valleys, so that before the gorge of the Rhine was formed the Moselle ran at as high a level as the ancient Rhine; and just as the gorge of the Rhine was being deepened, so the Moselle was by degrees also enabled to deepen its channel. The same was the case with other rivers, right and left of the Rhine; and by applying this principle to the other great rivers of Europe we may hope in the long run to explain the physical history of all the systems of drainage of all parts of the continent.

One other point remains to be stated with regard to the physical history of the Rhine. Geologists well know that in older times the glaciers of Switzerland were on an immensely larger scale than at present. Large as they appear to us at the present day, they are of pigmy size when compared with their magnitude at a comparatively late period of the world's history. The Rhone glacier then spread across all the area now occupied by the Lake of Geneva, till it abutted on the Jura; and the old Rhine glacier extended all over the Lake of Constance, and reached at least half way from Schaffhaussen to Basle. The body of water which flowed directly from such glaciers must have been very great, and enormous must the moraines have been that were shed from the ice-sheets. From an examination of the pebbles that form the superficial gravel on the present plain of the Rhine below Basle, it is certain that a large portion of them have come from the Alpine regions. Such a great moraine as was shed from the western edge of the old glacier of the Rhine was constantly being attacked by the waters that flowed from its end, and thus by degrees pebbles were carried onward into the plain. The result is that a large part of the gravels of the Rhine is simply the waste of old moraines shed from the glaciers of Switzerland, added to by material carried down by the streams of the Vosges and the Schwartzwald, also partly derived from the moraines of ancient glaciers on a smaller scale.

Last year it was my lot to deliver here a lecture on the history of old continents, and I attempted to show that one old continent in particular retained its identity through a very long period of geological time; that from the close of the Upper Silurian period, all through the Old Red Sandstone and Carboniferous periods, through the Permian and New Red Sandstone epochs, over great part of what is now Europe, that continent, with many physical changes, still retained its identity. Such a vast continent remaining through all those geological periods implies a succession of epochs of time, which, as far as years and cycles of years are concerned, the mind has as yet only hints of data which some day may help us to grapple with such a problem, and not till astronomy comes more boldly to the help of geology, may we begin to hope for the solving of the problem of the actual value of geological time. However that may turn out, it is certain that during the long continental epoch alluded to there were, over and over again, many changes in physical geography far greater than that petty change which I have been endeavouring to sketch out to-night. The floras and faunas of the world in that old time changed, not in the minor degree I have been speaking of to-night, but were more completely remodelled again and again in great part, even generically. Mountain ranges rose, glacial periods intervened and passed away. Great lakes, sometimes fresh, sometimes salt, appeared and were obliterated by great terrestrial changes. At one time vast lakes, like those of the heart of Africa and North America, covered prodigious areas of land; at another, equal or larger areas were covered by salt lakes as large as the Caspian and the Sea of Aral. And when you think of the continental episode in the modern geological history of Europe to which I have drawn attention, you will see how small it really is, though it may look large to our minds, compared to the old continental epoch of which I spoke last year. This you may depend upon, that though to the superficial eye it may seem as if the world had always been going on just as it is doing now, and that through all time to come it will go on just the same, with its mountains, valleys, rivers, lakes, and seas, yet it is none the less certain that changes, such as I have described to-night, are but the forerunners of other mutations as great, aye, and far greater, that will take place in the future. Just as there is as yet no certainly measured limit to the geological time of the past, so also we know of no measurable limit to geological time to come. But why should I keep you with words such as these, when I may convey a whole chapter in physical geology, condensed into eight lines, by the greatest of our living poets:—

> There rolls the deep where grew the tree.
>   O Earth, what changes hast thou seen!
>   There, where the long street roars, hath been
> The stillness of the central sea.
>
> The hills are shadows, and they flow
>   From form to form, and nothing stands:
>   They melt like mist, the solid lands,
> Like clouds they shape themselves and go.

SCIENTIFIC SERIALS

IN the *Journal of Botany* for March, the editor, Dr. Trimen, commences a series of useful articles (which is continued in the April number) on the Botanical Bibliography of the British Counties, being a list of country and district floras arranged topographically. The other paper of greatest interest in this number is one of a kind of which this very useful journal has now published a considerable number, and which may ultimately throw considerable light on some of the problems connected with the distribution of plants, On the Flora of the Leeds and Bradford District, by J. A. Lees.—The number for April commences with an article of some importance in systematic botany, A Revision of the genera *Dryobalanops* and *Dipterocarpus*, by Prof. Thiselton-Dyer, in which a number of new species are described, including two belonging to the previously monotypic genus *Dryobalanops*, and illustrated by a plate (two more being promised in the next number). The editor also gives in this number one of the most valuable specialities of the journal, his list of New Species of Phanerogamous Plants in periodicals published in Great Britain during 1873.

THE *Scottish Naturalist* for April publishes a number of papers on almost every branch of Botany and Zoology, including one on Geology, of more or less interest to Scottish naturalists.—Mr. J. A. Harvie Brown proposes the establishment of a Natural History Publication Society, something on the model of the original plan of the Ray Society, for the purpose of publishing original papers on Natural History, principally on Mammalia and Aves, and for reprinting in fac-simile rare and useful tracts, pamphlets, &c., on the like subjects.—We have also further instalments of the lists of the Lepidoptera and Coleoptera of Scotland, by Dr. Buchanan White and Dr. D. Sharpe. This quarterly magazine seems to fill a most useful place in forming a channel of intercommunication between naturalists north of the Tweed.

Memorie della Soc. degli Spectroscopisti Italiani, December. This number contains a paper be G. Lorengoni, On the Observation of a partial eclipse of the sun in May last, observed by the spectroscopic and direct view methods. He discusses at length the advantages and accuracy of each method, and concludes that the former method is the best of the two.—G. de Lisa gives a table of spots on the sun, observed at Palermo in December, giving a mean of about eight spots each day.

The January number contains a note that a new Spectroscopic Station has been established at Turin, and an equatorial by Fraunhofer has been erected there.—Prof. Draper contributes a paper on his beautiful diffraction of spectrum photographs, similar to the account of the same in NATURE some weeks ago.

Astronomische Nachrichten, No. 1,978.—M. M. Henry gives the elements of planet (126) Velléda, epoch 1874, January, 0·0 Greenwich M.T.

$$\begin{aligned} M_0 &= 149^\circ\ 55'\ 51''{\cdot}1 \\ \Pi &= 347^\circ\ 49'\ 11''{\cdot}3 \\ \Omega &= 43^\circ\ 10'\ 12''{\cdot}8 \\ i &= 2^\circ\ 56'\ 10''{\cdot}6 \\ \phi &= 6^\circ\ 5'\ 31''{\cdot}4 \\ \mu &= 930''{\cdot}9792 \\ \log.\ a &= 0{\cdot}3873777 \end{aligned}$$

Leopold Schulhof gives the following elements of the comet discovered by Winnecke in February last:—

$$\begin{aligned} T &= 1874, \text{ March, } 9{\cdot}95342 \text{ Greenwich Time} \\ \Pi &= 300^\circ\ 36'\ 42'' \\ \Omega &= 31^\circ\ 31'\ 18''{\cdot}2 \\ i &= 58^\circ\ 17'\ 14''{\cdot}5 \\ \log.\ q &= 8{\cdot}642852 \end{aligned}$$

The star in Perseus RA $2^h\ 13^m\ 56^s$ Dec. + $58^\circ\ 1'\ 53''{\cdot}5$ has been observed by A. Kruger to have varied from 8·5 mag. to 10 mag. in November 1872, and to have increased to 8·5 again in January last.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, March 25.—John Evans, F.R.S., president, in the chair.—The following communications were read:—On the Upper Coal-Formation of Eastern Nova Scotia and Prince Edward Island, in its relation to the Permian, by Principal Dawson, F.R.S. The author described the Carboniferous district of Pictou county as showing the whole thickness of the Carboniferous system arranged in three synclinals, the easternmost consisting of the Lower series up to the Middle Coal-formation, and including all the known workable Coal-measures in the district—the second towards the west of the middle and the lower part of the Upper Coal-formation—and the third showing in its centre the newest beds of the latter. On the north the bounding anticlinal of the first depression brings up the New-Glasgow Conglomerate, which contains boulders 3 ft. in diameter, often belonging to Lower Carboniferous rocks, and represents the upper part of the Millstone-grit or the lower part of the Middle Coal-formation. The author regards this as representing an immense bar or beach, which protected the swamps in which the Pictou main coal was formed. The succession of the deposits above the Conglomerate was described in some detail as seen in natural sections. The Upper Coal-formation, as shown in the section west of Carribou Harbour, consists of—(1) Red and grey shales, and grey, red, and brown sandstones; and (2) Shales, generally of a deep red colour, alternating with grey, red, and brown sandstones, the red beds becoming more prevalent in the upper part of the section. In Prince Edward Island beds apparently corresponding to these are found, and also gradually become more red in ascending. These are overlain, apparently conformably, by the Trias. The author gave a tabular list of 47 species of plants found in the Upper Coal-formation of Nova Scotia and Prince Edward Island, and stated that all but about ten of these occur also in the Middle Coal-formation. The number of species decreases rapidly towards the upper part of the formation; and this is especially the case in Prince Edward Island, some of the beds in which are considered by the author to be newer than any of those in Nova Scotia. The plants contained in the upper deposits were compared with those of the European Permian, and a correlation was shown to exist between them, so that it becomes a question whether this series was not synchronous with the lower part of the Permian of Europe, although in this district there is no stratigraphical break to establish a boundary between Carboniferous and Permian. The author therefore proposes to name these beds Permo-Carboniferous, and regards them as to some extent bridging over the gap which in Eastern America separates the Carboniferous from the Trias.—Note on the Carboniferous Conglomerates of the Eastern Part of the Basin of the Eden, by J. G. Goodchild.—An Account of a Well-Section in the Chalk at the north end of Driffield, East Yorkshire, by R. Mortimer.—On Slickensides or Rock-Striations, particularly those of the Chalk, by Dr. Ogier Ward.

Royal Horticultural Society, April 1.—Scientific Committee.—Dr. Hooker, C.B., Pres. R.S., in the chair.—Prof. Thiselton Dyer exhibited seeds of the plant called in gardens *Theophrasta imperialis*, sent from Rio Janeiro by Dr. Glaziou. From the evidence now forthcoming it appears that the plant belongs to a different family, *Sapotaceæ*.—Dr. Hooker showed a photograph from Mr. Russell, of Falkirk, of a fruiting specimen of *Encephalartos villosus*, sometimes called in gardens *Zamia Mackenii*. The plant is a native of Natal, and a similar species has been discovered on the Niger by Barter, and a third in Zanzibar, by Kirk. A plant discovered by Schweinfurth in Central Africa is probably the same as that mentioned by Kirk.—Dr. Masters presented a classified list with notes of species of Passiflora and Tacsonia cultivated in European gardens.—Mr. Renny made some observations on the drawing, by Montagne, of *Artotrogus*, exhibited at the last meeting, which together with the original specimens, Mr. Berkeley had been kind enough to allow him to examine leisurely. He was able to clear up a mistake which De Bary seems to have fallen into in his description of *Peronospora infestans* (Ann. des Sc. Nat., 4e sér., t. xx. p. 105, 1863). De Bary had not met with the resting spore of that species, but suggested that Montagne's *Artotrogus hydnocarpus* might be the desired organ; but he had doubts on the point, as Montagne had written to him that he found it also on Turnip. The facts are, that Mr. Broome found a mould on decaying Turnip, which he sent to Montagne, who pronounced it to be a species of his genus *Artotrogus*, though he does not appear at any time to have supplied a specific name. He doubtless announced to De Bary that *Artotrogus* was to be met with on Turnip, and it was De Bary's assumption that *A. hydnocarpus*, the only published species, was the one spoken of. De Bary, having a confident belief that the various species of *Peronospora* are parasitic each only on the plants of one genus, or at most of one family, seems to have been thus led to the doubt he has expressed.

General Meeting.—H. Little in the chair.—Prof. Thiselton Dyer commented on the interesting plants exhibited. Amongst these were the two forms of *Primula verticillata*, one from Sinai the other from Abyssinia; *Boronia megastigma*, a new Australian plant with a very agreeable smell; the stem and foliage of the splendid Bamboo *Dendrocalamus giganteus*, in cultivation at Sion House; and cut blooms of *Sterculia nobilis* from the same collection.

Entomological Society, April 6.—Sir Sidney S. Saunders, president, in the chair.—Mr. Frederick Smith made some interesting observations relative to the habits of the bee-parasites belonging to the genus *Stylops*.—Major Parry communicated a paper entitled Further Descriptions of Lucanoid Coleoptera; and Mr. Smith read descriptions of the *Tenthredinidæ* and *Ichneumonidæ* of Japan, from the collections of Mr. George Lewis.—Further notes were read from Mr. Gooch, of Natal, respecting the destruction of the coffee plantations there, by Longicorn Beetles.

Royal Astronomical Society, April 10.—Prof. Adams, president, in the chair.—Mr. De la Rue gave a verbal description of a piece of apparatus which he had devised for carrying out M. Janssen's method of photographing Venus near to ingress and egress upon the sun's disc. The instrument is intended to be attached to the photo-heliographs and weighs less than 11 lbs., inclusive of a small driving clock, which carries a revolving plate of about 10 in. in diameter, on which small photographs of Venus and the sun's limb are to be taken in rapid succession. Lord Lindsay also described the form of instrument which he had devised for the same purpose; it appeared to be very similar to that described by Mr. De la Rue, except that it is mounted on a separate pillar from the telescope in order to avoid tremors.—Lord Lindsay also read a paper On a Method of Determining the Solar Parallax, from observations to be made at the next opposition of Juno, which occurs in November of this year. He proposes, while in the Mauritius, to make a series of heliometric measures of the distance of Juno from the nearest fixed stars; and by comparisons of the measures taken soon after Juno has risen above the eastern horizon with those taken before it sets at the western to determine the terrestrial parallax. By this method he will be able to make his measures during all the clear nights of the month or six weeks before and after opposition. And although the parallax will be considerably less than in the case of Venus, he considered that he had reason to hope that the probable error of the result would, owing to the number of the measurements and the ease of dealing with points of light instead of discs, be less than either in the case of the transit of Venus or the opposition of Mars.

Society of Biblical Archæology, April 7.—Dr. Birch, president, in the chair.—The following papers were read: On Four Songs contained in an Egyptian Papyrus in the British Museum. Translated with notes by C. W. Goodwin. Of these four songs three partook of the same nature, and were amatory compositions, written in a highly imaginative and poetical style with much voluptuousness of expression, having a very striking resemblance extending throughout whole passages, to the language of the Canticles. The fourth song or hymn is of a very different nature, and is evidently one of the solemn dirges used at festivals during the exhibition of the figures of Osiris, as related by Herodotus. This hymn is in the text ascribed to King Antuf, a monarch of the XIth dynasty.—Nimrod et les Ecritures Cuneiformes, by Joseph Grival (read in English). In this essay the author maintained that Merodach, under his Accadian name of "Amarud the eldest son of the Lord of Urhi," was identical with Nimrod the "géant chasseur" of the Septuagint.

EDINBURGH

Royal Physical Society, March 25.—On some Organisms found in the Stomach of the Herring, by F. W. Lyon.—Note on Entozoa, genus *Bothriocephalus*, found in the intestinal canal of a fish (*Cottus scorpius*), by James M'Bain.—On Recent Meteoric Chemistry, by Andrew Taylor. Mr. Taylor, in this paper, gave a *résumé* of the present state of our knowledge of the chemistry of meteorites.—On British Madreporiæ, by C. W. Peach, A.L.S. Mr. Peach read a paper on this subject, first stating that his attention had been drawn to the subject by a paper by Prof. P. Martin Duncan, on the Madreporiæ dredged by the explorers in the *Porcupine* in 1869 and 1870. He then exhibited a series of specimens he had collected in the seas of Shetland, Cornwall, &c., the most abundant being Caryophyllia, varieties *Smithii* and *Borealis*.—On the Fossil Plants of the Silurian Rocks of the Pentland Hills and of Sutherlandshire, by C. Wm. Peach, A.L.S. In this paper Mr. Peach showed that one of the large plants collected by Mr. Brown in the Upper Silurian rocks of the Pentlands was identically the same and in a similar matrix as the one collected by him in Sutherlandshire. This same plant had also been found in the Upper Silurian of the Gaspe sandstones in Canada. He further said that the rocks in Sutherlandshire were Lower Silurian, thus showing that land plants—and of a pretty high type—came in much earlier there than from either of the other localities.

PARIS

Academy of Sciences, April 6—M. Bertrand in the chair.—The perpetual secretary announced to the Academy the loss which it had sustained in the person of M. P. A. Hansen, correspondent for the Astronomical section, who died at Gotha on March 28.—The following communications were read:—On Polygons inscribed in and circumscribing curves, by M. Chasles.—Solar Cyclones: conclusion of the reply to Dr. Reye, and observations concerning an article from the "Bibliothéque universelle" of Geneva, and a reclamation by M. Norman Lockyer, by M. Faye. The author has tabulated the dates, localities, times, velocities, &c. of thirty-one cyclones.—Earthquake shocks felt in Algeria on March 28, 1874; a letter from M. Ch. Sainte-Claire Deville to the perpetual secretary. The communication included a note from Captain Brocard, containing a seismo-graphic indication of the shocks.—Observations made at the Observatory of Toulouse during the months of February and March, 1874, by M. F. Tisserand. The author communicated observations on the eclipses of Jupiter's satellites, and announced at the same time that regular observations of sun-spots had been organised with an equatorial of 0·108 m. aperture, after the method of Carrington.—Experimental researches on bi-hydrated sulphuric acid, by MM. Js. Pierre, and E. Puchot.—Scientific ascent to a great height made (in a balloon) on March 22, 1874, by MM. J. Crocé-Spinelli and Sivel. The authors had ascended 7,300 metres, the temperature at that elevation being −22°. The observations recorded in this communication are spectroscopic and physiological. Particular attention was given to the two obscure bands right and left of the double D line. At about 5,500 metres the right-hand band disappeared, and the band to the left vanished at about 7,000 metres, thus confirming M. Janssen's idea of these bands being of terrestrial origin. The observers adopted M. Bert's suggestion of respiring oxygen to correct the effects of the rarefaction of the air. A carrier pigeon released at 5,000 metres tried at first to remount to its cage, but finally descended, describing curves of from 200 to 300 metres in diameter, with a velocity of translation of about 40 or 50 metres per second.—Action of electric fluid upon gases, third note, by M. Neyreneuf. The author promised from his observations a satisfactory explanation of the stratification of the electric light.—On a new process for the study and determination of the alcohol in wines, by M. Duclaux. The process depends upon the fact that mixtures of alcohol and water give for different compositions different numbers of drops when allowed to flow from a pipette of constant orifice. (The method is a practical application of Dr. Guthrie's researches upon drops to which no allusion was made.)—Note accompanying the presentation of new astronomical objectives of large dimensions, by M. Secretan. The largest was 24 centim. in diameter and had a focal distance of 3·25 metres. Its price was 6,300f.—On a new (electric) couple specially prepared for the application of continuous currents in therapeutics, by M. J. Morin.—On a system of continuous alarm signals to prevent railway collisions or collisions of ships at sea during foggy weather, by M. C. J. de Mat.—Geological considerations on the probable origin of the drift soil called *diluvium*, by M. E. Robert.—On the employment of coal-tar alkalies for the destruction of *Phylloxera*, by M. A. Rommier.—Direct construction of the centre of curvature in a point of the section made in a surface by any plane, by M. A. Mannheim.—On the diffusion between moist and dry air through a septum of porous clay, by M. L. Dufour.—Measurement of the electromotive force of batteries in absolute units, by M. A. Crova.—Density of hydrogen combined with metals, by MM. L. Troost and P. Hautefeuille. The observed density is about 0·625.—Experiments concerning combustion in the animal organisation, by M. P. Schutzenberger.—On the brominated derivatives of pyruvic acid, by M. E. Grimaux. The author described di- and tri-brompyruvic acids and touched upon the constitution of the acid itself.—Modifications employed in the preparation of iron reduced by hydrogen, for the purpose of obtaining the metal perfectly pure, by M. Crolas.—Note on the determination of lime in meteoric waters, by M. H. Marié-Davy.—On asphyxia from insufficiency of oxygen, by M. Félix le Blanc.—On the use of oxygen in ballooning, by M. W. de Fonvielle.—Injection of ammonia into the veins to oppose accidents caused by snake bites, by M. Oré.—On the functional irritability of the stamens of *Berberis*, by M. E. Heckel.

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THURSDAY, APRIL 23, 1874

HERBERT SPENCER'S SOCIOLOGY

The Study of Sociology. By Herbert Spencer. (London: Henry S. King and Co.)

THERE are not a few signs, of which the book before us is an important one, that thought is moving in the direction indicated by Mr. Mill in the quasi-prophetical conclusion to his "System of Logic," where he expresses his belief in the prominence of sociological inquiries in the intellectual achievements of the next two or three generations of European thinkers. What has been called by Hegel the speculative historical method has taken a considerable step in advance since Mr. Mill wrote thus. History, written from the speculative or philosophical stand-point, may be regarded as a special Sociology—Sociology, that is, applied to the investigation of the laws of growth and development of some one society, as exhibited either throughout its whole career, or within some limited period. General Sociology stands to history in a position analogous to that occupied by general physiology with regard to the special physiology of man or of any other species of animal. This analogy will serve to throw some light upon the fact that there has been much greater progress made in the special department of speculative history than in the wider field of Sociology. Up to a certain point, the broader generalisations of the higher science must be preceded by the more restricted generalisations of the lower. But when this point has been reached, when the higher principles have been formulated with approximate completeness, then the advances along the lines of general and special thought will proceed *pari passu*; progess in one will by its reflex influence make possible a correlative progress in the other. The relations between Sociology and history appear to be nearing this stage.

Division of labour obtains in literature as well as in industry. His special turn of mind, not less, perhaps, than the character of the task to which he has devoted himself, has made Mr. Spencer a labourer in the wider field of Sociology. The volume under review, taken in connection with the two parts already issued of the "Descriptive Sociology," of which Part I. has been noticed here,* enables us, to some extent, to anticipate the character of the more elaborate and comprehensive work, the "Principles of Sociology," the promised fourth division of Mr. Spencer's "System of Philosophy." In the "Descriptive Sociology" we have an insight into the laborious thoroughness with which Mr. Spencer is preparing the foundations for his generalisations, while in the "Study of Sociology" we are introduced to his conception of the nature of the social science, of the difficulties in the way of the sociologist, and of the discipline necessary to the formation of a habit of thought adapted to sociological inquiries.

It is not necessary here to follow Mr. Spencer into the polemic contained in the first two chapters of his book, directed against the popular notions of sociological phenomena; against the dogmatic, unscientific, and off-hand way in which important and complicated sociological questions are decided not only by the vulgar, but also by men who are guided by a strict scientific method in the less intricate questions of physical science; and against the supporters of the special-providence and great-man theories of history. Mr. Spencer being the assailant, it is almost superfluous to say that the polemic is a vigorous one; indeed it may be open to question whether the assault would not have been more successful had it been conducted with less vigour and more circumspection. To use Mr. Spencer's own metaphor, a considerable correction for the "personal equation" of combativeness will be found necessary.

The possibility of a science of Sociology is shown in various ways. One cause of the denial of this possibility is to be found in the prevalent confusion between a science and an exact science. Sciences are either merely logical or mathematical also; in Mr. Spencer's phraseology, qualitative or quantitative. It is not affirmed that Sociology can be a quantitative science; but this is not to deny its scientific character altogether. Sociology presents in an extreme form that absence of quantitative definiteness which geology, biology, and psychology present in a considerable, though less, degree. Mr. Spencer puts his own case and that of the objectors to the scientific character of Sociology very forcibly in the following dilemma:—"In brief, then, the alternative positions are these. On the one hand, if there is no natural causation throughout the actions of incorporated humanity, government and legislation are absurd. Acts of Parliament may, as well as not, be made to depend on the drawing of lots or the tossing of a coin; or rather, there may as well be none at all: social sequences having no ascertainable order, no effect can be counted upon—everything is chaotic. On the other hand, if there is natural causation, then the combination of forces by which every combination of effects is produced, produces that combination of effects in conformity with the laws of the forces. And if so, it behoves us to use all diligence in ascertaining what the forces are, what are their laws, and what are the ways in which they co-operate."

Sociology is concerned with men aggregated into societies. Aggregates derive their essential properties from the natures of the individuals of which they are composed. Aggregation, though it may foster the developme
some, and check that of others, of the characte
constituent units, cannot give rise to social
which there is no foundation in those of t
Setting out from this axiomatic princi
scribes "the growth, development, str
tions of the social aggregate, as broug
mutual actions of individuals whose na
like those of all men, partly like those o
partly distinctive. . . . Not that the
exhibits these or those special truths, but
having certain properties, an aggregate of a
have certain derivative properties which for
matter of a science."

Nowhere, perhaps, than in sociological p
the truth of the doctrine of evolution, the cen
of Mr. Spencer's philosophy, more strikingly
and nowhere, subject to proper limitations, is
prove more serviceable. To borrow Comte's ph
Sociology is either dynamical or statical. The
of Sociology is throughout an exemplification of

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* See NATURE, vol. viii. p. 544.

The statics of Sociology, at any given epoch, deals with phenomena which are the results of evolution. When Sociology is regarded in its dynamical aspect, the doctrine of evolution properly understood and limited, recognising and accounting for both the relative perfection and imperfection of a given social state, occupies the true mean between the altogether optimist view of social progress which finds expression in the lines—

> As round and round we run,
> Ever the truth comes uppermost,
> And ever the right is done;

and the altogether pessimist view embodied in the dictum of a distinguished living thinker, "The history of mankind is a huge *pis-aller*." But when Sociology is regarded in its statical aspect, an abusive use may easily be made of the doctrine of evolution. A given social state bears a relation to the past social states from which it is an outgrowth, and also to existing circumstances and conditions. Led away by the tendency of modern thought, so happily described by Mr. Bagehot as making everything "an antiquity," the sociologist is apt to dwell upon the first of these relations, to the exclusion of the second. From such one-sidedness Mr. Spencer does not appear to be altogether free.

It is always useful to know the nature, the magnitude, and the position of the difficulties that have to be encountered in the course of an inquiry. Mr. Spencer has given more explicitly and in fuller detail than any previous writer has done, an analysis of the difficulties in the way of sociological investigations. These difficulties are objective and subjective; difficulties inherent in the object of sociological science, and difficulties originating in the observer himself. The data of Sociology, the actions of men incorporated into societies, are distributed over long periods of time, and wide areas of space. The sociological inquirer must necessarily rely for his data upon past and contemporary records. But records may not exist; deep-lying circumstances of importance may be obscured by superficial circumstances; evidence will suffer vitiation through the want of perspicacity or of impartiality in the observer. A comprehensive, patient, and judicious employment of the comparative method is the means by which order can be educed out of the mass of data which the recorded histories of cieties offer. Mr. Spencer and his collaborateurs will deserve the g attitude of every sociological inquirer, for the extensive co lection and collation of these materials, now in progress the atlas-like folios of the "Descriptive Sociology."

Formidable re the objective difficulties which beset sociological hes, not less formidable are the subjective di This class of difficulties originates either in ual or in the emotional character of the observ of a faculty adequate in plasticity many-sidedness and complexity of tion, and the tendency to automor the measure of things, are the and th bstacles to Sociology. Automor st fertile sources of error. "To pencer, "any fact in social evolu resulting from the joint actions rtain natures; and this even by le to do but very imperfectly. Our interpretation must be automorphic; and yet automorphism perpetually misleads us."

In Sociology man is at once the observer and the observed; the inquirer is a unit of the aggregate whose laws he is investigating. We may observe a transit of Venus with the impartiality due to the absence of personal concern; we are not impartial observers of a social event with which our own interests are intimately bound up. Accuracy of observation is thus interfered with by sentiment. From the observer's emotional nature spring the various kinds of bias, educational, patriotic, class, political, and theological, described and abundantly exemplified by Mr. Spencer in a succession of chapters deserving of careful study, but to which space prevents more than a reference.

Attention to questions of scientific discipline and method is so rare among scientific men, that Mr. Spencer's book would deserve commendation for this feature, if for no other. Discipline should have reference to the work to be performed. Sociology being the most complex of the sciences, the sociological inquirer needs a discipline capable of producing an adequately powerful instrument of research. Falling back upon his classification of the sciences, into Abstract sciences, which investigate the *forms* of phenomena, Abstract-concrete sciences, which investigate the *factors* of phenomena, and Concrete sciences, which investigate the *products* themselves in their totality, Mr. Spencer shows the need in Sociology of the discipline in the necessities of relation derived from the first; in the distinctness given to the notion of simple causation derived from the second; and in the formation of the conception of continuous, complex, contingent, and fructifying causation derived from the third. Not, of course, that there can be an exhaustive or even a deep study of all or any of these sciences; a disciplinary study is all that is contended for, a study sufficient to enable the sociological inquirer to grasp the cardinal ideas proper to each science. But there is a more intimate dependence of Sociology upon the sciences of physical and psychical life, therefore the sociologist stands in need of a deeper acquaintance with biological and psychological truths. Amongst the most interesting and valuable chapters in the book are those in which Mr. Spencer enforces the need of an adequate preparation in biology and psychology. Positive arguments are supplemented by negative arguments, arguments based upon striking exemplifications of the errors that have arisen in the practical sciences of politics and education from ignoring biological and psychological teachings.

The view taken by Mr. Spencer of the method proper to sociological inquiries seems, as far as can be gathered from his own procedure, to differ little from that advocated and expounded by Mr. Mill. Placing Sociology next after psychology in his System of Philosophy, and asserting, as he does everywhere, the dependence of social phenomena upon psychological facts connected with the social units, Mr. Spencer's method appears to be to trace out deductively the connection of the empirical sociological truths, arrived at by generalisation from the data furnished by historical records, with the ultimate laws of human nature established by psychology. This is essentially Mr. Mill's inverse or historical deductive method applied to sociological inquiry.

In its style the "Study of Sociology" somewhat disagreeably reminds the reader of one of Mr. Spencer's earliest works, "Social Statics." It has two main faults—it is needlessly polemical in its tone, and it is disfigured by numerous unscientific exaggerations of language. Mr. Spencer is undoubtedly right in defending against Mr. Arnold the superiority of the guidance of psychology and logic, over mere linguistic culture, in producing a style clear, forcible, and free from tautology. But there is neither psychological nor logical defence for the two faults indicated. Amongst minor faults of style may be enumerated an occasional laxity in the use of analogies; the degeneration of the psychologically sound practice of prefacing the enunciation of an important truth by a forcible illustration of it into a disagreeable mannerism, some very marked examples of which occur in the opening paragraphs of several of the chapters; the frequent occurrence of words ineuphoniously formed, to say the least of them, such as "re-revenge," "irrelation," "wholesaler;" and the over-abundant formation of compounds, especially noticeable in the compounding of the adjective and its qualifying adverb, as in "logically-conclusive," "profoundly-untrue," "equally-long."

Some of the defects pointed out are probably traceable to a desire to popularise the work as far as possible, in forgetfulness, to some extent, of the necessity to maintain the character which should appertain to it as one of a scientific series. But, while it is to be regretted that there should be such blemishes to mar the general effect of a book so full of suggestive thought and of appropriate illustrative facts, it must be allowed that the "Study of Sociology" forms a valuable addition to sociological literature, and leads the student in this department of human thought to anticipate with pleasure the appearance of the work to which it and the "Descriptive Sociology" are but forerunners.

W. H. BREWER

FRITSCH'S "SOUTH AFRICAN RACES"

Die Eingeborenen Süd-Afrika's, ethnographisch und anatomisch beschrieben. Von Gustav Fritsch. (Breslau: Hirt, 1872.)

IT is to be hoped that this work will have a good effect on the Science of Man, as a much-needed example which, once seen, will no doubt be followed. So far as the writer of the present notice is aware, the attempt at a systematic monograph of a race has never yet been made with so near an approach to success. With all our hundreds of volumes full of information as to the lower races of mankind, it is generally a difficult task for the ethnologist to piece together out of them anything like a complete picture of any tribe, with scientific fulness and accuracy of bodily, mental, social, geographical, and historical detail. Where, for instance, could he go for full information as to the two African races of whom Dr. Fritsch's work treats, the Kafirs and Hottentots?

The student's best source has been hitherto the conscientious dictionary-like summary, brief yet tedious, contained in 60 pages (Vol. II.) of Waitz's "Anthropologie der Naturvölker." The chapters in Mr. J. G. Wood's "Natural History of Man" are fuller and more life-like, but they are far too popular in topics as well as in style. Nor had either of these writers ever lived among the races about whom he compiled information. From a study of the descriptions drawn up by travellers, missionaries, and officials, who have known the Kafirs and Hottentots by personal knowledge, it is possible to get much of the information wanted, but how long will it take even to glance over the volumes of Shooter, Galton, Callaway, Hahn, Casalis, Grout, Maclean, Andersson, and a dozen more? Each other savage or barbaric race of the world demands in like manner the reading through of a small library, consisting mostly of miscellaneous literary matter, in which the ethnographic information is imbedded. The state of things is briefly this, that anthropological evidence is at present so bulky and so scattered, as to be

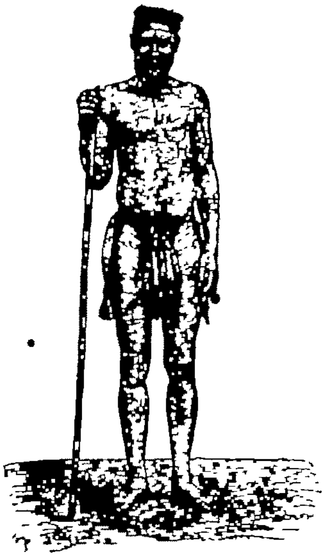

FIG. 1.—Zulu of Natal.

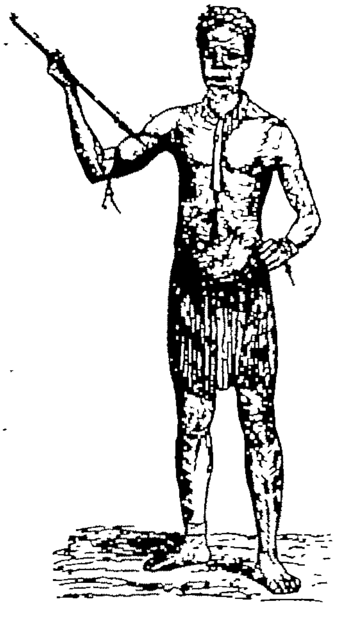

FIG. 2.—Young Kafir.

unmanageable except by those who can give half a lifetime to it. It is highly desirable to have the whole available knowledge as to each race condensed into a monograph like the present, by a competent ethnographer who knows that race by personal study in its home. It would be a real service to the ethnographers now at work drawing up accounts of native tribes in India and elsewhere, to put into their hands Dr. Fritsch's book as a model. As with all its excellencies of plan and execution, it is in many respects open to improvement, it would serve as a stepping-stone to yet more perfect works.

In popular language, the two indigenous races of South Africa are known as *Kafirs* and *Hottentots*, one the well-known Moslem term for "infidels" picked up by the Portuguese from the Arab traders of the sixteenth century, the other an imitative epithet, "hot-en-tot," given by the Dutch colonists to the tribes using "clicks" in their speech. Neither term is now satisfactory, and Dr. Fritsch is justified in adopting the native names by which the two races denote themselves. For the *Kafir* tribes he uses the term *Abantu*, *Bantu* (plural of *ntu*, a man), and for the *Hottentot* tribes their designation of *Koi-koin* (*i.e.* "men of men," from *koi*, a man).

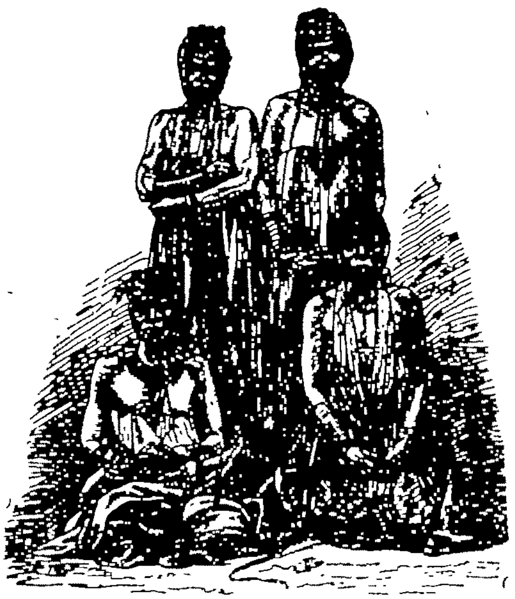

FIG. 3.—Sandili's Wives.

Dr. Fritsch, as a professed anatomist, examines with almost exhaustive minuteness the bodily characters of these two races. The closer appreciation of race-types, which is now supplanting the vaguer generalities of twenty years ago, is in no small measure due to the introduction of photographic portraits, instead of the old misleading sketches by artists unable to clear their minds of the artistic types of Europe. Without photography it would be impossible to obtain a collection of portraits such, for instance, as those lately published in Colonel Dalton's "Descriptive Ethnology of Bengal." The portrait engravings from South African photographs in Dr. Fritsch's album (unfortunately arranged on somewhat different dimensions from the volume it accompanies) are at the same high level of truth and art. When race-types are so well-marked as among these South African tribes, even small figures will show their principal physical peculiarities. A selection from the small-scale woodcuts in the main volume, likewise taken from photographs, are here produced from copies of the blocks lent by the publishers.

Figs. 1 and 2, representing a middle-aged and a young Kafir, show the characteristic slimness of the figure, due to the wall-sided chest and narrow hips. The lean forearm, a peculiar conformation of the deltoid and biceps, a somewhat finely-formed hand, and an ungraceful setting back of the lower extremities and inclination of the pelvis, are other points of speciality. The narrow skull is well seen in the figures, with the broad-winged flattened nose showing the nostrils in full face, the fleshy pouting lips, and the hair naturally felted. Add to this the deep-brown colour of the skin, which is shown in No. 1 of the specimen tints given in a table at the end of the volume, with the deep-brown eyes and black frizzy hair, and the total as nearly represents the ideal Kafir of the Ama-Xosa type as ethnologists can conceive it. Fig. 3, representing a group of wives of a chief,[*] shows with coarse distinctness the typical Bantu features.

In strong contrast with this Kafir type is that of the Koi-koin or Hottentots, including as one of its divisions the Bushmen. Whereas the dark-brown or almost black Zulu stands little short of 5 ft. 8 in., the Hottentot, whose brownish-yellow complexion has been compared to a dry leaf, averages only 5 ft. 3 in., and the tiny dirty-yellow Bushman under 4 ft. 8 in. Bearing in mind their yellow complexion and diminutive size, some idea of the Bushman type may be gained from Fig. 4. The high cheek-bones and pointed chin give the face its peculiar triangular shape, while the characteristic snub-nose is shown in the old Bushman, Fig. 5.

Dr. Fritsch justly observes that the Bantu and Koi-koin races have hardly any essential race-character in common, unless it be the crisped hair; and even this is generally (though not through all varieties) distinct, the Bantu hair being irregularly felted into a mass, whereas the Koi-koin hair grows in little tufts, which have been compared to the bristles of a blacking-brush. The steatopygy of the Hottentot-Bushman women is shown by an extraordinary collection of portraits in Dr. Fritsch's volume; few physical race-characters are more striking than this, and it is unfortunate that illustrations of it cannot be inserted here. As in other parts of their structure, so in cranial proportions the two races in question are markedly distinct, as is fully proved by the set of lithographed skulls with tabulated measurements. The Kafir skull is narrow and high, the proportion of length to breadth being about 100 : 71·9, while the height may be taken at 73·8, being thus slightly greater than the breadth. The Hottentot skull, on the other hand, is narrow and low, the proportion of length to breadth being about 100 : 72·7, while the height of such a skull might be only 71, which is less than the breadth. The Bushman skull shows this character in still more extreme proportions in a cranium whose length is 100, the breadth being 73·8, against a height of 70·2, which gives about double the difference of the Hottentot.

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[*] This group illustrates in a curious way the conventional but not irrational development of the ideal of beauty from the ordinary forms of normal life. This ideal once fixed among any nation, there ensues a desire to exaggerate it. In the present instance, in grown-up Kafir women, the tendency of the breasts to become long and pendulous is considered not contrary to beauty, and is accordingly artificially increased by binding down, as shown in the figure.

The elaborate anatomical data amassed by Dr. Fritsch may afford the means of more fully working out the ethnological problems of the South African races. The evidence here brought forward of the more extreme characters of the Bushman type as compared with the Hottentot, seems to tell in favour of the view put forward by Prof. Huxley some years ago, that the Hottentots are the result of crossing between the Bushman and the Negroid tribes. Beyond this, there naturally arises another question: do the Kafir tribes, with their complexions varying from dark-brown to blue-black, owe their bodily differences from the Negro of Equatorial Africa to an intermixture of Bushman blood during a long course of ages. The evidence of language is here important. So far as it is concerned, the Kafir of South Africa is essentially a Negro, for his dialects belong to the great series of prefixing languages, the peculiar character of which is so well shown in the formation of the plural. Just as the Mpongwe language of the Gaboon makes the plural of *omamba*, snake; *imamba*, snakes; and farther east the individual inhabitant of *Unyamwezi* is a *Mnyamwezi*, and the people as a whole are *Wanyamwezi*:—so in Zululand *umuntu* is a man; *abantu*, men; and *Amazulu* is the plural name of the nation of whom an individual is *Uzulu*. The Bushman-Hottentot, or Koi-koin group of languages, are on the other hand distinguished by their tendency to monosyllabic words, their suffixes, and the "clicks" which to so extraordinary an extent are used as consonants. According to Dr. Bleek's classification, this family of languages has also relations farther north on both sides of the Continent; but this is a point which requires further examination. Now, though the fundamental types of the Kafir and Bushman languages are so absolutely distinct, it has come to pass that certain of the Kafir tribes, notably the Zulus, use to some extent in their speech clicks of the Hottentot type, whereas nothing of the kind appears in the languages of their Negro kinsfolk of the equator. Did they catch this habit by mere imitation from the Hottentots and Bushmen, or, as seems more in accordance with experience, did Hottentot mothers in past generations teach it to children of a mixed race? This line of argument, it seems to me, may possibly lead to more definite results.

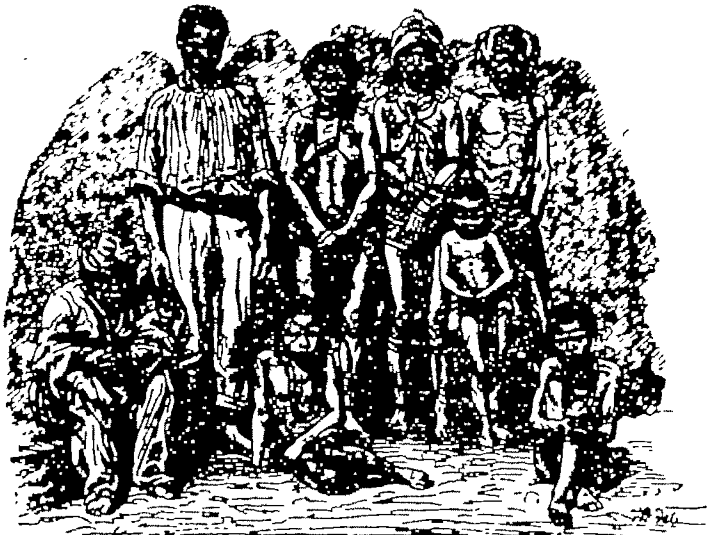

Fig 4.—Bushmen of Orange River Republic.

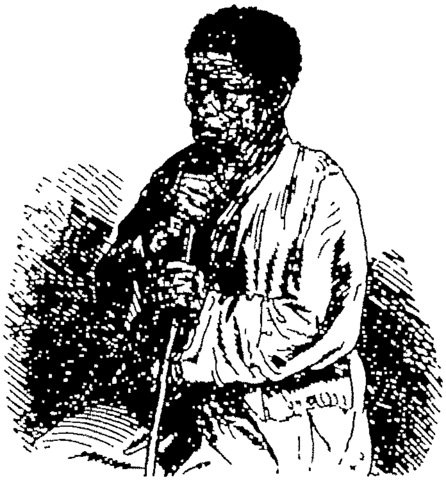

Fig 5.—Bushman of West Colony.

Dr. Fritsch gives a valuable summary of information as to the industrial, social, and intellectual condition of the South African races. The latter is not, however, of such special excellence as the descriptions of physical race-characters. Indeed, Dr. Fritsch is on the whole a better judge of bodies than of minds. His account of the native religions is below his general level, as may be judged from his describing the Zulu religion without mention or apparently knowledge of the remarkable native documents collected by Dr. (now Bishop) Callaway, which throw such clear light not only on the religious ideas of these barbarians, but on the origin and development of religion among mankind at large. That savage theologies show representative stages in the evolution of human thought, and as such deserve and repay the most careful study of their inmost meanings, is a fact which is daily coming into clearer view among ethnologists, but it seems hardly to have entered Dr. Fritsch's mind. While mentioning this weak point of his, it is worth while to notice that a much fuller dissertation on the native languages, such, for instance, as Prof. Steinthal might have drawn up, would have been of interest to students whose wants are only partially supplied by the meagre though valuable classificatory sketch here given, mostly on the authority of Dr. Bleek. Our author also shows glimpses of ill-temper in dealing with authors he dislikes, such as Mr. J. G. Wood, whom he falls upon in season and out of season. An instance of the latter kind of attack is seen where Mr. Wood, speaking in perhaps too enthusiastic terms of the physical beauty of youthful savages, naturally introduced the well-known story of Benjamin West, the Quaker painter, comparing the Apollo Belvedere to a young Mohawk warrior. Dr. Fritsch, quite missing the point of the story, solemnly quotes Mr. Wood as asserting, in proof of the classical beauty of the

Kafirs, that "an American Quaker, West, took the statue of the Belvedere Apollo for the representation of a Mohawk Indian." Having made Mr. Wood talk this extraordinary nonsense, he then reviles him for being illogical. This is not the treatment Mr. Wood merits. No one denies the faults of his work, especially the unhappy straining after the picturesque which has made so many of his artist's illustrations worse than worthless. But his genial and suggestive descriptions of South African native life give a permanent value to his popular volume, while in his special line as a student of savage arts and implements, Dr. Fritsch can hardly expect to rival him.

EDWARD B. TYLOR

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Flowers of the Primrose destroyed by Birds

FOR above twenty years I have observed every spring in my shrubberies and in the neighbouring woods, that a large number of the flowers of the primrose are cut off, and lie strewn on the ground close round the plants. So it is sometimes with the flowers of the cowslip and polyanthus, when they are borne on short stalks. This year the devastation has been greater than ever; and in a little wood not far from my house many hundred flowers have been destroyed, and some clumps have been completely denuded. For reasons presently to be given, I have no doubt that this is done by birds; and as I once saw some greenfinches flying away from some primroses, I suspect that this is the enemy. The object of the birds in thus cutting off the flowers long perplexed me. As we have little water hereabouts, I at one time thought that it was done in order to squeeze the juice out of the stalks; but I have since observed that they are as frequently cut during very rainy, as during dry weather. One of my sons then suggested that the object was to get the nectar of the flowers; and I have no doubt that this is the right explanation. On a hasty glance it appears as if the foot-stalk had been cut through; but on close inspection, it will invariably be found that the extreme base of the calyx and the young ovary are left attached to the foot-stalk. And if the cut-off ends of the flowers be examined, it will be seen that they do not fit the narrow cut-off ends of the calyx, which remains attached to the stalk. A piece of the calyx between one and two-tenths of an inch in length, has generally been cut clean away; and these little bits of the calyx can often be found on the ground; but sometimes they remain hanging by a few fibres to the upper part of the calyx of the detached flowers. Now no animal that I can think of, except a bird, could make two almost parallel clean cuts, transversely across the calyx of a flower. The part which is cut off contains within the narrow tube of the corolla the nectar; and the pressure of the bird's beak would force this out at both the cut-off ends. I have never heard of any bird in Europe feeding on nectar; though there are many that do so in the tropical parts of the New and Old Worlds, and which are believed to aid in the cross-fertilisation of the species. In such cases both the bird and the plant would profit. But with the primrose it is an unmitigated evil, and might well lead to its extermination; for in the wood above alluded to many hundred flowers have been destroyed this season, and cannot produce a single seed. My object in this communication to NATURE is to ask your correspondents in England and abroad to observe whether the primroses there suffer, and to state the result, whether negative or affirmative, adding whether primroses are abundant in each district. I cannot remember having formerly seen anything of the kind in the midland counties of England. If the habit of cutting off the flowers should prove, as seems probable, to be general, we must look at it as inherited or instinctive; for it is unlikely that each bird should have discovered during its individual life-time the exact spot where the nectar lies concealed within the tube of the corolla, and should have learnt to bite off the flowers so skilfully that a minute portion of the calyx is always left attached to the foot-stalk. If, on the other hand, the evil is confined to this part of Kent, it will be a curious case of a new habit or instinct arising in this primrose-decked land.

Down, Beckenham, Kent, April 18 CH. DARWIN

Signor D'Albertis' and Dr Meyer's Discoveries in New Guinea

HAVING just returned to Europe, I read in NATURE, vol ix. p. 77, a communication which contains an assertion of Dr. A. B. Meyer, to the effect that I did not cross New Guinea at all, and that he claims the honour of having done so himself.

From what Dr. Meyer says, the public are led to believe that I have claimed the honour of crossing this unknown and little-explored island; if he had read "A Month among the Papuans of Mount Arfak," he might easily have ascertained that I never asserted this. There the reader will see that I only claimed to have penetrated the country to a distance of thirty miles, and to have ascended to a height of between 3,000 and 4,000 feet; but I *was* the first European to see alive and shoot many rare Birds of Paradise peculiar to New Guinea. One of these was entirely new to science, and has been called *Drepanornis albertisi* by Dr. Sclater (NATURE, vol. viii. p. 305); it may be the same bird subsequently described as new by Dr. Meyer.

I have no wish to deprive the last-named gentleman of the honour of having crossed a greater or lesser portion of New Guinea, but I object most decidedly, either indirectly or by insinuation, to being deprived of the credit of being the first European to penetrate into the interior of that interesting country.

April 20 LUIGI MARIA D'ALBERTIS

Spontaneous Generation

MR. RAY LANKESTER's letter in last week's NATURE affords fresh evidence of his lack of acquaintance with the several stages through which the "spontaneous generation" controversy has passed, or he would not now cite as a "most important result" only made known by recent experimentation, a fact which has been well known and repeatedly verified since the time of Spallanzani. I allude to the influence of the prolongation of the period of exposure to heat in retarding or altogether arresting the putrefactive tendencies of organic solutions. I have not thought it needful on previous occasions to point out the various misconceptions and the apparent ignorance of facts shown by Mr. Lankester in his querulous communications to your columns on the subject of "Spontaneous Generation." There are one or two points, however, to which I will now venture to solicit his attention, and that of your readers generally.

Mr. Lankester says:—"It is probably now familiar to those interested in the matter, that the experiments of Dr. Sanderson have established the fact that in an infusion of turnips and cheese prepared as directed by Dr. Bastian, heating to a temperature of 102° C. is sufficient to prevent the subsequent development of life (Bacteria) in the infusion, even when the exposure to that temperature is only maintained for a few minutes." To this statement I have to add that since the publication of the experiments above alluded to by Dr. Sanderson, I have heated flasks, sealed in the ordinary way and containing the fluid above mentioned, to a temperature of 105° C. for ten minutes in a chloride of calcium bath, and have found these fluids swarming with Bacteria after six days. I have also heated in the same manner simple neutralised turnip-infusion (filtered through cotton-wool instead of filtering paper) to a temperature of 105° C. for ten minutes, and by subsequently keeping these less putrescible fluids at a higher temperature (about 35° C.) they became turbid and swarmed with Bacteria in three days. Neither Dr. Sanderson's experiments nor those of Mr. Lankester and Dr. Pode have, therefore, the cogency which Mr. Lankester imagines them to possess. But, as I have endeavoured to point out on a previous occasion (NATURE, vol. viii. p. 548), experiments of this kind at the present stage of the controversy can teach us nothing definitely as to the death-point of Bacteria and their germs, though they are of interest with regard to the question of the degree of heat which suffices to check the productivity of the fluids in question.

We are now told that Mr. Lankester himself, and those with whom he sides, are agreed as to the fact that Bacteria are killed at "a temperature a little below 70° C." Of course I cannot tell to what extent Mr. Lankester is in possession of the views of Prof. Huxley and others, but if what he states is really true, the statement is of a reassuring nature; it looks like progress, and leads me to hope that the only remaining doubt may soon be solved. How long does it take for the "through-heating" of certain "possible" Bacteria germs? This is now the knotty problem which, according to Mr. Ray Lankester, seems alone to require solution before we can positively decide as to the heterogenetic origin of Bacteria. Perhaps I may help him on his way to the solution of this difficulty by calling his attention to certain experiments made in Calcutta by Dr. Timothy Lewis, in reference to the existence of living tape-worm germs in cooked meat ("Report of Sanitary Commissioners with the Government of India, 1871"). Dr. Lewis says:—"The temperature of legs of mutton which had been put into the boiler almost as soon as the water was put into it averaged 140° F. (60° C.) in the interior at the moment the water had reached the boiling point (212° F.), and after boiling for five minutes the temperature had reached 170° F. (76° C.)." Now with these facts in his possession, and with some suggestions from physicists of his acquaintance as to the mode of conduction of heat generally, Mr. Lankester may perhaps soon solve his problem, so far as this is practicable. The problem itself may be stated thus:—If the through-heating of several pounds of protoplasm in the shape of a leg of mutton, when immersed in water, takes place at such a rate as to raise the central portions of the joint to a temperature of 60° C. by the time the water has reached 100° C., and if the exposure of the leg of mutton to this heat for the space of five minutes suffices to raise its central portions from 60° to 76° C., how many seconds, minutes, or hours will it take to heat an infinitesimal part of a grain of protoplasm (all through) to the temperature of 76° C.—that is, to a degree of heat decidedly above the death-point of bacterial protoplasm as given by Mr. Ray Lankester? The Bacterium-germ in question, it must be recollected, cannot be supposed to have undergone any extreme amount of desiccation previous to its immersion in the experimental fluid, since such desiccation would have already destroyed its life, according to Dr. Sanderson.

Whilst Mr. Lankester is seeking the solution of the problem above stated, perhaps he might with advantage also reflect a little more closely upon the possible value or otherwise of some of the negative results to which he is so fond of alluding. It is perhaps scarcely necessary for me to remind Mr. Lankester that the obtaining of such negative results is always easy, and may show nothing more than the relative incapacity of the experimenter for performing careful work according to instructions. Not long ago Mr. Lankester, upon the strength of his own negative results, triumphantly announced that he was about to prove to the world the falsity of my views, and so help to justify the opinion which he at the same time expressed as to my being "the mesmerised victim of delusion," "an abnormal psychological phenomenon," and many other fine things. But unfortunately for Mr. Lankester, just about the same time Dr. Sanderson (whose opinions he so much respects) had an opportunity of satisfying himself that I could demonstrate the experimental results which Mr. Lankester failed to obtain. Dr. Sanderson helped to show, in fact, that my positive results were worth more than the many negative results obtained by other workers.

Finally, I think it necessary to add a few words concerning the views of my colleague, Dr. Sanderson, on the subject of heterogenesis, simply because I find his experiments and supposed views frequently quoted by Mr. Lankester, and others, as evidence of the erroneous nature of my conclusions

I have been led by my experiments to believe in Heterogenesis and also in Archebiosis, but I regard the recognition of the present occurrence of Heterogenesis as of far more importance than the recognition of Archebiosis. Now the controversy between Needham and Spallanzani, and also that between Pasteur and Pouchet was as to the present occurrence or non-occurrence of heterogenesis. This was what they understood, and what the majority of people at the present day still understand, as "Spontaneous Generation." And as to the reality of this process, Dr. Sanderson has been convinced. He admits that Bacteria may appear in flasks, and other situations, where we are warranted in believing that no bacterial matter pre-existed—which is exactly equivalent to a belief in "Spontaneous Generation," in the sense implied by Pasteur and others. In support of this statement I have only to make the following quotations from his papers and reported speeches of the last two years. Referring to experiments made in 1871, Dr. Sanderson says: "Bacteria could not be shown to be present either actually or in germ in the healthy liquids or tissues, or in the products of healthy inflammation" (*British Medical Journal*, May 11, 1872, p. 508). This statement was made with reference to man, and also to the lower animals with which he had experimented. In another part of the same communication as it stands revised in the "Transactions of the Pathological Society," for 1872, Dr. Sanderson says: "If a few drops of previously boiled and cooled dilute solution of ammonia are injected underneath the skin of a guinea-pig, a diffuse inflammation is produced, the exudation liquid of which is found, after twenty-four hours, to be charged with Bacteria." Other chemical agents will act in the same way even when every precaution against external contamination has been adopted; and as a drop of this fluid introduced with equal care into the peritoneum of another animal is always capable of exciting the phenomena of pyæmia, Dr. Sanderson has made known the very important fact that this process "can be proved to be capable of originating from inflammations produced by chemical agents *under conditions which preclude the possibility of the introduction of any infecting matter from without.*" Again, in a speech delivered last month before the Clinical Society, and reported verbatim in the *British Medical Journal* for March 24, Dr. Sanderson insists upon the complete establishment of the truth of this latter proposition both for man and the lower animals. He says: "We must admit that the whole process of pyæmia can originate in the organism independently of external influences." But, as he also says: "In every pyæmic inflammation—whether it be a primary or a secondary one—in every form of pyæmic action, you have always the presence of septic products," that is of Bacteria. Now if Bacteria by their germs do not normally exist in the tissues of animals, and if you can determine their presence there at will under conditions which, as Dr. Sanderson says, "preclude the possibility of the introduction of any infecting matter from without," what must be the mode of origin of the Bacteria in such cases, and how can Dr. Sanderson do other than yield his assent to the doctrine of "Spontaneous Generation," or Heterogenesis, so far as the origin of Bacteria is concerned?

University College, April 6 H. CHARLTON BASTIAN

Earthquake in St. Thomas

ON the morning of the 11th instant at 4.30 A.M., a smart shock, accompanied by a rumbling noise, like that of a waggon rolling over rough pavement, travelling, as is usual here, from east to west, woke up the inhabitants of St. Thomas. It was followed within a few seconds by another shock, to the full as abrupt in its character as the first; the movement appeared to be not so much undulatory as vertical.

The concussion produced was felt still more distinctly within the harbour itself, where the jar communicated to the ships resembled, as one of the captains described it, that which might be produced by a heavy bale falling through the hatchways into the hold. Simultaneously the water of the bay, then perfectly still, assumed a turbid appearance, as though clouded by mud and sand; and a little later the surface was agitated by a strong ripple from the south, lasting some time.

On the same morning early the royal mail steamer *Corsica*, commanded by Capt. Herbert, was at anchor discharging cargo off the harbour of Dominique, about 170 miles distant from St. Thomas, S.E. The harbour is on the side of the island, and sheltered from the swell produced by the trade winds; the weather calm. Just about 5 A.M. a succession of heavy rollers broke in; they lasted for half an hour, and rendered all communication with the shore during that space impossible. No shock was felt on board the *Corsica*, but Captain Herbert caused note to be taken of the marine phenomenon, not doubting that it must have been due to an earthquake, as indeed was evidently the case.

The centre of disturbance would appear to have been in this case under the sea at some distance S.E. from St. Thomas, a direction often indicated in such occurrences. On one occasion only, that of the severe shock of November 1867, did the movement seem to have been propagated from due south, its centre being in the deep soundings between the islands of St. Thomas and Ste. Croix.

During the same day two other slight shocks, one at about 10 A.M. the other at noon, were felt at St. Thomas; they were unaccompanied by noise.

W. G. PALGRAVE

St. Thomas, W. I., March 21

Physical Axioms

CONVINCED that the fulfilment of astronomic predictions can never *demonstrate* the laws of motion, and yet feeling myself quite destitute of intuitive belief in those laws, I have been led to think that in the present controversy truth may lie somewhere between the positions respectively enunciated by Mr. Spencer and his critic.

By reasoning which seems to me equally lucid, ingenious, and unanswerable, Mr. Spencer has shown that certain ultimate mechanical laws are tacitly assumed in every process of experimental verification. But I do not see that this vitiates *completely* the inference drawn from such verifications. The pure empiricists argue that because certain observed results coincide with the results of calculation, therefore the assumptions on which the calculation was based must be true. Now without doubt the *demonstrative* character of this inference vanishes entirely under Mr. Spencer's searching criticism. But it seems to me that a *high probability* remains behind. For were there any but an excessively minute error in the laws of motion, our astronomical observations could agree with the results of calculation only by a conflict of errors—a conflict which Mr. Spencer himself hints at. But there are overwhelming chances that these errors would not be so accurately adjusted throughout an immense variety of cases as exactly to compensate one another in every single instance. Hence I cannot but regard the laws of motion as hypotheses, the truth of which is shown by experiment to be overwhelmingly probable. The doctrine here assumed may be illustrated by an appeal to those old friends of probability students—the dice. If I throw double sixes ten times running I naturally conclude that the dice are loaded. This supposition almost necessarily involves the sameness of the ten throws, whereas the supposition that they were not loaded is consistent with an immense number of other results. Our minds choose the former alternative in obedience to an instinct which might with much show of propriety be formulated into an axiom. We may, however, deduce a justification for it from two ultimate intuitions of our nature—belief in uniformity of sequence and the general doctrine of chances—intuitions by which the mind apprehends respectively the ultimate law of knowledge and the ultimate law of ignorance. Belief in any special fact beyond individual experience can be rationally arrived at only by applying the former law to that knowledge which our individual experience furnishes, and the latter law to that ignorance which our individual experience has failed to enlighten.

It is the *approximate* truth of the laws of motion to which I have throughout referred. That there may be an *excessively minute* error in all physical and even all geometrical principles, Prof. Clifford has long ago shown how unphilosophical it is to deny.

F. W. FRANKLAND

Royal College of Chemistry, April 18

The Fertilisation of Fumariaceæ

Apropos of the interesting discussion on this subject which has appeared in your columns, I should much like to know whether any of your readers have observed the mode of fertilisation in *Corydalis claviculata*. Last summer I spent a considerable time in attempting to find this out, but without success. In every flower which I gathered in the mature state, I found the style broken off at the articulation immediately above the ovary, as if to prevent the possibility of fertilisation after a certain period. As the interior parts are completely concealed by the corolla, it was difficult to determine whether the separation had actually taken place on the flower, or was the result of the dissection, but I believe the former to be the case. In a large number of observations, extending over a considerable time, I never saw an insect visit the plant (this was in Westmoreland), though seeds were freely produced. Muller does not mention this species in his classical work on the subject, "Die Befruchtung der Blumen durch Insekten."

ALFRED W. BENNETT

ALLOW me to bring before the notice of readers of NATURE a small point bearing on the fact of the bright hue presented, after fertilisation, by the flowers of *Fumaria capreolata*.

Is it not possible that the pale colour may be more attractive to the fertilising insects than a brighter one would be? May not the drawing-principle be the result of correlation between the art-manifestations of the attracting and the æsthetic susceptibilities of the attracted organism, and not depend solely on gaudiness of the flower? If this be so, we know that these susceptibilities have, at any rate sometimes, a very limited range, as is seen in the bee-orchis, where the similarity of the labellum to the body of a bee is very close, both in colour and in form, and cannot be useless, seeing that a great amount of developmental force is expended in its production. On this view also the rejection of highly-coloured poisonous caterpillars may in part be referred to the non-agreement of their hues with the orthodox colour-notions of birds. On the other hand, if mere gaudiness is aimed at, why should there be such diversity exhibited? why would not one colour answer the purpose in every instance?

The present case is capable of ready explanation on the supposition that it comes under the influence of natural selection; for, as Mr. Spencer has shown, the hue of the flower results from a diminished amount of nutritive material supplied to the coloured parts, so that the least vigorous individuals would have these most highly coloured at the time of fertilisation. But since the pale flowers are preferred by the insects, they would stand a better chance of being fertilised than would the bright ones, so that a process of selection would be set up resulting ultimately in the disappearance of the latter.

If it be established that cross fertilisation is not the rule with the flowers of this fumitory, of course it is a fact which has nothing whatever to do with the present argument, and the explanation given by Messrs. Darwin and Muller is entirely satisfactory. I cannot but think, however, that special attention will bring to light many cases of cross-fertilised flowers becoming more highly coloured after fertilisation, the phenomenon being explained simply as a decomposition-phase in the life-history of the contents of the cells composing the coloured organs.

S. MOORE

I VENTURE to suggest the following as possibly an explanation of the fact observed by Mr. Traherne Moggridge, that the flowers of *Fumaria pallidiflora* attain their brightest colouring when the time for their fertilisation has past.

In plants with a racemose inflorescence the individual flowers do not open simultaneously, but more or less in succession. The flowers lowest in the raceme open first: by the time they have in *Fumaria pallidiflora* attained their brighter colour, those a little higher up on the rachis are just at the stage for fertilisation, and the former may serve to attract insects to the latter, just as in some plants (*e.g.* Poinsettia) we may presume that the highly-coloured bracts attract insects to the comparatively inconspicuous flowers which they surround. The flowers a little way up the raceme would serve in their turn to attract insects to those above them; and these again to those still higher; the process going on for a considerable time in Fumaria, as it is quite common for the pedicels in the lower part of a raceme to be bearing fruit that has attained its full size, while at the top there are flower-buds still unopen.

Quisqualis indica affords another instance of flowers assuming a more intense colour after fertilisation. Its flowers grow in short spikes; on first opening and during fertilisation, are white, very faintly tinged with pink; but subsequently turn a light reddish-orange, and finally a purplish-red.

T. COMBER

Newton-le-Willows, April 7

Power of Memory in Bees

ILLUSTRATIONS drawn from experiments or observations made upon animals lower than ourselves in the scale of life must always possess great interest. That impressions received by us in early life are more permanent than those made in after years, and that the memory of the old is less retentive in the reception of new impressions than is that of children, are circumstances universally acknowledged. On October 29, 1873, I removed a hive of bees in my garden, after it was quite dark, for a distance of 12 yards from the place in which it had stood for several months; and between its original situation and the new one there was a bushy evergreen tree, so that all sight of its former place was obstructed to a person looking from the new situation of the hive.

Notwithstanding this change, the bees, every day, flew to the locality where they formerly lived, and continued flying around the site of what had been their home, until, as night came on, they many of them sank upon the grass exhausted and chilled by the cold. Numbers, however, returned alive to their new position, after having looked in vain for their hive in its old place. At night I picked the exhausted bees up, and, having restored warmth to them (by leaving them for a time upon my coat-sleeve), I returned them to their companions.

Here was an illustration that the faculty of *memory* was superior to that of *observation*; but that was not all. Nearly every bee which I picked up during the twenty-three days through which this effort of memory lasted was an *old one;* as was easily deduced from observing the worn edges of the wings: showing that, whilst the young insects were quick in receiving new impressions, and in correcting errors, the nervous system of the old bees continued *acting in the direction which early habit had affected.* So true is it that "One touch of *Nature* makes the whole world kin."

Marlborough House, Torquay JOHN TOPHAM

Pollen-grains in the Air

WILL you allow me to ask Mr. Hubert Airy, in reference to his interesting paper on the "Microscopic Examination of Air," in NATURE, vol. ix. p. 439, on what ground he refers the "triangular pollen" captured on his slide to the birch and hazel? Observations of my own have led me to the conclusion that the pollen of plants which depend exclusively on the wind for their fertilisation is perfectly spherical, at all events before the form of the grain is disturbed by the emission of the pollen-tubes, and this indeed one might expect from *à priori* considerations. Among the pollen-grains I have especially observed, are those of *Corylus avellana*, *Betula alba*, and *Populus balsamifera*. I shall be much obliged if any of your readers could refer me to any accurate published description of the form of pollen-grains beyond those contained in Fritzsche's "Beitrage zur Kenntniss des Pollen." ALFRED W. BENNETT

6, Park Village East, N.W.

Lakes with two Outfalls

I AM a little surprised to find, by the recent letters in your paper, that Science makes so wide a mouth over this phenomenon, though its exceptional character, and the general correctness of Colonel Greenwood's theory, must readily be recognised. My surprise is occasioned by the fact that Norway, which is now visited by thousands of educated English tourists every year, can supply, not one, but several, I had almost written many, apparent examples of this double outflow. I have not myself passed the watershed at the Lesjeskaagen Lake, though I was close to it in August last, and would have examined it if I had known its importance; but I know enough of the locality to think that Colonel Greenwood is probably right in his explanation of it. But there is another, which I have passed, and which is situated on perhaps the most frequented route in Norway, viz. that from Lœrdalsören over the Fille Fjeld, to which I hardly think the same explanation would apply. Between Nystuen and Skogstad is a chain of lakes crossing the watershed, the highest of which (not the one marked on the Vei-cart over Norge, I think) sends its waters to the west, past Nystuen to the Sogne Fjord, at Lœrdalsoren, and on the east by the Lille Mjosen, and Aadalen to the Tyrifjord, and so past Drammen to the Christiania Fjord. This lake is a small one, and the double outflow is close to the high road. I cannot imagine any commercial object for an artificial cut, and it must be well known to hundreds who annually pass it. The Vei-cart shows several other instances, I know not how authentic, though I have always found it fairly accurate, erring rather by omission than commission. But in lat. 62° N., long. 24° 40′ E. or thereabouts, is a very remarkable watershed, having a complication of outfalls; the Bredals-Vand sending one to the N.W. to the Geiranger Fjord, and a second to the Vaage-Vand and Gudbrandsdalen; which is also joined by a draft from a lake to the S.W., which likewise sends a feeder to the Opstryen Vand, and so W.S.W. to the Nord Fjord. This I have not myself seen, but I was at Merok on the Geiranger for some days last August, and was assured by my landlord that the map was correct in this particular. As the Norwegian peasantry are well-educated, intelligent, and truthful, and this route forms their regular short cut to Christiania, I cannot doubt but that it is the fact. However, I have engaged to go over the track this summer with Captain Dahl, the well-known jolly commander of the *Erkno*, and I will take care to ascertain the truth and report the result. If, moreover, there are any geological or geographical points to be attended to, and Colonel Greenwood will kindly furnish me with instructions, I shall be happy to attend to them.

I have a strong recollection of having passed two or three cases of double outfall on a small scale in my wanderings; but not having been aware of the importance of the point, I did not take notice sufficiently precise to enable me now to put my finger on them with certainty, but my general conviction is strong, that Norway can furnish several, if not many examples, which are the more significant from the fact that it is one of the oldest countries in the world. W. B. THELWALL

Burghley Road

WILL you permit me to correct a mistake as to a matter of fact in NATURE, vol. ix. p. 441. Loch-na-Davie, Arran, has two outlets, as is correctly represented in the Ordnance Map, and also in that in Bryce's "Geology of Arran." In August 1872 I walked up the north stream from Loch Ranza to its outfall from Loch-na-Davie. I think Colonel Greenwood ought at least to have made himself acquainted with the Ordnance Map.

Edinburgh A. CRAIG CHRISTIE

THE "CHALLENGER" EXPEDITION *

IV.

TRISTAN D'ACUNHA

AMONGST the places in the Atlantic marked out by the Circumnavigation Committee as being of especial interest, the small island of Trinidad is noted with those whose vegetation is absolutely unknown, or all but so. From this fact Trinidad became a point of attraction which Mr. Moseley was most anxious to reach. Owing, however, to unfavourable winds and other causes, as well as to a desire of those in command of the ship to proceed south, the visit to this little island was abandoned, with the hope of calling there on the return voyage. After a narrow escape, also, of missing Tristan d'Acunha, the vessel anchored on the north side of the island, and the morning was spent in searching the low lands under the cliffs, 500 feet being the greatest height that was attained during the stay. On this side the island rises in a range of perpendicular cliffs of black volcanic rock, in appearance somewhat similar in structure to that exposed in section in the Grande Cural in Madeira. At the base of the cliffs here are *débris* slopes, and a narrow strip of low shore land of an irregular rocky and sandy nature. The settlement lies on a stretch of low land, broader and more even, and extends westward. The ascent to the plateau above the cliffs is comparatively easy, owing to the deep gullies by which the cliffs are broken.

Though the extent of the island is small, its actual area being not more than 16 square miles, the botanising was confined to the irregular strip of shore land just alluded to, and to the gully immediately above the settlement. Further exploration would have been made, but a sudden squall coming on, the recall was hoisted from the ship, and the party had to leave the island, after a visit of only six hours. Grasses, sedges, mosses, and ferns grow on the cliffs, and occasional patches of *Phylica arborea* Th., a rhamnaceous tree peculiar to the islands, as well as a species of *Empetrum*; these plants, however, are more prominent towards the summit. At the foot of the water-courses under the cliffs are bright green patches of *Rumex frutescens* Th. Mosses and liverworts cover the lower part of the cliffs, and the latter also abound beneath the grass in some situations to such an extent, indeed, as to cover the earth as with a green sheet. *Spartina arundinacea* Carm., grows in rounded tufts amongst the other herbage, and in the clefts of the rocks was seen in abundance *Asplenium obtusatum* Forst., and *Lomaria alpina* Spreng. It is remarkable that the plants of *Lomaria* when found in stony places, and in a comparatively starved condition, were mostly furnished with fertile fronds, whilst those growing in rich vegetable mould were barren. Amongst flowering plants the most common were *Apium australe* Th., *Pelargonium australe* Jacq., *Sonchus oleraceus* L. our common annual sow-thistle, *Hypochæris glabra* L. a closely allied plant to the sow-thistle, and also found in many parts of England. A cinchonaceous plant, *Nertera depressa* Banks, was very abundant, and

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* These Notes are founded on letters sent home by Mr. H. N. Mosely. Continued from p 451.

Oxalis corniculata L., with its yellow flowers, was likewise seen, but not in any quantity.

An interesting plant—*Chenopodium tomentosum* Th.—grows abundantly on Tristan as well as on Inaccessible Island; it is known as the tea plant, and the leaves, which are strongly scented, are used for making a decoction which is drunk with milk and sugar.

In the gully above the settlement, shrubs of *Phylica arborea* commence at an elevation of about 400 ft. No trees are found in this locality, having all been cut down at different times for fire-wood, but on other parts of the island there is abundance of wood. The diameter of the trunks of the trees on the upper plateau, it is said, reach to 18 in. On some fresh-water ponds close to the sea was a quantity of confervæ, but no chara was seen, a species of *Isolepis* also grew on the edges of these ponds which was not seen on the other two islands. A few willow bushes grew in a sheltered situation in a ditch near the cottages, and seemed to be thriving. Growing round the island is a belt of *Macrocystis pyrifera* Ag., a gigantic sea-weed, abounding in the southern temperate zone, and stretching up from thence along the Pacific to the Arctic regions. It occurs in immense lengths, single plants of from 100 to 200 ft. being common, and it is said that they are sometimes seen from 700 to 1,000 ft. in length, forming cable-like masses nearly as thick as a man's body, and having the appearance of huge buoys.

The surf on the rocky coast of Tristan is so heavy that the more delicate sea-weeds stand no chance, but are dashed and torn into numerous pieces.

The temperature of the fresh-water ponds at the sea-level gave a result of 54° F. while the water of the streams running down the cliffs stood at 50°, the difference being due evidently to the influence of the snow-water from above.

FUNERAL OF THE LATE DR. LIVINGSTONE

On Saturday last the remains of David Livingstone, which left Central Africa now nearly a year ago, were interred in Westminster Abbey, in presence of a multitude such as was probably never collected therein on any similar occasion. The funeral procession, which started from the Geographical Society's Rooms, Savile Row, was of great length, though of the plainest description possible under the circumstances; we have not learned whether this was in accordance with the wishes of the late traveller's relatives, or whether it arose from scarcity of funds. Every mark of respect was shown to the procession along its route, and at several advantageous points considerable crowds had collected to witness the last journey of the great explorer.

Men of all ranks and of all pursuits in life formed part of the procession, and stood around the grave during the service in the Abbey. The patriarchal Dr. Moffat, Livingstone's father-in-law, and the traveller's two sons, Thomas and Oswell Livingstone, Mr. James Vavasseur, Sir F. Steele, Dr. Kirk, Mr. W. F. Webb, the Rev. Horace Waller, Mr. H. M. Stanley, Mr. E. Young, Sir W. Fergusson, the Duke of Sutherland, Sir Bartle Frere (President of the Royal Geographical Society), Sir H. C. Rawlinson, Vice-Admiral Baron de la Roncière le Noury (President of the French Geographical Society), Dr. Hooker (President of the Royal Society), Mr. C. R. Markham, Mr. R. H. Major, Mr. H. W. Bates, Dr. Houghton, Mr. J. Young of Kelly, are the names of some of those who followed the body to the grave; there were besides, deputations from Edinburgh, Glasgow, and other places, and the carriages of Her Majesty the Queen, the Prince of Wales, and of many other noble and distinguished persons formed part of the procession. Among those who were waiting inside the Abbey were men of every shade of thought, political and religious, men distinguished in every walk of life, deputations from many religious bodies, from the establishment outwards, and representatives of various scientific Societies. The bearing of the crowds both outside and inside the Abbey showed that they were brought together from genuine admiration and sincere respect for the memory of the simple-minded hero.

We think the character of the assemblage which gathered to do honour to Livingstone's remains is one proof that he has done a work calculated to call forth the admiration and gratitude of those whose suffrages constitute fame of the highest and most enduring kind. If to conceive a great and noble purpose and to carry it out even unto death, with indomitable energy, determination, and the greatest skill, in the face of every possible discouragement, discomfort, and obstacle, be a mark of greatness, his contemporaries have certainly made no mistake in raising David Livingstone to the lofty pedestal which he at present occupies. He has probably added more largely to the sum of exact geographical knowledge than any other explorer has hitherto done. As Dean Stanley eloquently said in his funeral sermon on Sunday afternoon:—"By his indomitable resolution we have now revealed to us, for the first time, that vast tract of Central Africa which, to the contemplation of the geographer, has been literally transformed from a howling wilderness into the glory of Lebanon. The blank of unexplored regions which in every earlier map formed the heart of Africa is now disclosed to us adorned with those magnificent forests, that chain of lakes 'glittering'—to use the native expression—'like stars in the desert;' those falls more splendid, we are told, even than Niagara, which no eye of civilised man had ever before beheld. And to his untiring exertions, continued down to the very last efforts of exhausted nature, we owe the gradual limitation of the basin within which must at last be found those hidden fountains that have lured on traveller after traveller, and have hitherto baffled them all."

A deputation of gentlemen interested in the family of the late Dr. Livingstone waited on Monday upon the Chancellor of the Exchequer and the Secretary of State for Foreign Affairs, for the purpose of representing to Her Majesty's Government the very general anxiety that was felt throughout the United Kingdom that some substantial recognition, in the shape of an adequate provision for his family, should be made of the services of the great traveller. A requisition to the Prime Minister, asking him to confer a pension on the family of Dr. Livingstone, was on Monday night circulated among members of Parliament at the House of Commons. A large number of signatures has already been attached by gentlemen on both sides.

About three years ago, Her Majesty, at the recommendation of Mr. Gladstone, conferred a pension of 300*l.* a year upon Dr. Livingstone, who, however, it is sad to think, never knew that his services had been so recognised by the Government. Upon the death of Livingstone the pension ceased, but it was deemed by Mr. Gladstone a matter of sheer merit, due to the great explorer, to confer some pecuniary benefit upon his children, and the figures on the civil list were thereupon reduced from 300*l.* to 200*l.*, which is actually the amount that will henceforth be paid by the Government to those he has left behind him. Though Dr. Livingstone made a large sum of money out of the first book he published, still he disbursed more than half that amount in his promotion of the exploration of the Zambesi.

Livingstone's devotion to the cause of science and of philanthropy has thus been the means of leaving his family very inadequately provided for; but as he has added so greatly to the glory of his native land, and as he spent his life in the service of civilisation, we feel confident that those for whom he was therefore unable to provide will be well cared for.

From a letter in yesterday's *Times* we see that the Diary kept by Jacob Wainwright for nine months after Livingstone's death will shortly be published.

THE COMING TRANSIT OF VENUS *

II.

THERE is perhaps no problem which has been so constant a source of interest to the learned in all ages as the solving of the mystery of the solar system. The labours of Copernicus, Tycho Brahé, Kepler, and Newton have given us a general knowledge of the nature of the planetary motions; and the investigations of later mathematicians have enabled us to predict, with wonderful accuracy, the future positions of the planets. But the dimensions of the solar system are not known with the same precision.

It is true that we know the *relative* distances of all the planets from the sun with tolerable exactness. This problem has been attacked in two totally different methods. The first is by measuring directly the changes that are produced in the motions of the planets when the earth has moved through a certain portion of its orbit. In the case of the planets Mercury and Venus, which move in smaller orbits than that of the earth, the direct observation can easily be made. For let us suppose VV′ and EE′ (Fig. 8) to be the orbits of Venus and the earth, and S to be the sun. Let us watch the position of Venus night after night until she is as far away from the sun as possible. If we measure her apparent distance from the sun by astronomical means, we shall know that the sun, Venus, and the earth occupy positions such as S, V, and E; the directions ES and EV being known from our observations. By measuring off the distances SV and SE on the diagram, we actually find the relation between the earth's distance from the sun and that of Venus. The same can be done with Mercury; but for the superior planets the direct mode of observation is more difficult.

FIG. 8

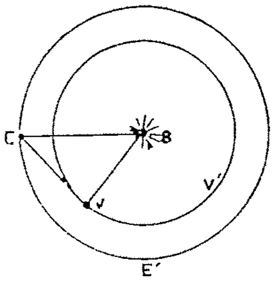
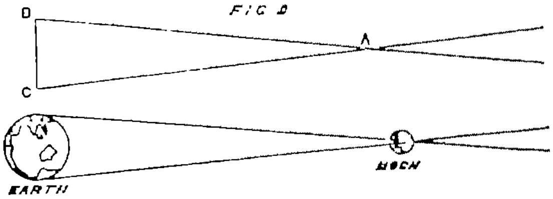


FIG. 9

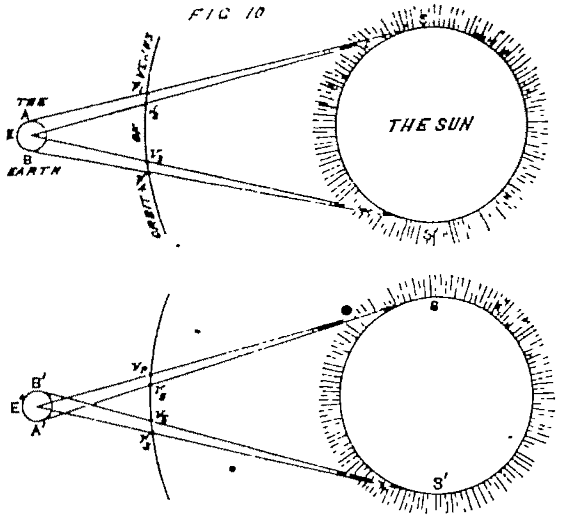


But there is an indirect method which is much more easy to apply. Kepler's three laws have been shown to be necessary consequences of Newton's theory of gravitation. Now Kepler's third law tells us how to find the relative distances of two planets from the sun when we know the relation between their periods of revolution. The exact law is this:—Multiply the number of years taken by a planet to go round the sun, by the same number. This gives us a first number. Then find a second number which, multiplied by itself twice, gives us the first number; this second number is the distance of the planet from the sun (the earth's distance being called 1). To take an example: Jupiter takes about 11 years to go round the sun; 11 multiplied by 11 gives us a first number, 121. Now if 5 be multiplied by 5 we get 25, and if this be again multiplied by 5 we get 125, which is almost the same as the first number, 121. Hence we are right in saying that Jupiter is about five times as far from the sun as the earth. If we had used the exact number of years we should have got the exact distance. Now it is very easy to find the period of revolution of a planet. For we can easily measure the interval between two dates when Jupiter and the earth, for example, are in the same line with the sun; in other words, we can measure the "synodical revolution" of Jupiter; and from this it is easy to calculate the time of Jupiter's revolution round the sun.

By applying these methods to all the planets we can lay down their orbits upon a plan; *all we wish now is to find the scale upon which our plan is drawn.* If we knew the distance of the earth from the sun, or if we knew the distance between any two of the planetary orbits, we should know the scale upon which our plan is laid down. Various methods have been adopted for this, but the one which makes use of a transit of Venus has generally been considered to be the most accurate.

One method which has successfully been applied to measuring the moon's distance is that used by surveyors. The surveyor chooses two spots, B, C, whose distance he measures. Suppose it to be one mile. He draws this distance, say, to one inch on a sheet of paper. He then takes a telescope, mounted so as to enable him to measure any angle through which it is turned. He places the telescope at B, pointing towards C. He then turns it till it points at the distant object, and finds what the angle of B is. He then draws the line BA upon the paper, and he knows that the distant object lies somewhere on the line BA. He then does the same with C, and thus he knows that the remote object lies on CA. But A is the only point lying both on BA and CA; hence A corresponds to the distant object. If on measuring CA he finds it to be 30 inches, then since CB, which is 1 inch, means one mile; CA, which is 30 inches, means 30 miles, and this is what he wanted to find out.

FIG. 10



If, instead of taking a base-line (as it is called) of one mile, the diameter of the earth, or 8,000 miles, be taken; then, if the moon be the distant object, we can determine its distance in almost the same way. It is in this manner that the moon's distance has been measured. It is easy to see that if the angle at A (Fig. 9) were very small, a slight error in measuring either of the angles B or C would make a great difference in the distance deduced for the remote object. Hence, if the moon's parallax were very small, this method would be unsuitable. But the parallax of the sun is very small, and hence we cannot find the sun's distance with any exactness by this method.

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* Continued from p. 449.

But if any one of the planets ever came so close to the earth as to make its parallax tolerably large, then we could determine the scale upon which the solar system is built up. Now Venus and Mars are two planets which at certain times come closer to the earth than any other planet. But, unfortunately, when Venus is most near to the earth she is generally invisible, because the whole of her illuminated side is turned away from us. Mars, however, is a planet that gives us a very favourable opportunity for determining its distance. The advantage is increased by this peculiarity, that every fifteen years Mars is at its shortest distance from the sun, at the same time that the earth is at its greatest distance, the two planets being also in the same line with the sun, so that they are closer than we might have thought possible. In fact, on these occasions Mars is nearer to the earth by $\frac{1}{7}$th part than she is if the conjunction take place when both the earth and Mars are at about their mean distances from the sun. Suppose then that under such circumstances two observers, one at Greenwich and the other at the Cape of Good Hope (where there is a fine observatory), observe the position of Mars as compared with that of a star at the same time. The position of Mars will be displaced by parallax; and by comparing the apparent distance of the planet from the fixed star at these two places we can find the sum of the parallaxes in these cases. Hence we can find the distance of Mars, as already explained.

This was the first method to give a value of the solar parallax with anything like accuracy. At the suggestion of Cassini, the French sent out an expedition to the Cape, under the astronomer Picard. The value obtained for the sun's parallax was 9″·5. Prof. Henderson in 1836, and Mr. Stone, in 1862, utilised this method. Another opportunity will occur in 1878.

Before proceeding to the method of the Transits of Venus, it will be well briefly to allude to some other methods by means of which the solar parallax, or the sun's distance, has been estimated.

It has been found that light takes a sensible time to propagate itself through space. Hence, when one of Jupiter's satellites passes into the shadow of the planet, this fact is not communicated to our vision for something like 38 minutes, the time taken by light to pass from Jupiter to the earth. Now, when we are on the same side of the sun as Jupiter, this distance is shorter by the whole diameter of the earth's orbit than when we are at the opposite side of the sun. Hence, in the former case, the eclipses will seem to take place sooner than the predicted time, and in the latter case later. The difference in either case is about 8 minutes, and as we know that light travels over 298,500 kilometres per second,* this tells us that our distance from the sun is about 91,000,000 miles.

But our knowledge of the velocity of light has been utilised in another manner to solve the same problem. You see that if we know the earth's velocity in miles, we can find its distance from the sun. For if it goes 1½ million miles in one day, it must go over 365 times that in a year, and *that measures in miles the circumference of our earth's orbit*, and hence we can get our distance from the sun. How then are we to find the velocity of the earth in miles. This depends on a curious property of light. In a steady down-pour of rain you hold your umbrella upright if you are standing still, but incline it forward if you are walking fast. This is to make the umbrella catch the rain-drops. The amount of inclination you give it depends upon the rate at which you are walking compared with the velocity with which the drops fall. The same thing happens with light. We have to incline our telescopes forward a little in the direction in which the earth is moving to catch the rays of light; and at opposite seasons of the year the earth is moving in contrary directions, and the telescope has to be pointed in sensibly different directions. The inclination that a telescope receives is known, and the velocity of light being known, we can find the velocity of the earth, and hence, as I have shown, the distance of the earth from the sun.

There is another method of peculiar interest depending upon the motions of the moon. The law of gravitation says that the attraction of each body for each other one depends upon the distance between them. The moon is attracted to the earth by a force, depending upon the distance of the moon, which is known in miles. But the moon is caused to deviate from its natural course on account of the sun's attraction. This depends upon the distance of the sun from the earth, and if this be not known exactly in miles we shall see that it is impossible to apply calculation to foretell the motions of the moon; for, if upon any scale we attempt to lay down upon paper the relative positions of the sun, earth, and moon, we shall place the moon at its proper distance, and the sun, though in its proper direction, will not be placed at the proper distance, and we shall not know the direction in which it attracts the moon, nor the magnitude of this attraction, and we shall make our calculation wrongly, and the moon's observed place will differ considerably from its calculated place.

Such a difference was actually detected by the illustrious Hansen, whose tables of the moon are the best we possess. Hansen saw that this must be due to a wrong assumption as to the distance of the sun, and communicated his doubts to the Astronomer Royal* in the year 1854. This led to a re-discussion of our knowledge of the subject which has confirmed Hansen's views, and which leads us to see the importance of knowing accurately the sun's distance, if we wish ever to have our tables of the moon so accurate that we may determine the longitude by their aid. This method for investigating the solar parallax was first used by Laplace.†

More recently, M. le Verrier has suggested a new method that promises in time to be the best.‡ In the lunar theory, an equation appears connecting the relative masses of the earth and sun with the solar parallax, so that if we know the one we can find the other; and from a peculiarity in the equations, a small error in determining the relative masses will affect only very slightly the deduced parallax. Le Verrier finds the ratio of the masses of the earth and sun by determining the effect of the earth's attraction upon Venus and Mars. This being applied to the lunar theory, a value of the solar parallax is obtained.

The method, however, which has found most favour up to the present time, is the employing of transits of Venus to measure the sun's distance. When a transit of Venus occurs, the first evidence of the phenomenon is given by a slight notch being made in the contour of the sun's edge at a certain spot. This notch increases until the full form of the planet is seen. The first appearance of a notch is called the time of first external contact. But when the planet appears to be wholly on the sun, her black figure is still connected with the sun's limb by a sort of black ligament, of which we shall say more hereafter. When the whole of the planet is just inside the sun's edge, the time of first internal contact has arrived. The breaking of the ligament is a very definite occurrence, and was, until lately, taken to indicate the true moment of internal contact. The second internal and external contacts take place as the planet leaves the sun.

In 1663, the celebrated James Gregory, in his famous work the "Optica Promota," *prop*. 87, *Scholium*, alludes to the possibility of determining the sun's parallax by means of the transit of an inferior planet. He has been showing methods of finding the parallax of a planet by comparison of observations made at different parts of the earth upon the position of the planet compared with that of a star. He then takes, in place of a fixed star, another planet, the two being in one line, as seen from the earth. The application of this to the case of Mercury or Venus and the sun, was obvious.[*]

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* As determined by Foucault, *Comptes Rendus de l'Acad. des Sciences*, vol. lv., p. 501; also by Cornu, *Comptes Rendus*, Feb. 10, 1873.

* *Monthly Notices, R.A.S.*, vol. xv., Nov. 1854.

† *Système du Monde*, t. ii. p. 91.

‡ *Comptes Rendus*, July 22, 1872.

But Halley was the first to see clearly what a powerful means of determining the sun's parallax an observation of contact really is. So far as I can discover, he first mentions the method in a letter to Sir Jonas Moore, written at St. Helena in 1677,[*] just after having seen a transit of Mercury. The exactness with which he believed the time of contact to be determinable, led him frequently afterwards to urge his countrymen to make every effort to utilise the method on the occasion of the transits of 1761 and 1769, when he should be dead.[†] And thus, in addition to his celebrated prediction of a comet, he left a second legacy to his successors, who, as Englishmen, might be entitled to be proud of his foresight though he could not live to reap the glory of it.

It is a matter of some difficulty to show, in an elementary manner, the way in which the value of the sun's parallax can be found from observations of contact. We will try, however, to put it in a light which anyone, with a little attention, will understand.

1. It must be thoroughly understood, from what has already been said, that if we know the amount of the sun's parallax we know its distance. In other words, if we know the angle subtended by any known distance on the earth's surface at the distance of the sun.

2. We know that the relative positions of the earth, Venus, and the sun, are given by supposing the earth to go round the sun in 365 days, and Venus in 224 days. Or, if we please, we may take no account of the earth's revolution, but suppose it fixed, in which case the revolution of Venus *relatively* to the earth (*i.e.* the synodical revolution) is 584 days.

3. If, then, Venus moves round the sun through 360° relatively to the earth in 584 days, she moves through $\frac{1}{584}$ of that in one day, and through $\frac{360}{584 \times 24}$ of a degree in one hour; which is at the rate of about $1\frac{1}{5}$ seconds of arc in a minute of time.

Now we are ready to understand Halley's reasoning.

Let A (Fig. 10) be the position of an observer on the earth at the time of 1st internal contact. S is the sun, and V_1 is now the position of Venus. This observer sees the contact earlier than a hypothetical observer at the earth's centre would see it, by the time Venus takes to move over V_1V_2. If we knew by calculation the instant when an observer at E would see it, and the observer at A saw it 8 minutes sooner, then, since Venus moves over $1\frac{1}{5}''$ in a minute, she has moved over $8 \times 1\frac{1}{5}$ or $9\frac{3}{5}''$ of arc in this time, and hence we learn that the angle A S E $= 9\frac{3}{5}''$.

Suppose that by the time of the last contact the point A on the earth's surface has been carried by her rotation to B: the time of the last contact will now be too late by 8′; since the whole duration of the transit as seen by this observer is 16′ too long, and the angle moved over by Venus in 16′ is the sum of the sun's parallax as seen from A and from B.

But we cannot calculate with absolute accuracy the duration a transit would have when seen from E, because we should require to know more accurately than we do the values of Venus' and the sun's diameters.

Halley got rid of this by taking another station which should be in the position A at the beginning of the transit. In the case we have been considering the time of the first contact would here be too late by 8 minutes; and if this place had reached B′ by the end of the transit, the time of contact would be too soon by 8 minutes. Hence in this case the whole duration would be shortened by 16 minutes; but in the former case it was lengthened by 16 minutes. Hence 32 minutes is the time taken by Venus to pass over an angle equal to the sum of the parallaxes in the four cases considered. This difference of duration, whether it be 32 minutes or anything else, is a quantity which can be observed. Now Venus moves over about $1\frac{1}{5}''$ of arc in a minute, or $38\frac{2}{5}''$ in these 32 minutes. Hence one-fourth of $38\frac{2}{5}''$ or $9\frac{3}{5}''$ would appear, from the above hypothetical observation, to be the value of Venus's parallax.

It must be noticed that we have here supposed that the transit takes exactly twelve hours, whereas the longest transit cannot exceed 8 hours. We have also supposed that two stations had been selected which were exactly situated so as to bring out the full effect of parallax at the time of each observation. These suppositions have been introduced only to simplify the understanding of the method. Anyone who has followed the above explanation will see how the method may be applied to actual cases that may occur.

Halley saw (what many people fail to see even now) that the great accuracy of the method consists in this, that in one second of time Venus moves over about $0''\cdot 02$; and if we can determine the time of contact, with an error of no more than a second, we are measuring the sun's parallax with an error of no more than ·02 of a second of arc.

Halley even pointed out the best stations for observation. We may consider the earth to be at rest if we suppose Venus to move with the velocity she has relative to the earth. He supposed that the planet would cross near the sun's centre, and that the transit would occupy about eight hours. An observer in India would see the commencement of the transit four hours before mid-day, and the end of the transit four hours after mid-day. But, in the meantime, the part of the earth where he is has been moving from west to east, and Venus has moved from east to west, hence the duration of transit will have been shortened. But at Hudson's Bay the transit begins just before sunset and ends just after sunrise, that part of the earth having moved in mean time from east to west so as to lengthen the transit; and thus at one place the duration of transit is lengthened, and at the other shortened, and the difference of time depends upon the parallaxes of Venus and the sun[*] at the two stations, and after finding these parallaxes we can calculate the equatorial horizontal parallax.

GEORGE FORBES

(*To be continued.*)

THE LECTURES AT THE ZOOLOGICAL SOCIETY'S GARDENS

I.

ON Tuesday, April 14, Mr. P. L. Sclater, F.R.S., gave the Introductory of the twelve lectures which are to be continued during the spring. His remarks on that occasion were chiefly confined to the subject of Zoological Gardens in general. After an interesting account of the most important continental gardens, including those of Paris, Amsterdam, Antwerp, Berlin, and Hamburg, he went on to speak of the different animals which thrive best in captivity, taking each order of each of the great classes of the vertebrata separately, and pointing out that whilst some, as the Carnivora, thrive well in confinement, others, as the Insectivora, can hardly be kept in a menagerie at all.

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[*] Hooke's "Lectures and Collections," 1678.

[†] "Catalogus Stellarum Australium;" also "Phil. Trans.," 1694 and 1715.

[*] This lengthening or shortening of the time of transit will be rendered more evident by an analogy. A person standing still sees a carriage pass between him and a distant house. The carriage will take a certain time to pass the house. But if he be also moving, and in the same direction with the carriage, the transit of the carriage will take longer; but if he move in the opposite direction to the carriage, the transit will take a shorter time. If, then, two persons be seated at opposite sides of a merry-go-round, so that at the time the carriage seems to be passing the distant house, one observer is moving with the carriage and the other in the opposite direction; then one observer will see the time lengthened, and the other shortened. Now, the world is such a merry-go-round, and the positions of these two people correspond to the positions of India and Hudson's Bay, as pointed out by Halley.

On the following Friday Mr. Sclater commenced the first of four lectures On the Geographical Distribution of Mammalia. A fauna constituting the animals inhabiting a country, and a flora its plants, the lecturer went on to illustrate the fundamental law that the animals and plants found in far distant countries are usually different, and that those of near countries closely resemble one another. We find the animals in France much like those in England, those in Ceylon much less so, and those in Australia as different as possible. It might at first sight be thought that difference of climate caused the differences that are observed in geographical distribution, but that such is not the case is proved without difficulty by taking different countries in the same latitude and with a similar climate and comparing them. For instance, on and near the equator we have Borneo, part of Africa, and the country bordering the Amazons; nothing can be more different than their faunas, and yet they are similarly circumstanced, so far as temperature and climate are concerned. So the polar seas of the northern and southern hemispheres are very different as regards their animals, although nearly identical in climate. The auks and seals of the one are replaced by the sea lions and penguins of the other. The faunas of the Himalayas and of the Andes, mountains both in hot countries, are very different also.

The meaning of the terms "specific area" and "generic area" was then explained. A species, the aggregate of similar individuals, has an habitat or area of distribution which is definitely circumscribed. In some animals this area is large, as in the case of the lion; in others, as in the case of the aye-aye of Madagascar, it is extremely limited. Among birds this limitation, strange as it may appear, is sometimes extreme; on each of the two nearly adjoining mountains of Pichincha and Chimborazo there are species of humming-birds found, which occur nowhere else. The area which includes all the areas of the species of a genus forms a generic area. These areas are continuous, or were so at one time; physical changes having sometimes intervened to produce an apparent interval.

From these observations it is evident that the locality in which an animal is found is as important a fact in estimating its individuality as are its internal structure and general configuration. This point is frequently but too little taken into account.

The lecturer, having said thus much on the general subject, proceeded to show how the class of Mammals was to be distinguished from the other classes of Vertebrates, and stated that for geographical purposes the mammalia, or those animals which suckle their young, might be most conveniently divided into terrestrial and aquatic. Our knowledge respecting the former of these sections is, as might be imagined, much greater than of the latter; nevertheless, within the last few years the aquatic mammalia have received considerable attention, and have become much better known.

(*To be continued.*)

NOTES

THE magnificent bequest of 10,000*l.* has been made by the late Mr. E. R. Langworthy to the Owens College, Manchester, for the purpose of developing the chair of Experimental Physics. A splendid opportunity is thus afforded to the Professor of Physics in Owens College not only to advance original research in connection with that subject, but also of teaching the students of his class in the only effectual way by which physics can be taught. Physics, in short, can now be placed on the same footing in that University as chemistry. The terms in which the bequest is made are so forcible and clear that they deserve to be quoted here:—"I bequeath to the trustees of the Owens College ten thousand pounds, and I desire that the same may be applied by them as they may think best in order to establish in connection with that institution a professorship of Experimental Physics. It being my wish that students may be instructed in the method of experiment and research, and that Science may be advanced by original investigation. And I also desire that the professor from time to time appointed may be selected on account of his knowledge having been especially obtained by original investigation, and that his appointment shall be contingent upon the continuance of such investigation. And I declare that the above desire shall not be construed as a trust and bind the trustees to establish a professorship; but in case it shall be deemed advisable such money may be applied in such other way as the trustees for the time being may think fit, provided such money is only used for the purpose of promoting Science." The late Mr. Langworthy deserves credit not only for his liberality, but for the sound and advanced views he held as to how Science should be taught, and as to the necessity of encouraging original research in connection with the chairs of Science in our Universities. Mr. Langworthy has also bequeathed 10,000*l.* each to the Salford Library and Museum, and to the Manchester Grammar School, in the latter case for the purpose of founding twenty scholarships.

THE Chair of Chemistry in the University of Glasgow is vacant. We hope the Home Secretary in filling up the vacancy will, in the spirit which urged the late Mr. Langworthy to make the magnificent bequest above referred to, show by the appointment he makes the appreciation in which he holds original research. It is now high time that it should be distinctly understood that no man deserves to be appointed to a Chair of Science in any of our Universities unless he has shown that he has that knowledge of his subject which can only come from original investigation.

THE Professorial Chair of Physiology in University College, London, has become vacant by the resignation of Dr. Sharpey, who has held it since the year 1836.

SIGNOR L. M. D'ALBERTIS, the distinguished Italian traveller, who has lately penetrated into the mountains of New Guinea, and discovered the remarkable Bird of Paradise which bears his name (*Drepanornis albertisi*), has just returned to this country from Sydney, *viâ* San Francisco, bringing with him his large collection in every department of natural history which he formed during his expedition.

WE would call attention to the Swiney Course of Lectures on Geology which are at present being delivered by Dr. W. B. Carpenter, F.R.S., in the Middle Class School, Cowper Street, Finsbury. The course was commenced last Thursday, and will be continued on Mondays and Thursdays at 8 P.M.; there will be twelve lectures in all. We are sure that many of our London readers, on being made aware that such a course of lectures is being delivered by such an authority, will be glad to take advantage of the opportunity, especially as the lectures are free to the public.

THE first of the course of lectures at the Zoological Gardens given in pursuance of the provisions of the Davis Trust, was delivered on Tuesday the 14th, by Mr. P. L. Sclater, being an Introductory Lecture on the animals in the Gardens, of which he gave many particulars that seemed greatly to interest the audience. Last Friday Mr. Sclater gave the first of his course of four lectures On the Geographical Distribution of Mammals, in which he dealt with the general laws of the distribution of animals on the globe. Both lectures were well attended, the picture gallery being nearly full.

AT the annual election to Mathematical and Physical Science Postmasterships in Merton College, Oxford, early in October an election will be made to two Physical Science Postmasterships, each of the value of 80*l.* a year, and tenable for five years from election, provided that the person elected do not accept any appointment interfering with the full course of academical studies. There is no limit of age, but candidates, if already members of the University, must not have exceeded six Terms from Matriculation. The persons elected, if not members of the University, will be required to pass the University Examination for Responsions within a year of election. The subjects of examination will be Chemistry and Physics. There will be a practical examination in Chemistry. Candidates will have opportunities of giving evidence of a knowledge of Biology; but it must be borne in mind that in such cases the examiners will look for evidence of an acquaintance with the principles of Chemistry and Physics equal in extent to that which is required in the Preliminary Honour Examination in the Physical Science School. A paper will be set in algebra and elementary geometry, which, *cæteris paribus*, will be of weight in the election to Postmasterships. Further information may be obtained from the Tutor in Physical Science.

MR. R. HIND, writing to the *Times*, sends the positions of two telescopic comets, discovered within the last ten days. He says:—"The first was detected by Prof. Winnecke, at Strasburg, on the morning of April 12. It is a diffused nebulosity, about four minutes in diameter, somewhat extended on the side opposite the sun. Our observations during the past night give the following place:—April 21, at 3h. 22m. 9s. A.M., mean time at Twickenham—right ascension, 20h. 50m. 41·6s.; polar distance, 88° 10′ 50″; present diurnal motion about 5′ in R.A., and 1° 5′ in P.D., both decreasing. The second comet was found by M. Coggia at Marseilles, on April 17. It is much smaller than the above, but has a strong nuclear condensation. Last evening its observed position was:—April 20, at 9h. 47m. 15s. mean time—right ascension, 6h. 25m. 15·6s.; polar distance, 20° 15′ 23″. Its motion is slow, towards the south-west.

THE instruments used by Dr. Livingstone in his last journey, a sextant, thermometer, and chronometer, are still exhibited in the map room of the Royal Geographical Society, together with some of his maps made in 1856-7. Those who have not before seen any of the maps will be interested in noticing the great care and neatness with which the work is done, and the amount of information crowded into them. There are also several portraits of the traveller taken at different periods.

AT the last meeting of the Linnean Society, Dr. Masters and Messrs. Hiern and Maw were appointed to represent the Society at the forthcoming congress of botanists in Florence.

THE removal of the Library of the Geological Society from Somerset House to Burlington House, has been completed, and it is hoped that by the 29th it will be opened to Fellows

MR. LEONARD LYELL, B.Sc., has been appointed Professor of Natural Science in the University College of Wales.

THE Brothers Henry, astronomers at the Paris Observatory, have invented a modification of Leon Foucault's process for testing his telescopic glass mirrors. They are using that process at Secretan's in the construction of lenses used for dioptric astronomical instruments. One instrument constructed by them has been tried at the observatory and proved highly satisfactory; an object of an inch is equal to one of two inches when the surface has been worked under their optical supervision. They reject every part of glass which is not perfect. The first *lunette astronomique* so constructed has been sold.

IN the last sitting of the Academy, M. Becquerel, senior, one of the greatest electricians of the age, was presented with a medal in commemoration of the Fiftieth Anniversary of the Academy, of which he became a member four years afterwards. In 1824 the sittings were private, and only open to a very few learned persons. The admission was considered to confer a great honour, and was a step preliminary to membership. It was only in 1834 that the secrecy was removed on the proposition of Arago; Biot raised an opposition to it, but was outvoted. The publicity of sittings was coupled with the publication of *Comptes Rendus*, a weekly journal, exclusively devoted to the papers read before the Academy, and which has rendered immense services for a period of thirty-nine years.

WE very much regret that Sir John Lubbock's bill for the Preservation of Ancient Monuments was thrown out of Parliament last week by a very considerable majority. Patriotism seems to be at a discount in the House of Commons.

PROF. BASTIAN, of Berlin, has received favourable news from the German expedition on the west coast of Africa. Dr. Gussfeldt, who is at the head of the expedition, has advanced into the interior, and reached the Fangela country, which, it is believed, is the right point for further advance into Central Africa. The travellers at the latest dates were at the station of Chinchato, and were busy with the preparations for the more important expedition.

THE German exploring expedition into the Libyan Desert, under the leadership of Gerhard Rohlfs, returned to Cairo on April 17.

MM. ANDRE and Rayet are at present publishing, at Gauthier Villars', a work on "The History of Astronomical Observatories." The first part which is on sale is devoted to British observatories. The learned astronomers remind their countrymen that at the end of the last century France had a greater number of astronomical establishments than all other countries. The same thing can now be said of Great Britain.

MR. HARRISON, as President of the Institution of Civil Engineers, will give a *conversazione* on Tuesday, May 19, in the west galleries of the International Exhibition at Kensington, which have been kindly placed at his disposal by H.M.'s Commissioners. As in the two preceding years, models of engineering works and of recent scientific inventions will be transferred to the west picture galleries from other portions of the Exhibition, and these will be supplemented by similar objects specially lent for the occasion.

DR. CARPENTER has replied to Mr. Carter's letter to Prof. King on the structure of the so-called *Eozoon canadense*. He complains that Mr. Carter makes his charges without having, according to his own admission, read what has been written in favour of the view of the organic origin. In support of this view the examination of specimens by Prof. Schultze at the end of last year is referred to, by which he was completely satisfied as to the Foraminiferal character of Eozoon. Dr. Carpenter says he does not pretend to affirm that the doctrine of the Foraminiferal nature of Eozoon can be *proved* in the demonstrative sense; but he does affirm that the convergence of a number of separate and independent probabilities all accordant with that hypothesis, while a separate explanation must be invented for them on any other hypothesis, gives it that *high probability* on which we rest in the ordinary affairs of life, in the verdicts of juries, and in the interpretation of geological phenomena generally.

The Society for Promoting Christian Knowledge has begun to issue a series of "Manuals of Elementary Science." Is it to be regarded as a sign of the times that this Society as a publishing body is devoting to the spread of a knowledge of Science funds which have been avowedly collected for the purpose of "promoting Christian knowledge"?

The re-mapping of England by the Geological Survey, giving the drift in its various divisions is steadily progressing, and in a short time a large part of Lancashire will be published. The quarter sheets, numbered by the survey 81 N.W., 88 S.W., 89 S.E., 90 S.E., 91 S.W. are engraved and in the hands of the colourist, and the work for 91 S.E., 90 N.E., 89 N.W. has been completed and the maps are in the engravers' hands. The sheet N.E. Somerset, and the London district have been ready some time. The old maps giving the rock mapping will continue on sale, for information that cannot be so readily gained in any other way arises from a comparison of the mapping of the rock with that of the surface drift.

According to the Abbé David, the Chinese river Hangkiang, until lately almost unknown, is an important river of commerce, traversed by vessels of every size. A considerable portion, however, is difficult of navigation, owing to the existence of numerous rapids and many rocks.

The death of Rev. John Bachman is announced as having taken place at Charleston on February 24. In the decease of this gentleman, Science loses one of the oldest of American naturalists, and one who has been quite prominent in the history of American zoology. He is well known from his association with Mr. Audubon in the preparation of the great work on the North American mammals, of which one edition was published, in folio, at 400 dols., and another, in quarto, at 40 dols. This, as far as its illustrations and biographies are concerned, still forms the standard treatise on the subject, although the systematic portion has been in a measure superseded by later and more critical investigations. It was, however, preceded by several monographic papers upon squirrels, hares, shrews, and other species, and also by papers upon the seasonal and other changes in colour in birds and mammals. Dr. Bachman's friends claimed for him the distinction of having been the first person in the United States to practise the art of artificial impregnation of fish, although this is stoutly contested by Dr. Garlick and other writers.

Mr. R. B. Walker writes from Corisco Bay, in Western Africa, in regard to a young gorilla which he had alive for some time, and hoped to forward to the Zoological Society of London. Contrary to the usual assumption in regard to this species, the specimen in question proved to be extremely docile and perfectly tame. When first purchased it was shy and suspicious, but not spiteful. At the expiration of about a week it was led around without resistance, and it ate whatever eatable thing it could lay its hands on, including a basin of condensed milk with a raw egg beaten up in it. It was quite tame, eating, sleeping, and playing with a large bull-terrier, the two animals being constantly together. It unfortunately disappeared one night, and was supposed to have fallen overboard.

The forthcoming number of Petermann's *Mittheilungen* will contain the conclusion of the account of the return journey of Count Wiltschek's Arctic expedition through North-east Russia, and some remarks on the geognostic survey map of the coast of the Waigatt Strait in North Greenland, between Disco Island and the mainland, by M. Steenstrup. The number will also contain an account of Gosse and Warburton's travels through West Australia (recently referred to in NATURE), accompanied, of course, by an excellent map.

A Roman Company, we learn from *La Nature*, proposes to lay a railway between Naples and Mount Vesuvius.

We would direct the attention of our physiological readers to a short paper which has just appeared in the "Proceedings of the Royal Society," by Mr. E. A. Schäfer, on the Intracellular Development of Blood-Corpuscles in Mammalia, in which he shows, in the subcutaneous tissue of the new-born rat, how the red-corpuscles, statically developed together with the primitive capillaries, become the dynamically circulating blood-discs of the older animal, as in the *area vasculosa* of the embryo chick.

We are glad to see that the Leeds Naturalists' Field Club and Scientific Association has just concluded the most successful year of its existence, its operations during the past twelve months having been attended by most gratifying and steady progress. We have received the syllabus of a number of lectures (by Mr. L. C. Miall) and excursions to take place during the present and next months, illustrative of the geology of the West Riding. There will be four lectures, illustrated by seven excursions.

We have received a short and carefully compiled sketch of the Geology of the County of Suffolk, written by Mr. J. E. Taylor, of the Ipswich Museum, a gentleman who, by the popularity of his lectures and the large audiences which he draws, is doing more than anyone to develop a genuine and lasting love for natural history in that part of the country.

Messrs. Longmans & Co. have in the press a "Manual of Industrial Chemistry." It is a translation of Profs. Stohmann and Engler's German edition of Payen's "Précis de Chimie Industrielle," by Dr. J. D. Barry. It will be edited and supplemented with chapters on the chemistry of the metals, by Dr. B. H. Paul, and will be copiously illustrated. The same publishers also have nearly ready an "Introduction to Experimental Physics, Theoretical and Practical," by Adolf F. Weinhold, Professor in the Royal Technical School at Chemnitz, translated and edited by Benjamin Loewy, F.R.A.S.; it will also have a preface by Prof. G. C. Foster, F.R.S., and be illustrated with numerous woodcuts.

The additions to the Zoological Society's Gardens during the past week include a Mourning Kangaroo (*Halmaturus luctuosus*) from the south of New Guinea, deposited by Signor L. M. D'Albertis; two Gold Pheasants (*Thaumalea picta*) from China, presented by the Rev. A. B. Frazer; a White-cheeked Flying Squirrel (*Pteromys leucogenys*) from Japan, presented by Mr. A. Gower, H.B.M. Consul at Kobe; a Common Fox (*Canis vulpes*) from Russia, presented by Mr. J. W. Ouchterlony; a Long-nosed Crocodile (*Crocodilus cataphractus*) from West Africa, presented by Mr. H. T. Cooper; and a Red Kangaroo (*Macropus rufus*), born in the Gardens.

SCIENTIFIC SERIALS

The *Geographical Magazine*, No. 1, April.—Such is the title of the successor to *Ocean Highways*, which a "Notice" informs us, "has been discontinued," Mr. C. R. Markham, C.B., F.R.S., "having taken the editorship of the *Geographical Magazine*, issued under new proprietorship." We certainly prefer the outside appearance of the new magazine to that of its predecessor, the cover being much more tasteful and business-like; it has made an excellent start also as to contents. The first article, accompanied by a map, is on "The Basin of the Helmund," which includes all the streams that flow down into the great lake or swamp of Sistan, and lies athwart the line of advance from the north towards India. A large part of this area is still entirely unknown, and the article gives an account of the existing materials whence a knowledge of the region can be obtained. The next article gives an interesting account of the Russian Staff-Captain N. M. Prshevalski's Travels in Mongolia in 1870–73. Captain Prshevalski "has acquired most valuable scientific information which, combined with the map he intends shortly to publish, will shed a flood of light on the geography, zoology, and botany of Mongolia and Northern Thibet." This is succeeded by an article on "The Hydrographical Department of the Admiralty," giving a brief history of this most important department of the naval service, and a sketch of its labours. The article contains some valuable hints as to how the department might be made more efficient than it is if Government would only be a little more wisely liberal. An article on the Island of Hormuz, by Lieut. A. W. Stiffe, gives an account of the present state of the island and of the remains of its ancient grandeur. We can only name the other original articles:—"A Highway to Bolivia," by Mr. Alfred A. Geary; "The Kashgar Mission," of Mr. Forsyth and party; "Dr. Beccari's Travels," in which Prof. H. H. Giglioli gives the latest news of the Italian traveller and naturalist, who has left Makassar for Kandari, an unexplored region of S.E. Celebes, where he hopes to secure specimens of the great Anoan antelope (*Anoa depressicornis*); "Geographical Progress in India in 1873;" and "the Products of West Africa," by M. W. Robinson. There are, besides the usual Reviews, Correspondence, Proceedings of Societies, &c.

Bulletin de l'Academie Royale de Belgique, No. 2, 1874.—The principal paper in this number is one by M. Montigny, in which it is sought to show that "the frequency of variations of the colours of stars in scintillation is generally in relation with the constitution of their light, according to spectral analysis." The author's observations embrace two distinct periods—one from Oct. 1870 to end of March 1871 (47 nights of observation), and the other from June to Dec. 1873 (19 nights). After referring to Secchi's three types of star-spectra, he gives a table of the stars observed, indicating the type of spectrum, the scintillations observed in a second at 60° zenith distance, the size, &c. It is found (1) that the stars scintillating most belong to the first type, or those with four spectral lines, while the stars showing weak scintillation are generally in the third group or type of nebulous bands and dark lines; (2) that the average, 86 (scintillations per second), of the first type exceeds considerably that of the third, which is 56. The average of the second group (the spectra resembling that of the sun) is 69, and thus intermediate, though a little nearer that of the third; (3) while some stars little differing in size resemble each other also in numerical intensities of scintillation (especially in the first type), no marked connection appears between the frequency of scintillation and the order of size of the stars; the last two types even present equal mean sizes, though their scintillations differ considerably. The average scintillations of the three types are in proportion of the numbers 14, 11, and 9. The author points out how his researches not only confirm M. Dufour's law that the red stars scintillate less than the white ones, but affords an explanation of it. The more frequent scintillation of the white stars is due to the fact, that, with equal distance of the observer, the total separation of the coloured bundles of rays, dispersed by the atmosphere, and which have emanated from a white star, is greater than in the case of a red star; the original rays of the white star being more numerous and more exposed to undergo frequent interception by the passage of aerial waves.—M. d'Omalius d'Halloy contributes a note on the Devonian system, and MM. Quetelet and Terby give accounts of auroræ boreales observed in January and February.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, March 1.—This number opens with the concluding part of a paper by M. Muhry in orographic meteorology. The author adduces evidence from hygrometric phenomena, that the permanent equatorial ascending-current forms the transition of the polar, into the returning anti-polar, current; he also proposes a new classification of clouds, according to ascent or descent. Some particulars are furnished, in a note, as to the climate in southern parts of Europe—Gibraltar, Tarifa, and San Francisco: and M. Jelinek translates a paper by Mr. Kingston of Canada, treating of the most suitable arrangement of thermometers in determining the temperature of the air.

March 15.—The beneficial effect of Alpine health-resorts has been attributed to the greater abundance of ozone in the mountain air. Dr. Haller here communicated the result of observations on the subject in July 1872 and 1873, made at Fusch Bad, in the Alps, at a height of 1,179 metres. Comparing data obtained at the meteorological central observatory of Vienna (194 metres), it appears, that in the bright and warm July of 1873, the ozone-contents of the air at Fusch Bad were considerably greater; by night about 2°·3, and by day 2°·6. In July of 1872, which was cold and rainy, the average of ozone was by night somewhat less (0·6) at Fusch Bad than at Vienna; by day, however, it was 2° greater. It seemed likely that, on further ascent, an increase of ozone would be met with, but after climbing to 23,000 metres, there was no marked difference.—This paper is followed by an account of M. Poey's recent observations (French Academy), on the relation between sun-spots and cyclones in the Antilles.—From a study of meteorological phenomena at St. Louis, Dr. Wislizenus finds that the electricity of the atmosphere shows a three-fold periodicity, daily, yearly, and secular (or cyclical). As to the second, the quantity of positive electricity increases in the colder months, reaches its maximum in January, and diminishes with increase of temperature to a minimum in July. The cyclical periodicity is probably one of ten years.—Among other subjects treated in this number are the formation of rain-stations in Bohemia, the inadequacy of the ozonometer at present in use, the decrease of water in springs, rivers, and streams.

Gazzetta Chimica Italiana. Fasc. I. e II. 1874. These numbers contain the following papers:—Studies in Toxological Chemistry. I. Search for solanine in cases of poisoning. II. Extraction of the alkaloids from the viscera, and search for nicotine, brucine, and strychnine. III. Detection of hydrocyanic acid in cases of poisoning, by Prof. F. Selmi.—Old and new Reactions of ordinary Phenol, by E. Pollaci.—A product of condensation of Oxalic Aldehyde, by H. Schiff. The substance obtained is formed according to the equation $6C_2H_2O_2 + H_2O = C_{12}H_{14}O_{13}$.—Action of Amides upon the Phenols, by Dr. J. Guareschi. The author has tried the following reactions:—paracresol and benzamide, methyl salicylate and benzamide, and ethyl salicylate and benzamide.—Concerning the action of Sulphur upon Calcium Carbonate, by Prof. A. Cossa.—Reduction of Silver Chloride by means of Sodium Hydrosulphite, by G. Scurati Manzoni. The chloride is reduced according to the equation $Na_2SO_2 + 2AgCl = 2Ag + 2NaCl + SO_2$.—On the Expansion of Fused Sulphur, by G. Pisati.—Upon the Reactions of Phenol, by G. Tasca-Lanza. The remainder of these numbers is principally devoted to abstracts from foreign journals. There is also a complete translation of Prof. Clerk-Maxwell's lecture on molecules, which has already appeared in our columns.

Journal de Physique, March.—This number commences with a paper by M. Bertrand, in which several known theorems relating to static electricity are demonstrated in a new and simple manner, which reduces them to a common principle.—M. Chautard describes an improvement on Mayer's acoustic pyrometer.—M. Lespiault calls attention to a new method proposed by M. Galle for estimating the height of the corona of aurora borealis. As applied to the aurora of February 1872, it gave 56 geographical miles (or 415 kilometres) for the absolute height. The agreement between results obtained from four different stations appears to confirm the principle on which M. Galle's method is based.—An ingenious mode of sending signals in opposite directions simultaneously, in a telegraphic apparatus of compressed air, is described by M. Deprez.—M. Gripon gives an account of some experiments made with a tuning-fork; referring to movement of cords or wires connected with it, vibration of wires in liquids, movement of liquid in a tube vibrated by fork, &c.—We further note a useful summary, by M. Violle, of MM. Favre and Valson's recent researches in crystalline dissociation, and an account of Prof. Tyndall's investigation as to acoustic transparency and opacity of the atmosphere.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti: t. vii. Fasc. iv.—In this number we find the continuation of Prof. Lombroso's researches on anthropometry and physiognomy of criminals. The results arrived at, from an extensive range of observation, are worthy of study. Among other things, the author concludes that criminals have, in general, a greater stature and weight, more ample chest, and darker hair than the normal; that they present a series of sub-microcephali (53 to 51) double the normal; that the index of the cranium tends to the brachicephalic, especially in assassins; that there is frequent cranial asymmetry; that, tested by the dynamometer, criminals show less force than the normal, but greater than lunatics; that, more often than in sane people, the eyes are chestnut or dark, and the hair is thick and black (especially in murderers); that incendiaries, and, still more, thieves, have very often the iris grey, and always a stature, weight, muscular force, and cranial capacity less than assassins or homicides. In concluding his paper, Prof. Lombroso remarks that prognathism, abundance and curliness of hair, scarcity of beard, frequent dark colour of skin, oxycephalus, obliquity of the eyes, smallness of cranium, development of jaws, retiring forehead, large size of ear, similarity of the two sexes, and scant muscular force, are points of resemblance between the European criminal and the Austral or Mongolian man.—Dr. Polli traces the recent progress of the doctrine of zymotic disease, and of the treatment of it with sulphurised preparations. Figures are given which show the largely increased production of sulphite of magnesia and sulphite of soda by certain chemical works in Italy, for medicinal purposes alone, within the last ten years.—MM. Bizzozero and Manfredi contribute a note in pathological anatomy, On the Development of Contagious Molluscum.—The Architecture of Ants forms the subject of a communication from Prof. Maggi, who has been studying the habits of *Formica fuliginosa* Lat.—M. Tessori furnishes a geometrical demonstration of the error of representations given in many treatises on physics, as to deviation of the plane of oscillation of the pendulum.—In the department of moral and political science, Prof. Bucellati has a paper on central military prisons.

Archives des Sciences Physiques et Naturelles, March 15.—This number commences with a *résumé* of spectroscopic observations of the sun, made at Geneva, by M. Emile Gautier during the last three years. The results of this work (carried on under much less favourable climatic conditions than in Italy), are mainly a confirmation of those got by other observers. The protuberantial phenomena are classed under three heads; eruptions, exhalations, and detached formations; all of which the author illustrates with drawings. Like P. Secchi he was often struck by the fact (which has been doubted), that when a protuberance is observed near a pole, there is generally one symmetrical with it, at the other end of the corresponding solar diameter, and near the opposite pole. The decrease in the number and dimensions of protuberances appeared during these years (from 1869) to precede and exceed that of the spots. M. Gautier adheres to the hypothesis of spots being formed by scorial matter resulting from cooling of the surface by radiation.—In the next paper M. Humbert gives a useful summary of what has hitherto been done by the *Challenger* expedition.—The *Bulletin Scientifique*, which follows, is larger than usual. Among other notes in it, we find an account of some instructive researches by Dr. Macaluso, on polarisation of electrodes, by chlorine and hydrogen. There is also a notice of an important geological map of the Austro-Hungarian Empire, recently completed by M. de Hauer, whose name it bears. The publication, directed by Heidenhain from 1850 till 1863, represents at least twenty years' labour (under considerable difficulties), of a large number of eminent geologists. Each plate is accompanied with detailed explanations. We further note a *résumé* of some recent researches on the minute structure of the eye; and another paper on physiological antagonism of poisons, in which are described some observations by MM. Martin-Damourette, Rosbach and Fröhlich, and others, with regard to the effects of physostigmine, the active principle of Calabar bean, and atropine.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, April 9.—Prof. Cayley, vice-president, in the chair.—Mr. G. H. Darwin read a paper On Probable Error in Statistics. He stated that he had been at work at a statistical inquiry, and was desirous of forming some idea of what degree of accuracy he had a right to expect from the collection of a given number of cases. He put the problem into the following form:—A bag is known to contain a very large number of black balls and white balls, mixed at hazard; on drawing a large handfull of n balls, I find p are white and the rest black. What is the probable error in asserting that $\frac{p}{n}$ of all the balls in the bag, are white? n and p, though large numbers, are supposed to be small compared to the number of balls in the bag. Mr. Darwin then made some further remarks On the Combination of Statistics. The question he considered was the following:—If X and Y are measurements or estimations of quantities such that the errors are distributed according to the exponential law, what is the "probable error" of XY and $\frac{X}{Y}$ in terms of the moduli c and c' of X and Y respectively? M. J. W. L. Glaisher made some remarks on the papers, drawing the author's attention to the fact that the two questions had been treated of by Laplace and De Morgan.—Mr. Merrifield then gave a sketch of his paper entitled Determination of the Form of the Dome of Uniform Stress. He remarked that the general question of the equilibrium-figure of a thin dome is indeterminate, even when the law of thickness or density is given, and it thus differs from the question of the arch, by requiring the assumption of a further condition in order to render its form determinable. If the two following conditions are introduced simultaneously into the general equations, he stated that a very remarkable simplification occurs in the analysis:—(1) that the thrust along a meridian shall equal the thrust along the parallel per unit of area at every point; (2) that the normal thickness shall vary in such a manner that the area under compression shall be proportional to the thrust. These seem to be the conditions necessary to the economical use of building materials of homogeneous character, for the maximum stretch is evidently least when the stress is equally distributed through the whole of the material. The form obtained bears a general resemblance to the upper half of a claret bottle, and the dome evidently required a heavy lantern.—Mr. A. J. Ellis gave an explanation of his theory that ordinary (commutative) algebra is the calculus of similar triangles upon one plane. Taking two fixed points O and I, any third point A determines a triangle, so that if B be a fourth point, it is immediately possible to find a fifth point C, such that the triangle B O C shall be similar to the triangle I O A, and have the angles thus named turned in the same direction. Marking this operation by a, as being determined by the position of the point A, and terming it a *clinant*, he showed that clinants obey every law of commutative algebra, so that it was possible to consider any and every existing algebraical expression as a clinant, and hence as determining a point in a plane. Clinants thus embraced not only the integers and fractions of ordinary arithmetical algebra, but incommensurables, negatives, and imaginaries. Hence also if x and y be any clinants, and $f(xy) = o$, if x be determined by taking X anywhere, a corresponding point Y would be determined. Hence arose a complete calculus of the correspondence of points in a plane, which Mr. Ellis calls *stigmatic geometry*, and which he showed comprehended under one set of equations and greatly generalised, not only the algebraical geometries of Descartes and Plücker, but the homographic geometry of Chasles, and from a single general principle gave a perfect geometrical representation of all the imaginary cases as part of one conception with the real cases. The actual algebraical work, though having the old form and obeying the old laws of operation, is greatly simplified by the clinant signification attached to the symbols, and in especial the expression and determination of direction is rendered easy and certain. (A more detailed explanation will be given, the speaker said, in his "Algebra Identified with Geometry," at the present time in the printer's hands.)—Prof. H. J. S. Smith made a short further communication in reference to his former paper On the Higher Singularities of Plane Curves.—A paper by Mr. H. M. Taylor, On Inversion, with special Reference to the Inversion of an Anchor-ring, was taken as read. Some of the properties given in the paper have been already given by Maxwell (Quart. Journ. Math., vol. ix.) where excellent stereoscopic views of four species of cyclides are given, and by Cayley in the same journal, vol. xii., and in a paper in the Phil. Trans. by Casey. The novelty of the paper consisted in the point of view from which the properties of the cyclides are investigated, viz. as the inverse figures of the anchor-ring, many of whose geometrical properties are as easily seen as those of the circle.

Linnean Society, April 16.—H. Trimen, M.B., in the chair.—A number of papers were read, being Nos. 3-14 of the series of contributions to the botany of H.M.S. *Challenger* Expedition, as follows:—Notes on Freshwater Algæ collected in the boiling springs at Furnas, St. Michael's, Azores, and their neighbourhood, by H. N. Moseley.—Note on the foregoing communication, by Prof. Thiselton Dyer.—Notes on some collections made at Furnas, by M. Archer. The diatoms belong to species of most frequent occurrence in fresh water, and appear to be in no way affected by the high temperature. The other Algæ are mostly common species, several of them British, belonging to the genera *Spirogyra*, *Mesocarpus*, *Bulbochæte*, *Œdogonium*, &c.—Notes on plants collected at St. Vincent, Cape de Verdes, by H. N. Moseley—Enumeration of Algæ collected by Mr. Moseley at the Cape de Verdes, by Dr. G. Dickie.—Enumeration of the fungi collected during the expedition of H.M.S. *Challenger*, Feb.-May 1873, by the Rev. M. J. Berkeley.—Note on plants collected at St. Paul's Rock, by H. N. Moseley. The only aerial plant found on the island was a *Chlorococcus*.—Enumeration of the Algæ collected by Mr. Moseley at St. Paul's Rock, by Dr. G. Dickie.—Notes on plants collected at Fernando Noronha, Cape de Verdes, by H. N. Moseley.—Enumeration of Algæ collected by Mr. Moseley at Fernando Noronha, by Dr. G. Dickie.—Enumeration of Algæ collected by Mr. Moseley in 30 fathoms of water at Barra Granda, Pernambuco, by Dr. G. Dickie.—Enumeration of Algæ collected by Mr. Moseley at Bahia, by Dr. G. Dickie.

Chemical Society, April 16.—Prof. Odling, F.R.S., president

dent, in the chair.—Dr. A. W. Tilden read a paper On Aqua Regia and the Nitrosyl Chlorides. He finds that when the gases evolved on gently heating aqua regia are passed into concentrated sulphuric acid, a product is obtained which, at a low temperature, deposits crystals of nitrosyl sulphate, $NOHSO_4$. Both these crystals, and the liquid producing them, when mixed with sodium chloride and gently heated, evolve nitrosyl chloride NOCl, an orange-yellow gas, which may be condensed to a deep orange-red liquid boiling at − 8° C. The author could not obtain the dichloride $NOCl_2$, which Guy Lussac supposed to exist, but which he believes to be merely a solution of chlorine in the monochloride.—Dr. C. R. A. Wright then read a paper On Isomeric Terpenes and their Derivatives, Part IV. § 1. On Cajeput Oil, by C. R. A. Wright and C. Lambert. It was found that the cajeputol, $C_{10}H_{18}O$, boiling at 176°—179° C., obtained from oil of cajeput, combines with bromine forming the compound $C_{10}H_{18}Br_2O$. On heating this it splits up into cymene, $C_{10}H_{14}$, hydrobromic acid, and water. § 2. Action of Pentasulphide of Phosphorus on Terpenes and their Derivatives, by C. R. A. Wright. When cajeputol is treated with the pentasulphide, it yields a mixture of terpene and cymene, the latter being formed by a secondary action of the pentasulphide on the terpene. This was shown really to be the case by treating the terpene from oil of turpentine and hesperedene with the pentasulphide, when cymene was formed in both cases.

Anthropological Institute, April 14.—Prof. Busk, F.R.S., president, in the chair.—Mr. John Brent exhibited and described a series of flint implements from Canterbury and Reculver.—A description, by Mr. Howorth, was read of an Ashanti fetish letter, or curse. The document, which was lent by Capt. Gordon for exhibition, was written in the Arabic character and in the language of the Barbu tribe, on a sheet of rough paper of large foolscap size, folded about two inches square and tied with green thread. The letter contained a prayer that the English might fight among themselves and return to the coast, and that pestilence might overtake them. The Ashanti grievances were enumerated, and it stated that the white man came with covetous eyes and seized the land, and that covetousness brought down the curses of Suleiman the high priest. It was thought by the English scouts that it was Suleiman himself who endeavoured to stay the British troops on their approach by throwing down the fetish, and that his failure would probably cost him his life.—Capt. S. P. Oliver, R.A., contributed a series of papers On the Non-historic Stone Relics of the Mediterranean. The series comprised full accounts, with ample illustrations, of the Torre dei Giganti, Malta; Tumuli near Smyrna; Dolmen-mounds of the Albegna; Sardinian Nuraggis; and the Sepolture de is Gigantes of Sardinia.

Meteorological Society, April 15.—Dr. R. J. Mann, president, in the chair.—On the Climate of Patras, Greece, by Rev. H. A. Boys. The author shows that the climate of Patras is naturally mild and relaxing, seldom disagreeably dry, and not often very damp, being indeed drier by a good deal than any part of England.—Remarks on the Atlantic Hurricane of August 20 to 24, 1873, by W. R. Birt.—On the Meteorology of December in the southernmost part of the Southern Indian Ocean, by Robert H. Scott, F.R.S. This paper has been prepared for the purpose of giving information on the climate of Kerguelen Island to those gentlemen who are going out to observe the Transit of Venus in December next.—On the Diurnal Variations of the Barometer, by J. K. Laughton. Whilst it has long been well known that barometric maxima and minima occur daily with unfailing regularity, especially within the tropics, the cause of this recurrence is yet unknown; and though it has been attributed to the different temperature and humidity at different times of the day, such explanation is far from satisfactory, for the maxima occur at the times of mean temperature and humidity without regard to the direction of the change, and the minima occur indifferently at the times of both greatest and least temperature and humidity. It seems that an explanation is rather to be found in the inertia of the atmosphere, which in the first instance permits its elastic force to be increased by a rapidly increasing temperature before the inertia of rest can be overcome sufficiently to allow it to enlarge its volume in due proportion, but when that inertia of rest is overcome, then the inertia of motion permits it to move away from the place of observation in excess of what is due to the increased elasticity; the nocturnal maximum and minimum being caused by the resilient power of the air, which gives it alternately an inward and outward motion, and each way in excess of what is due to the decrease or increase of elasticity by reason of the inertia of motion. If this explanation is correct, we ought to find a certain tendency of the wind towards east in the morning and towards west in the evening; and this tendency does seem to be shown in the very few published observations which permit a comparison to be made. Further observations, as confirming or disproving the proposed theory, are much to be desired.

Victoria Philosophical Institute, April 13.—Mr. Edmund W. Gosse, of the British Museum, read a paper On the Ethical Condition of the Early Scandinavian Peoples, in which he illustrated the peculiar features of the civilisation of Scandinavia in pagan times, and showed in what salient points that civilisation differed from the spontaneous developments of morality in other cultivated heathen races—the Elder or Poetic Edda of Sœmund Sigfussen being taken as the text.

MANCHESTER

Literary and Philosophical Society, March 24.—Rev. William Gaskell, vice-president, in the chair.—On some of the Perplexities which the Art and Architecture of the Present are preparing for the Historians and Antiquarians of the Future, by the Rev. Brooke Herford.—A Few Observations on Coal, by E. W. Binney, V.P., F.R.S. From his observations the author was led to conclude that soft or cherry coal was chiefly composed of the bark, cellular tissue, and vascular cylinders of coal plants with some macrospores and microspores. That caking coal had much the same composition, except that it contained a greater proportion of bark in it. That splint coal had a nearly similar composition, but with a great excess of macrospores. That cannel coal, especially that yielding a brown streak, was formed of the remains of different portions of plants with a great excess of microspores, which had long been macerated in water. These conclusions were arrived at merely as to the composition of the different kinds of coal. No doubt each seam would be materially affected by the nature of the roof, whether the latter was an open sandstone or a close and air-tight black shale or blue band, for the former would allow the free escape of gaseous matter, and the latter would prevent its escape. It is well known that the character of the roof has a deal to do with the quality of the coal under it.

April 7.—E. W. Binney, F.R.S., vice-president, in the chair.—The chairman exhibited to the meeting some portion of the cast-iron roof from the Salford Station of the Lancashire and Yorkshire Railway, which after having been up for a period of four years was so much corroded and damaged that it had to be taken down. He attributed the effects to sulphuric acid and soot arising from the combustion of the coal used in the locomotives passing under it, aided by the action of steam and vibration.—On the Action of Nascent Hydrogen on Iron, by William H. Johnson, B.Sc. In a paper read before the Society last year, the author showed that a piece of iron immersed in hydrochloric, sulphuric, or other acid which evolves hydrogen by its action on the metal, on breaking gives off bubbles of gas from the surface of the fracture. It subsequently occurred to the author that these bubbles might be produced by subjecting the metal to the action of nascent hydrogen for some time, and without the aid of acid at all. To test this he connected two pieces of iron wire ·07″ diam. respectively with the copper and zinc plates of a battery of 50 Daniell's cells and immersed them in a vessel of Manchester town's water at a distance of one inch apart. On closing the current, bubbles of hydrogen were given off from the wire connected with the zinc, but none from the wire connected with the copper, the oxygen liberated at the pole apparently forming oxide of iron which in 12 hours formed a thick smudge at the bottom of the vessel. After 24 hours the surface of the wire connected with the zinc was unchanged, but on moistening the fracture bubbles were given off abundantly just as if it had been immersed in acid. The other wire, on the contrary, though much oxidised and eaten away, did not give off bubbles when broken. A variety of experiments were made in the same way with similar results. The author concludes that if the oxidation of the surface of iron be as a rule accompanied by the absorption of nascent hydrogen into the interior of the iron, then the diminution of strength and toughness consequent on this will affect iron ships, telegraph cables, and other structures in which iron is largely used and which are constantly immersed in water.

EDINBURGH

Geological Society, March 13.—Mr. Andrew Taylor exhibited a specimen of coal converted by a recent explosion in a Lancashire pit into anthracite, and even in some parts into graphite.—A paper by Mr. Payne was read, On the Oolitic coalfield of Brora, Sutherlandshire. One of the coal-seams, about 3 feet 6 inches thick, is being worked at a depth of from 720 to 300 feet.—Mr. Taylor then read three short papers on (1) An analysis of various coals and peats. (2) Specular iron recently discovered in New South Wales. (3) Shale recently discovered at Waitata, New Zealand.—Mr. Peach stated that, in the course of preparing these sections, he had made a discovery which may yet prove to be of some service in the Fine Arts, viz., that the pounded dust of such shale as this, an enormous bed of which occurs in New Zealand, yields a colouring material closely resembling sepia, a costly substance.

April 16.—Mr. David Milne Home, F.G.S., president, in the chair.—The first paper was read by Dr. Ramsay H. Traquair, Keeper of the Natural History collection in the Edinburgh Museum of Science and Art, On the Structure and Affinities of the genus Cheirolepis. Dr. Traquair submitted the following conclusions at which he had arrived on the matter:—(1) That Agassiz was correct in ascribing branchiostegal rays and irregular dentition to the cheirolepis, but the larger teeth are placed in a distinct row internal to the smaller ones, not in the same line as Agassiz described them. (2) That the plates described by Powrie as principal jugulars belong to the shoulder girdle, being in fact the interclavicular plates of Parker; and that cheirolepis has no jugular plates. (3) That the osteology of cheirolepis shows it to be so closely allied to Palæoniscus that it ought to be included in the same family, notwithstanding the minuteness and non-overlapping character of the scales.—Mr. George Lyon read a paper On a Species of Griffithides (Trilobite) from a limestone quarry south of Dalkeith, near Edinburgh, and which belongs to a genus extremely rare.—Mr. David J. Brown read a paper On a new Theory of the Formation of Till, or Boulder-clay. The author submitted that till is in reality formed by glaciers, after they enter the sea, tearing up the rocks that form its bed, and grinding them to boulders and mud, and that this mud deposited along with the boulders forms boulder-clay.

VIENNA

Geological Institute, Jan. 7 (anniversary meeting). The Director, Fr. v. Haner, read the annual report, which states, that during the last year the palace of Prince Liechtenstein has been purchased for the collections, the library, laboratory, and the working rooms of the institute. The staff has been reorganised, and now consists of the Director, Fr. v. Haner; Vice-Director, Fr. Foetterle; Chief Geologists, D. Stur, G. Stache, and E. v. Mojsisovics; Chief of the Chemical Laboratory, K. v. Haner; Geologists, H. Wolf and K. Paul; two adjuncts, O. Lenz, the second at present being vacant; two assistants, A. Redtenbacher and K. John; two practitioners, C. Doelter and R. Hörnes.—After mentioning the share which the institute took in the general exhibition of last year, the report announces that geological explorations have been carried on during the last summer in the Bukovina as well as in the Tyrol, whence the examination of the northern chain of the Austrian Alps was finished with the Bregenzer-Wald (Vorarlberg), whilst that of the central chain was continued in the environs of the Oetz valley and the Ortler mountains, and that of the southern chain was begun in the environs of Lienz, in the valley of the Drau. Grateful allusion is also made to the liberal foundation of a capital of 12,000 florins in bonds of the Southern Railway Company, the gift of Albert Schloenbach, of Salzgitter, Hanover, in memory of his late son, the eminent geologist, Urban Schloenbach. The annual interest of this sum will be given to officers or friends of the Geological Institute, to enable them to travel in foreign countries to compare geological observations made in the Austrian dominions with those abroad. The first to whom it has been granted is D. Stur, whose studies on the exact geological position of the Bohemian coal-beds are likely to lead to very interesting results; results, however, which require a comparative study of other coal basins, and chiefly of the rich collections of fossil plants in the Museum of Dresden, for their secure confirmation.—The following specimens have been newly arranged in the museum of the Institute:—The silurian fauna of Galizia; the Devonian fauna of Moravia; the carboniferous flora of Ostrau-Orlau-Karwin, of Schazlar-Schwadowitz, of Kladno-Schlan, of Swina, of Stradonitz, of Radnitz and its environs, of the Pilsen basin, of the Rossitz basin, the flora and the fauna of the old red in Austria, Moravia, and Bohemia; the cretaceous flora of Moravia and Bohemia; the flora of many tertiary deposits in Bohemia, and of Wieliczka and Swaszawice, in Galizia. In the chemical laboratory, more than 300 analyses and assays have been performed, the library has been augmented by 661 volumes, and the collection of maps by 194 sheets. The progress of the publications appears very satisfactory: besides the periodicals, the *Jahrbuch*, the *Mineralogische Mittheilungen* and the *Verhandlungen*, four sheets of the "Memoirs," were edited, viz., Vol. V., No. 4, On a Fossil Saurian from Lesina, with 2 plates, by Prof. A. Kornhuber; No. 5, On the Cephalopods of the Gosau beds of the north-eastern Alps, with 9 plates, by Dr. A. Redlenbacher; No. 6, Fauna of the beds of Aspidoceras acanthicum, with 13 plates, by Prof. M. Neumayr; and Vol. VI., No. 1, The Fauna of the Flambach and Halstatt beds, with 32 plates, by Dr. E. v. Mojsisovich. Also, a Geological Map of the Environs of Vienna, on the scale of 1 : 28800, with explanations by Th. Fuchs, and a Catalogue of the Objects exhibited by the Institute at the General Exhibition, have been published. Finally, the most important work has been the completion of M. v. Haner's large geological map of the Austro-Hungarian empire, printed in colours on the scale of 1 : 576000, the last four sheets of which were published last year. Further communications were made by T. Hirschwald, On the Transformation of Wood into Brown Coal, in the Mine Dorothee, on the Ober-Starz; by S. Nedeljkovic, On the Sanidin-Trachytes of Syrmia; Dr. A. Redlenbacher, Remains of *Ursus spelæus* from a cavern near Wildalpe, Upper Styria; Dr. G. Stache, On the Fauna of the lower eocene beds of Cosina, in Istria; Dr. C. Doelter, On some Eruptive Rocks in the Transylvanian Erzgebirge.

GOTTINGEN

Royal Society of Sciences, Jan. 3.—M. Wielen communicated the results of an examination of Greek names of makers inscribed on ancient earthenware lamps in several archæological collections in Athens, Corinth, and Smyrna.—M. Lolling presented a paper on the Theseion and the Hephaisteion in Athens.

RIGA

Society of Naturalists, Nov. 5, 1873.—M. Russwurm furnished some interesting particulars as to the seal-fishing on the Russian coasts. The Baltic supplies annually about 12,000 animals, with a value of 125,000 roubles (the rouble = 3*s*. 1½*d*.); the White Sea and neighbouring parts, 18,000 animals, worth 212,000 R.; the Caspian Sea, 100,000 animals, worth 900,000 R. The Russians (unlike the Finns, &c.) do not eat the flesh of seals, but throw it away. The various species met with, as also the mode of capture, were described.

Nov. 19.—Dr. Gutzeit gave an account of a new official map of Russia, just completed at St. Petersburg.—M. Teich communicated some observations on the power of scent in butterflies; he thinks they are greatly guided by the sense of smell, which has its seat in the feelers.

Dec. 3.—Prof. Petzholdt read a paper on structural relations of ice and axes of crystals.—Prof. v. Sivers made some observations on driftwood collected in the Arctic regions by the recent German expedition.

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THURSDAY, APRIL 30, 1874

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

Association Française pour l'Avancement des Sciences. Comptes Rendus de la 1re Session, 1872. Bordeaux. (Paris, 1873.)

THIS, the first volume of the yet young French Society's Proceedings, does it infinite credit. It is a handsome, beautifully printed volume of 1,330 pages, containing upwards of 200 papers, addresses, and lectures on a wide variety of subjects connected with Science, pure or applied. The volume is also well illustrated, some of the plates appended being coloured, a feature which we think the British Association would do well to imitate in its "Proceedings."

The French Association, as our readers no doubt know, made a very auspicious start, the number of members amounting to somewhere about 800. There are two classes of members—1st, *membres fondateurs*, who subscribe one or more shares of the capital of the Association, a share amounting to 500 francs; there are about 250 members of this class, some of whom have subscribed several shares, among the latter being a considerable number of railway and other public companies: 2nd, ordinary members, paying an annual subscription of 20 francs, or a life-subscription of 200 francs; the names of about 50 life-members are in this volume. After an existence of scarcely three months, the Association possessed a capital of nearly 140,000 francs, and an annual revenue of more than 16,000 francs.

The French Association is modelled pretty closely after the older British one, its aim being, according to the rules, "to promote by every means in its power the progress and diffusion of the sciences from the double point of view of the perfection of pure theory and of the development of their practical applications." These ends it proposes to accomplish by means of meetings, lectures, publications, and donations of instruments or money to persons engaged in scientific researches. It appeals for help to all those "who believe that the cultivation of Science is necessary to the greatness and the prosperity of the country."

The Association is divided into four Groups, and each group into several sections; the Groups are—1. The Mathematical Sciences; 2. Physical and Chemical Sciences; 3. Natural Sciences; 4. Economic Sciences. The French Association devotes more attention to the practical application of scientific principles than does the British one; the 1st Group, for example, including Sections of Navigation and of Civil and Military Engineering; the 3rd Group including the Medical Sciences, and the 4th Group Agriculture. This arrangement may at present have some advantages in France, where there are probably fewer special Associations than there are in this country, and because, until the Association gets itself firmly established, it may be advisable to appeal to as many classes of supporters as possible: but we are inclined to believe that it will by and by find that it will serve the cause of Science more effectually by confining its attention to the pure sciences.

In points of administrative detail, the French follows very closely the British Association. One of its rules ordains that each year the capital fund be increased by 20 per cent. of its revenue. If it prospers in the future as it has done hitherto, we have no doubt that it will soon have a very large sum at its disposal.

As we noticed pretty fully the proceedings of the Association at the time of its meeting at Bordeaux in September 1872, it is unnecessary to notice in detail the papers contained in the volume before us. There will be found in its pages the names of many of the most prominent men of Science in France, and a few belonging to foreign countries, among the latter being Sir Benjamin Brodie and Dr. Gladstone. Two of the published lectures have been published in NATURE *in extenso*—that of M. Janssen on the Eclipse of December 12, 1871, and that of M. P. Broca on the Troglodytes of the Vézère.

M. de Quatrefages, the first President, in his eloquent and powerful opening address, speaks very highly, and we would fain hope with justice, of the work which has been done by the British Association. "Thanks to it," he says, "a part of the population has been reformed. The sons of those fox-hunters, who, as a relief from their rude pastimes, only knew of joys equally violent and material, are now botanists, geologists, physicists, and archæologists."

The President's impressive words as to the sphere of Science at the present day are well worth quoting:—"Science is at present everywhere; she is becoming more and more the sovereign of the world. What industry can dispense with the aid of mechanics, and is there any industry which would wish to be bound to the progress already realised by that Science? Is there one which would despise the help of Chemistry? What physician, worthy of the name, would consent to dispense with physiology, that complex science, daughter of chemistry? with physics and with mechanics, any more than with anatomy? What enlightened agriculturist does not understand that the problems of culture and of production are essentially questions of zoology, botany, geology, and chemistry? And in this great city (Bordeaux), one of the queens of universal commerce, what merchant will deny the importance of geography? Science is as indispensable to the military man as to the manufacturer, the physician, the agriculturist. Certainly I am far from denying the part which in war will always fall to courage, to inspiration. But inspiration must be enlightened by study; bravery must be furnished with arms equal to those of the enemy. Revive in imagination Renaud de Montauban or the Roland of legend; place them upon Bayard or Frontin; cover them with their enchanted armour, and dart them against a simple mechanic mounted upon his locomotive. You all know what will be the result of the shock: coursers and paladins will be brayed."

It will be remembered that the first meeting of the French Association took place while the country was yet sore with the humiliation inflicted upon it by Germany; and very naturally the address of the President, as well as the addresses of many others who spoke, took their tone, to some extent, from this condition of affairs. Still the character of these addresses, though intensely patriotic, is perfectly healthy, the various speakers showing that they possessed a clear perception of the most effectual means of raising and advancing their fallen country; they read aright the lessons of the recent war, and declared that Science alone, in its widest acceptance, could be the saviour and elevator of France. And, indeed, there is the greatest hope of a country that has produced men, and that in so great numbers, capable of doing the work the results of which are chronicled in the handsome volume before us; for we are persuaded that this first volume of the French Association's Proceedings will compare favourably with any single volume of the Proceedings of the British Association. The meeting last year at Lyons fully bore out the promise of the first meeting, and we have no doubt that this year's meeting at Lille will be at least equally successful. Let the members of the Association only do all in their power to keep up its high character and carry out faithfully its declared objects, and the beneficial results of its establishment both to Science and to France will, ere long, be evident. As it is, partly no doubt owing to the work of the Association, Science since the conclusion of the Franco-Prussian war has taken immense strides in France; everything taken into consideration, the amount of scientific activity which has recently been developed in that country is very wonderful, and calculated to call forth the gratitude of the friends of science and humanity.

NORTH AMERICAN BIRDS

A History of North American Birds. By S. F. Baird, T. M. Brewer, and R. Ridgway. Vols. i. ii. and iii. Land Birds. (Little, Brown and Co., 1874.)

THE ornithologists of the United States appear to be not less active than those of this country at the present moment. Whilst here we have Gould's "Birds of Great Britain," Dresser's "Birds of Europe," and Newton's new edition of "Yarrell," all appearing at the same time, so in America Coues's "Key" and Cooper's "Birds of California" are quickly followed by the present important work on the whole of the North American Ornis. For this undertaking Prof. Baird, the well-known Assistant-Secretary of the Smithsonian Institution, has obtained the assistance of two very efficient coadjutors, Dr. T. M. Brewer, of Boston, and Mr. Ridgway, already well known for his accurate work in ornithology.

The object of the present work, which aims at a wider grasp than any of its predecessors, is to give an account of what is known of the birds, not of the United States only, but of the whole of the Continent of North America north of the Mexican boundary. Greenland is included on the one side, and the newly acquired United States territory of Alaska on the other, so that many European and Asiatic forms, which have been lately discovered in these two countries, are now for the first time added to the American list.

The materials upon which this undertaking is principally based consist of the very extensive collections of birds from every part of the New World, in the Smithsonian Museum at Washington. The numerous expeditions for exploration and survey sent out of recent years by the Government of the United States into nearly every portion of their enormous western domain have been invariably accompanied by one or more collectors whose contributions have all been deposited in the stores of the Smithsonian Institution. But besides their collections these investigating naturalists have reaped a rich harvest of facts concerning the life-history of the creatures they have collected, and have deposited their records and journals also in the Smithsonian Archives. From these manuscripts, particularly from the notes of the late Mr. Robert Kennicott, who made most extensive explorations in Western America and in the most northern portion of the Hudsons Bay Territory, many of the most novel facts recorded in the present work have been drawn.

The special value of the researches of Mr. Kennicott and his fellow-workers in the north-west lies in the fact that a large number of the rapacious birds and waterfowl of North America resort in summer to these thinly-populated districts for the purpose of breeding. Their haunts, not having been previously invaded, much novel information on the nesting habits of the members of these two groups is for the first time published in this work.

Besides Messrs. Baird, Ridgway, and Brewer, whose names appear on the title-page, we are informed in the preface that two other well-known American naturalists have contributed to the present work—Prof. Gill having furnished a portion of the introduction, and Dr. Coues the tables of the orders and families.

The work is profusely illustrated by woodcuts, besides containing a series of illustrations of the heads of all the species, drawn upon separate plates. The woodcuts contain the outlines of the principal characters of every genus, embracing the shape of the bill as seen from above and from the side, the comparative lengths of the wing and tail feathers, and the outline of the tarsus and toes; besides reduced but well-executed and highly-characteristic whole figures of many of the species.

The tendency of the American ornithologists of late years has been rather to unduly augment the number of species by raising slight local variations in form and structure to specific rank. In the present work rather the opposite tendency is manifested, and we are not sure that it is not in some instances carried too far. For instance, the whole of the Purple Martins, of the genus *Progne*, recently divided by Prof. Baird into seven or eight species, are now treated of as one; and the different species of Redpole Linnets of Dr. Coues are again reduced to their primitive number. As, however, the distinctive characters, such as they are, are invariably stated with accuracy and precision, it does not really make much difference whether the forms are actually classed as species or varieties.

The three volumes of this elaborate work now before us contain the whole of the Land Birds. A fourth volume, shortly to be issued and to be devoted to the Water Birds, will complete the undertaking. There can be no doubt, as will be at once apparent to anyone who consults the work, that it is of a most complete and exhaustive character, and that it will fully sustain the well-known reputation of Prof. Baird and his fellow-labourers.

OUR BOOK SHELF

Our Common Insects. By A. S. Packard, jun. (Naturalist's Agency, Salem, Mass.)

In this fully illustrated little work Mr. Packard, the author of the excellent and much larger "Guide to the

Study of Insects," gives a short and popular account of entomology generally, by taking a series of types from amongst the best-known North American insects, and describing them in detail. We should have liked to find some of the descriptions rather more explicit, as they might have been, without any alteration in the size of the volume, if some of the illustrations had not been so frequently repeated. In a work like Euclid there is no doubt considerable advantage in having the figures so placed that it is not necessary to turn over the pages in referring to them, especially when it has to be read by boys; but when space is short and the subject of such general interest, we cannot help feeling that their repetition, three times in more than a single instance, is quite uncalled for. The author's own work at the development of Insecta, which he has published in the "Memoirs of the Peabody Academy of Science," enables him to take a larger view of his subject than that held by most. This is particularly indicated in the very suggestive chapter entitled "Hints on the Ancestry of Insects," in which the researches of Ganin, Lubbock, Brauer, Haeckel, and Muller are all brought to bear on such questions as the relation of the *Zoea* form of the embryonic Crustacean to the similarly undeveloped and generalised, here termed *Leptus*, form of Insecta, in which the configuration is ovate, the head is large, bearing from two to four pairs of mouth-organs resembling legs, and the thorax is merged with the abdomen; this general embryonic form characterising the larvæ of the Arachnida, the Myriapods, and the true Insects. The elaborate observations of the first-named of these authors on the development of *Platygaster error*, an ichneumon parasite, in the author's mind tend to confirm the theory held by him that the ancestry of all the Insects, including the Arachnids and Myriapods, should be traced directly to the worms. We recommend this small book to all interested in the progress of this branch of invertebrate zoology.

The Transactions of the Academy of Science of St. Louis, vol. iii. No. 1. (St. Louis, U.S., 1873.)

THIS volume contains a journal of the proceedings of the Society from March 1868 to January 1873, and a few papers *in extenso*. The latter are:—Notes on the Genus *Yucca*, by G. Englemann; On the new Genus in the Lepidopterous Family Tineidæ, with Remarks on the Fertilisation of the *Yucca*; and Supplementary Notes on *Pronuba yuccasella*, by C. V. Riley; Descriptions of North American *Hymenoptera*, by B. D. Walsh; Atmospheric Electricity, by Dr. A. Wislizenus, being the yearly report of atmospheric electricity, temperature, and humidity, from observations made at St. Louis; Catalogues of Earthquakes for 1871, by R. Hayes; and On the Occurrence of Iron Ores in Missouri, by J. R. Gage. Mr. Hayes, on the basis of the recorded earthquakes from 1739 to 1842 has found that the "largest maxima occurred in the years of the heliocentric conjunction and opposition of Jupiter and Saturn, with but three exceptions, and in these cases the increase began in those years, but the maximum was not reached till the following year." He suggests that "these planets induce electric currents which *call into action* those forces to which the causes of seismic phenomena are usually ascribed."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Herbert Spencer and *à priori* Axioms

MR. HERBERT SPENCER (vol. ix. p. 461) has "ended what he has to say on the vexed question of the origin of physical axioms" by laying down—

(1) That "the perceptions and inferences of the physicist cannot stand without *preconceptions which are the products of simpler experiences than those yielded by consciously-made experiments.*"

(2) That "the preconception which immediately concerns us is the exact quantitative relation between cause and effect."

(3) That "if definite quantitative relations between causes and effects be assumed *à priori*, the Second Law of Motion is an immediate corollary."

By speaking of it as an "*immediate corollary*," I presume that Mr. Spencer means that Newton's Second Law of Motion is the proposition obtained by substituting for the general term, *cause*, the particular term, *force*, and for the general term, *effect*, the particular term, *motion generated*; so that, according to Mr. Spencer, this law simply asserts "a definite quantitative relation between a force and the motion generated by that force." But surely the quantitative relation asserted by Newton is not only *definite*, but is further the *special* relation of *proportionality*; so that, if the law is an immediate corollary of an *à priori* assumption, the assumption must be that "the exact quantitative relation between cause and effect is that of direct proportionality," or in more familiar words, that "effects are proportional to their causes." Perhaps this is what Mr. Spencer meant to assert. At any rate let us admit it as a definite basis for reasoning, and endeavour to deduce some consequences from it.

"The cause of a stone falling when left to itself is its weight; but 'the greater the cause, the greater the effect,' therefore the greater the weight of the stone the more quickly will it fall, and thus of two stones let fall from the same height, the heavier will reach the ground sooner than the other." Something of this kind, it may be presumed, was the argument of Aristotle and his followers before the age of Galileo: and how on *à priori* principles is it to be refuted? Of course it is disposed of at once by the simple observation that the same force does not produce the same motion in different masses: but independently of some such observation or experiment, it seems to me impossible to deny that it *may be* true, though even an *à priori* philosopher might show that, as other alternatives are conceivable, it is not *necessarily* true. As a matter of historical fact, Galileo refuted it once for all by the "consciously-made experiment" of letting two different weights fall simultaneously from the leaning tower of Pisa.

But it may be said that the above argument is hardly "*definitely* quantitative." Let us then examine Newton's Second Law of Motion as an "immediate corollary" of our *à priori* assumption. Here the cause is "the motive force impressed," and the effect "the alteration of motion." But then the question arises—how are the quantities of this cause and effect to be measured? Newton carefully defines quantity of motion as proportional to *mass* and *velocity* jointly; that is, he measures it by *momentum*. From another point of view it would have been correct to measure quantity of motion by *kinetic energy* or *vis viva*, that is, as proportional to mass and the *square* of the velocity jointly. Further the "alteration of motion" might be measured either with respect to a given time or to a given space. Newton implies the former, and consequently the explicit statement of his second law is that "the *momentum* generated *in a given time* by an impressed force is proportional to that force." Substitute for this "the momentum generated in moving through a *given space*," or "the *kinetic energy* generated in a given time," and the law becomes untrue. Substitute "the *kinetic energy* generated in moving through a *given space*," and we have a law which is true, but not that which Newton asserted as his second law. Now among these four alternatives how is our *à priori* philosopher to decide? He might perhaps analyse them further and show that some of them are inconsistent with the others, and I believe he might reduce the questions to be decided to still simpler ones; but I fail to see (in common, I believe, with everyone who has thoroughly grasped the fundamental principles of rational mechanics) how, without recourse to consciously-made observations or experiments, he could arrive at a certain conclusion.

May we not say then that these great *à priori* principles, whatever value they may have in a "System of Philosophy," are of little avail in any special science, and that the "axioms" of such science, however much they may involve these principles, are not mere "immediate corollaries" therefrom?

If not intruding too much on your space, I am tempted to apply to Mr. Spencer's great principle of the "Persistence of Force" the same mode of treatment as I have applied above to the principle that "effects are proportional to their causes."

Mr. Spencer distinctly refuses to identify this principle with the great physical principle of the Conservation or the Persistence of Energy, the firm establishment of which undoubtedly marks one of the most important epochs in the history of Science. Force, in Mr. Spencer's use of the term, includes numerous species of which energy is but one. I feel sure that every mathematician and physicist would protest against the inclusion under one term of magnitudes of such different kinds as statical force and energy, or the work done by such a force; but not to dwell on this, I believe that Mr. Spencer would certainly acknowledge as one of his species, that which (in my view) is alone properly termed Force, namely, such as can be measured in terms of the weight of a pound or a gramme. What then does Persistence of Force of this kind mean? Does it mean that the numerical sum of the intensities of all the Actions and Reactions throughout the universe is constant? If so, it is untrue: for, to take a simple illustration, if a weight be supported, first by a single string, and then by two strings not vertical, the tensions are quite different in the two cases, and there is no equivalence between those which disappear, and those which are introduced in passing from one to the other. If not, we must take account of the *directions* of our forces, and then, if it mean anything, it appears to be but the expression of Newton's Third Law that "action and reaction are equal and contrary" in this form:—"The algebraical sum of all the forces throughout the universe is persistently zero." To every mathematician, at any rate, this assertion and the assertion that "the sum of the energies of all kinds throughout the universe remains persistently of the same definite numerical amount" are assertions of facts of such different orders, that to class them together is rather to introduce confusion of thought than to establish a grand general principle.

I have offered the above remarks because it appears to me only fair to the author of the article on Herbert Spencer in the *British Quarterly Review* to show that it is felt by others, who have made a study of the fundamental principles of rational mechanics, that his strictures on Mr. Spencer's treatment of those principles are in all essential points fully justified, however much they may wish that the expression of those strictures had been in some instances modified in its tone.

The Park, Harrow, April 20 Robert B. Hayward

I think it is positively due, not only to the writer of the now famous article in the *British Quarterly Review*, but to Newton's memory and to Science itself, that the correspondence which has been going on should not seem to terminate as a drawn game, at any rate in the opinion of some bystanders, who may from their antecedents be presumed competent to judge.

That Mr. Spencer will ever be convinced is, I suppose, hopeless; I at any rate am not going to try to convince him. But I can assure the *British Quarterly Reviewer* that he has my very deepest sympathy in his argument with an antagonist who is at once so able a master of fence as Mr. Spencer, and yet is so intensely unmathematical, it would seem, as to pass from "exact quantitative relation" to "proportionality;" or as to talk of the effect of a force, without defining how the effect is to be measured, without feeling the slightest difficulty.

Nor does it seem that Mr. Frankland, in Nature, vol. ix., p. 484, is quite justified in his conclusion that the truth lies *between* the two opposite views. And his own view is in fact entirely coincident with the Reviewer's, except, perhaps, on a point which is not relevant to the controversy, viz. how far the experimental proof of the so-called physical axioms is *complete*.

Will it comfort the Reviewer if I tell him some of my own experience? I, too, read Spencer after my degree; and on the first reading of the "First Principles" came to the sad conclusion that I had not understood any mathematics properly; so much fresh light seemed to be thrown on them. I read it again, and more critically, and doubted whether Spencer was quite correct. I read it again, and concluded that he was wrong in his physics and mathematics. I ought to add that I too was, like the Reviewer, A Senior Wrangler

I agree so fully with the chief contents of Mr. Frankland's letter (vol. ix. p. 484), that I wish to call his attention to one point in which his letter seems to me calculated to mislead.

He says, "the pure empiricists argue that because certain observed results coincide with the results of calculation, therefore the assumptions on which the calculation was based must be true. Now without doubt the *demonstrative* character of this inference vanishes entirely under Mr. Spencer's searching criticism. But it seems to me that a *high probability* remains."

Now, in the name of pure empiricists, I must protest against our being supposed to think that anything "must be true" in any other sense than that there is a "high probability" of its truth. I cannot refer to a better exponent of our views on this point than Prof. Clifford, to whom Mr. Frankland himself refers. And the idea of our having to thank Mr. Spencer for showing that the inductive proofs of the laws of motion (or of any other physical truths) are not *demonstrative* in any other sense than the above is quite new to us. What Mr. Spencer has done is to bring up instances of this so-called imperfectness in the demonstration as evidences that no *à posteriori* proof of the proposition can exist, when in point of fact they are specially characteristic of such a proof.

Those of your readers who have examined Mr. Spencer's ingenious proof of the second law of motion, contained in his last letter to Nature (vol. ix. p. 461), will not ascribe my not immediately answering his letter to any difficulty in so doing.

The Author of the Article in the British
Quarterly Review

Lakes with two Outfalls

In Nature, vol. ix., p. 485, Mr. Craig Christie begins a letter "to correct a mistake as to a matter of fact:" "Loch-na-Davie, Arran, has *two* outlets, as is correctly represented in the Ordnance map;" and he ends his letter: "I think Colonel Greenwood ought at least to have made himself acquainted with the Ordnance map."

I take the liberty to enclose to you the new Inch Ordnance map of Arran, to which my letter in vol. ix. p. 441 referred. You will see that as "a matter of fact" the map does *not* give two outlets, but only one.

I need not ask for your valuable space in reference to Mr. Christie's own "matters of fact," since my views with reference to them are printed in the *Athenæum* of July 22, 1865. He will see there that I have not only "walked up the north stream from Loch Ranza," but also by Glen Catacol and Glen Dzeven, and a third time from Corrie by Glen Sannox over the water-parting. Also that I have sounded the whole of this little pool of bog-water by walking it, bare-legged, without being over my knees in the deepest part, which was at the south end, where the only outlet is to Glen-Iorsa.

I shall have the pleasure to communicate with Mr. Thelwall in reference to his obliging letter.

George Greenwood

[The Ordnance map forwarded to us by Colonel Greenwood gives only one outlet to Loch-na-Davie.—Ed.]

As this subject appears to me to possess an interest apart from the issues hitherto in question, I trust you will allow me a little of your space.

From the fact that lakes do not ordinarily occupy the crest of a watershed, it would *à priori* appear more likely that a double outfall, if it exist, should lie in or towards adjacent districts than connected with opposed valley systems. The following instance, which I observed in Norway last summer, is, in view of Colonel Greenwood's letter (Nature, vol. ix. p. 441), worth mentioning. The lake exhibiting it lies about two miles inland (N.W.) from the elevated coast which faces Trondhjem, and is named Stor Lake; its length—nearly parallel to the Trondhjem fjord—is about seven miles, its greatest breadth about two. Like many Norwegian lakes, it presents a *facies* different to what we are most familiar with in Britain. Instead of occupying a single valley-basin, it consists of a chain of minor basins strung along an axis of depression (probably a pre-existing valley), and each separated from its neighbours by the subsided walls of the valley of which it is the cup-like enlargement. The form of Stor Lake is irregular, with long arms or creeks extended (obliquely to its longer axis) into the mouths of the valleys. In such lakes it might be expected now and then that the effluent waters should pass out at more than one of these channels, and in Stor Lake such is the case. One stream is discharged from one of the component basins, nearly at right angles to the lake's greatest length, the other issues along the depression on which I have said the basins are "strung"—bead-like. The former opening is of post-glacial date, and is superseding the original one for several reasons:—(1) it flows along the strike of a homogeneous bed of schist, whereas the other cuts across beds of various textures, and (2) its volume is greater. Its rival bears evident traces of progressive attenuation, and is not marked on any published map.

In this case the streams are nearly at right angles to each other when discharged; another instance, however, seems to be furnished in a neighbouring loch, Gròn Lake, in which they are collateral. Krefting's map (1868) represents the loch as bifurcating at its north-east end, each of the inlets giving rise to a stream; they seem about two miles apart, are marked by lines of about equal thickness, and flow nearly parallel to the Trondjhem fjord near Mosvigen.

I believe that instances of a like nature with these are by no means rare in Norway. I know at least one lake near Trondjhem, which at a former period seems to have had a double outfall, and many others in which, were the existing outlet dammed by a moraine twenty to fifty feet high, the water would find one or several openings elsewhere.

I have indeed noted several instances of lakes with two outfalls upon Prof. Munch's large map of Norway (1845), but failing to discover any confirmation in other maps, and finding it in other respects unreliable upon matters of such detail, I can assign no value to them.

It would be a fact of curious significance, as bearing upon Prof. Ramsay's theory of the glacial origin of lakes, if most authenticated instances of lakes with several outfalls could be referred to districts which have been traversed by a continuous sheet of glacier ice. When glaciers were confined within valley boundaries, as in Britain, their force was of necessity concentrated along lines, but upon level tracts or plateaux they were free to scoop wherever circumstances favoured erosion. Should it prove that Norway, North America, and Lapland give us the majority of lakes with several outfalls, no other theory can explain the fact.

St. James's Park, S.W. HUGH MILLER

Trees "Pierced" by other Trees

COLONEL GREENWOOD'S answer (NATURE, vol. ix. p. 463) to Mr. J. J. Murphy encourages me to mention a botanical phenomenon which I witnessed in 1865, but have scarcely ever mentioned before for fear of being disbelieved. I was standing on the bank of the little river Evenlode, in Oxfordshire, looking at an old pollard willow trunk about six feet high, when I observed in the decayed wood of the tree an upright sort of staff resembling a dark-coloured old school ruler, and of about that size. I knocked away some of the touchwood above and below, and found my ruler lengthened each way. At the point where it would naturally issue at the top, I found a small twig of undoubted ash, of which the leaves were fully expanded, sprouting up among the branches of willow. Upon clearing away a little more rotten wood I laid bare another ruler, which, like the first, appeared to lengthen upward to the top of the trunk and downward to the ground, but there was no second twig of ash above. The "rulers" were rough where they were totally enclosed by the willow, and had put forth little threadlike rootlets. But the part which I found exposed to the air was smoother and looked like a true branch, but was darker than the usual colour of ash. I afterwards drew the proprietor's attention to the tree, but he could not suggest any explanation. I daresay it is there and in the same condition to this day; if anyone wished it, I could easily describe where it might be found. One explanation I have had offered is, that an ash-seed had fallen down a deep crack in the willow. But there was no sign of such a crack—no crack-like cavity—one of the "rulers" being totally and closely enveloped with the rotten wood, and the other very nearly so. Whether it would have been possible for an ash-seed to germinate in a crack which must have been at least four feet deep and probably much deeper, and was open at the top only and was certainly no larger than the shoot which it formed, is a question I must leave to botanists. Another explanation was, that as ash-roots travel for a considerable distance underground, it was possible that two such roots, finding suitable pabulum in the rotten trunk of the willow, had turned upwards. But this also I must leave to men of science, and notably to Col. Greenwood. T. S.

PROF. TAIT ON "CRAM"

ON Wednesday, the 22nd inst., at the ceremony of capping the Graduates in Arts of Edinburgh University, Prof. Tait gave an address in which he touched on various subjects of Academical interest. On the subject of "Cram" he spoke as follows:—

"It is a mere common-place to say that examination, or, as I have elsewhere called it, artificial selection is, as too often conducted, about the most imperfect of human institutions; and that in too many cases it is not only misleading, but directly destructive, especially when proper precautions are not taken to annihilate absolutely the chances of a candidate who is merely crammed, not in any sense educated. Not long ago I saw an advertisement to the effect:—'History in an hour, by a Cambridge *Coach.*' How much must this author have thought of the ability of the examiners before whom his readers were to appear? There is one, but so far as I can see, only one, way of entirely extirpating cram as a system, it may be costly—well, let the candidates bear the expense, if the country (which will be ultimately the gainer) should refuse. Take your candidates, when fully primed for examination, and send them off to sea—without books, without even pen and ink; attend assiduously to their physical health, but let their minds lie fallow. Continue this treatment for a few months, and then turn them suddenly into the Examination Hall. Even six months would not be wasted in such a process if it really enabled us to cure the grand inherent defect of all modern examinations. It is amusing to think what an outcry would be everywhere raised if there were a possibility of such a scheme being actually tried—say in Civil Service Examinations. But the certainty of such an outcry, under the conditions supposed, is of itself a complete proof of the utter abomination of the cramming system. I shall probably be told, by upholders of the present methods, that I know nothing about them, that I am prejudiced, bigoted, and what not. That, of course, is the natural cry of those whose 'craft is in danger'—and it is preserved for all time in the historic words, 'Thou wert altogether born in sin, and dost thou teach us?' I venture now to state, without the least fear of contradiction, a proposition which (whether new or not) I consider to be of inestimable value to the country at large:—Wherever the examiners are not in great part the teachers also, there will cram to a great extent supersede education. I need make no comment on this, beyond calling your particular attention to the definite article which twice occurs in the sentence, and which gives it its peculiar value.

"I said, in my former address [eight years ago], that 'coaching' seems quite natural to all who are engaged in it, and, in particular, that it did so to myself more than twenty years ago. This shows that it is possible that something akin to the results of the profound speculations of Riemann, Helmholtz, and others, may hold in the moral if not in the physical universe. It is probably new to most of my audience to hear that very great authorities are as yet in doubt whether the properties of space itself are the same in different localities; whether, in short, in our rapid flight through space, we may not be insensibly getting into a region, our existence in which will involve a gradual change of form, in order that our physical substance may continue to fit the varying circumstances of our position. Assume that something like this holds in the world of mind, and you see at once how the same man may, while residing in Edinburgh, honestly denounce certain methods as wholly pernicious which a few years' residence in Cambridge may invest in his eyes with a perfection more than human. I do not say that this is an explanation; but the analogy is at least worthy of remark; and I leave further discussion of it to my old friend Mr. Todhunter, who, living in the middle of that singular region, tells me he thoroughly agrees with me in my main arguments against examinations, and then soundly rates me for my mode of propounding them."

After advocating the restoration of the B.A. degree to Edinburgh University, Prof. Tait spoke in forcible terms against the centralisation of our various Universities, Licensing Boards, &c, "with its inevitable acolyte cram." He illustrated in an original and striking way what he thinks would be the inevitable result of centralisation, by referring to the dead and motionless uniformity which must be the result of the degradation of energy. Prof. Tait drew a ludicrous yet melancholy picture of what would be the results of universal uniformity in the social world.

"The application of these ideas," he said, "to political and social questions, among which of course comes University centralisation, is not far to seek. What would the world of men be without what we may call 'social entropy'? Everyone would then be his own farmer, baker, butcher, brewer, banker, boot-black, &c.—all would be at the same dead level—no possible help from one to his neighbour, even if it could be required; no distribution of tasks, and therefore (in every department) that endless waste which is inevitable in operations conducted on a petty scale. No possibility of that mutual reliance and assistance which forms the friendships we delight in, none of that variety which is the real charm of life—no idea which would not simultaneously strike every unit of the race—no news, no books—nothing but sameness! None of the pleasure of being able to assist struggling worth, none of gratitude for generous aid. Nay, we might pursue it further. No difference of temper, character, tendencies, age, sex—a state lower than the lowest known in vegetation; but here the end must come. Or, to take a somewhat different point of view (though the basis is absolutely the same, for oscillation implies entropy), what if everything were always at its average value? Never absolutely either fair or rainy weather, clear or cloudy, calm or stormy, hot or cold; but a dead average. Never either absolutely day or night; no tides, no seasons; men never either absolutely awake nor absolutely asleep—continually in a semi-lethargic state—half happy, half discontented; half playful, half serious—neither running, walking, standing, sitting, nor lying, but a perpetual average. No catastrophes such as a birth, a marriage, or a death—no distinction between man and man—nothing of that variety which is the law of nature. Eternal, hideous, intolerable sameness, by necessity devoid of all capacity for action: the human race turned into a set of Nurnberg toy-solders, all cast in the same mould, of the same base material, and all similarly bedaubed from the same glaring paint-pots, and moving on the same lazy-tongs with the same relative velocities. No one to advise you in a difficulty, no one in whose superior strength of mind or body you could confide; nothing around you except what you feel must be but the image of yourself (as you will early have learned introspectively to look at it)—mean, sordid, and grovelling! No one whom you can respect, none to trust—all, like yourself, vile and despicable! Here I would gladly say—'Enough of such horrors,' and quit the disgusting theme. But, unfortunately, the application has still to come. It will be found very pertinent to many things which have been of late evolved from the innermost consciousness of statecraft, and hailed, with altogether inexplicable delight, by what seemed (till lately) to be at least a numerical majority of the representatives of our countrymen."

Prof. Tait then referred to the late Prof. Forbes and the recent discussion concerning his character and work. For what Prof. Tait has to say on this subject we must refer our readers to his address, which is printed in full in the *Scotsman* of the 23rd inst. He then spoke of the scheme for extending Edinburgh University, and the facilities which would thereby be acquired for teaching Science practically, as it ought to be taught, and thus tend to extinguish "paper-science," a term which "conveys to all who are really scientific men an impression of the most unutterable contempt." In conclusion, Prof. Tait referred to the difficulties attending the work of his own class, that of "Natural Philosophy," arising from the want of adequate means. He hopes to be able, at least, to put the Natural Philosophy Department in Edinburgh University on a proper footing for his successor.

THE SOIRÉE OF THE ROYAL SOCIETY

ON Wednesday, April 22, the first soirée of the Royal Society since their removal into their new apartments was given by the President, Dr. Hooker, and came off with the greatest *éclat*. There was a remarkably good display of scientific apparatus, and we think that the interdependence of the man of Science and of the manufacturer of instruments is at no time better exemplified than on occasions like the present. The apartments devoted to the purposes of exhibition were thronged by the most eminent in the various branches of Science—it might have been said with reason that a considerable fraction of the nation's mind had centred for the time being in the rooms at Burlington House. Not as an unhealthy sign either did we regard the presence of Archbishop Manning and the attention shown towards that divine by scientific men of the very opposite poles of thought.

Of the objects of interest displayed in the six rooms devoted to this purpose we can here only give details of the more prominent. In the first room several maps and photographs were exhibited by the Royal Geographical Society; also some splendid pieces of glass-work by Messrs. Chance, consisting of a dioptric fixed light (4th order) with nine prisms and six rings of lenses in four panels, a segment of a dioptric totally reflecting mirror first proposed by Mr. Thomas Stevenson, C.E., a dioptric holophote designed by the same engineer, and a lamp-burner designed by Mr. J. N. Douglass, C.E., with six concentric wicks. This burner can be used either for colza oil or for petroleum. The President exhibited also in this room some interesting objects from the Kew Museum. Amongst these we noticed some fossil copal gums from Zanzibar, carved cocoa-nut shells from the Fiji Islands, a vase made from the ash of *Moquilea utilis*, mixed with clay, from Parà, and different chemical and medical products from species of *Eucalyptus*.

In the second room Mr. Crookes exhibited his experiments showing the attraction and repulsion accompanying radiation. The pendulum described by Mr. Crookes in his communication to the Society was exhibited under various forms, and the experiments excited the liveliest interest. Here also Dr. C. J. B. Williams exhibited some new ear-trumpets, and Messrs. Whitehouse and Latimer Clark an electrical recorder for registering time, speed, distance, and number of passengers inside and out in tram-cars and omnibuses. This information is registered in four parallel columns in red ink on long strips of paper, by automatic pens.—Mr. Vernon Heath exhibited some autotype landscapes, and the president some Tappa dresses from Fiji, which reminded us strongly of the ornaments placed in our fire-stoves during the summer. Here also we were shown a microscope by Messrs. Powell and Lealand, with a $\frac{1}{50}$ immersion objective and the eternal Podura scale.

In the third room, the Entrance Saloon, were some exquisitely coloured drawings of the flora of Brazil, and landscapes by Miss North; likewise some coloured drawings of New Zealand birds, exhibited by Dr. W. Lawry Buller. The pair of new Paradise Birds collected by Signor D'Abertis* in New Guinea, promised by Dr. Sclater, was not exhibited.

In the fourth room, the Reading Room, Dr. Tyndall exhibited the apparatus (already described in our columns) for showing the stoppage of sound by a non-homogeneous mixture of air and vapours, and also experiments illustrating Savart's observations on the action of sound on a jet of water. Dr. J. H. Gladstone exhibited some photographs of fluorescent substances. Bottles containing fluorescent liquids, such as æsculin or quinine di-sulphate, appear in the photographs nearly as black as a bottle filled with ink; similarly, labels written with such liquids, although the characters are ordinarily invisible to the eye, show up their designs when photographed. In this room were to be seen also photographs of the Naples Aquarium, exhibited by Mr. W. A. Lloyd, and one of Dr. Dohrn's Zoological Station at Naples, lent by Mr. Darwin; likewise some lithographed plates of recent Foraminifera from the Abrolhos Bank, exhibited by Profs. W. K. Parker and Rupert Jones. Mr. J. Norman Lockyer exhibited a series of photographs of metallic and solar spectra enlarged by Messrs. Negretti and Zambra from photographs taken by his new method of comparing spectra by means of a perforated shutter sliding in front of the slit of the spectroscope. In this room the new sextant devised by Capt. J. E. Davis was exhibited. This instrument, which will be found particularly useful in night observations, permits the taking of a series of observations without reading off each observation; this being accomplished by the adaptation of a micrometer movement to the tangent-screw, and the application of indicators to the arc of the instrument. Mr. Alfred Tribe here exhibited some specimens of metals (palladium, copper, &c.) which had become agglomerated in a most remarkable manner by hydrogenisation; under ordinary circumstances the metals shown existed in the form of fine powders, but, as soon as charged with hydrogen, become agglomerated.

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* See NATURE, vol. viii. p. 305.

The fifth room, or Principal Library, is by far the largest apartment of the suite. Mr. C. V. Walker's electrical apparatus for carrying out the "block system," or "space intervals," between trains on the South-Eastern Railway, was here displayed. Messrs. Tisley and Spiller exhibited their compound pendulum apparatus in action, and distributed cards with the exquisite curves described upon them. This firm exhibited also the beautiful triple combination double-image prism belonging to Mr. Spottiswoode. Mr. E. B. Tylor's ingenious apparatus for illustrating refraction (already described in these columns) was exhibited in this room.* We observed also some splendid gold crystals exhibited by Mr. W. C. Roberts, Chemist to the Mint; Mr. W. H. Barlow's "Logograph," a recording instrument for showing the pneumatic action accompanying the exercise of the human voice; and a pair of gyrostats exhibited by Prof. Sir William Thomson. Messrs. Negretti and Zambra exhibited their ingenious thermometer for recording deep-sea and atmospheric temperatures, already described in NATURE. Mr. John Browning exhibited a good collection of apparatus. Mr. G. P. Bidder's micrometer, a most ingenious device for observing the transit of very faint stars, in which the spider lines, capable of the usual micrometer movements, are illuminated by a side light, and are reflected into the eye-piece by a mirror, thus appearing bright upon a dark ground, and by interposing coloured glasses between the lamp and the spider lines can be coloured at pleasure. Sir Charles Wheatstone's new photometer is well worthy of notice: the screen slides along the divided scale and its motion causes the increased overlapping of two sliding wedges of neutral-tint glass. The light is looked at directly through a hole in the screen, and the latter moved along the scale till the light just ceases to be visible. We noticed also a micro-spectroscope of very good definition, showing the absorption spectrum of cantharides. Mr. Apps exhibited a model and diagram of a fireproof building, and a model of an improved apparatus for indicating the speed of revolving shafts, both being the inventions of Sir David Salomons. The plan for rendering buildings fireproof consists in laying on water-pipes between the walls and floors of the building, these pipes being self-acting by means of fusible metal plugs or electrical communications. The last-named model is an application of the ordinary governor balls, which are connected with the shaft, and by a system of levers, with an index, which moves up a graduated scale. A double-action spectroscope with a divided object-glass, made by Grubb, of Dublin, was shown and explained by Lord Lindsay; this instrument is intended by its owner to be attached to a large equatorial for the observation of stellar spectra. Among other noticeable things in this room we may mention the Megohm, one million British Association units, by Messrs. Elliott Brothers; Mr. George Barnard's highly artistic water-colour drawings and the copies of sacred Icons of the Greek Church in Russia, and photographs by Mr. John Leighton. Col. Stuart Wortley's photographs from life are high examples of art, and the group of living corals (*Astroides calicula*) from the Bay of Naples, exhibited by the Crystal Palace Aquarium Company, attracted large numbers of admirers by their beauty. At 10 o'clock Dr. R. Norris, of Birmingham, exhibited in the meeting-room experiments to illustrate a form of contractive energy which displays itself in various substances. Among other things the Doctor showed that the statement that india-rubber *contracts* by heat is incorrect; this substance, it is true, contracts in the direction of its length, but it expands in breadth at the same time, thus resembling the so-called contraction of muscular fibre.

In soirées of this kind experiments illustrative of new chemical discoveries are generally "conspicuous by their absence." This surely cannot be due to the fact that the science does not permit of public demonstration; it arises rather from the "messy" nature of the materials employed by chemists, thus precluding the introduction of chemicals into such rooms as are devoted by the Society to their gatherings. We are of opinion that in not fitting up and adding to their now noble apartments a laboratory, an omission has been made which may be regretted in the future.

THE LECTURES AT THE ZOOLOGICAL SOCIETY'S GARDENS

II.

IN the second and third of his lectures On the Geographical Distribution of the Mammalia, delivered on the Tuesday and Friday of last week, Mr. Sclater described in detail the ranges of the different orders of terrestrial mammals; and to avoid unnecessary repetition, employed the well-known system of division of the earth's surface, proposed before the Linnean Society in 1857, from a study of the bird class, according to which there are six regions—(1) The *Palæarctic*, including Europe, Africa north of the Atlas Mountains, and Northern Asia. (2) The *Ethiopian*, including all Africa south of the Atlas Mountains, and the southern part of Arabia. (3) The *Indian*, including Asia south of the Himalayas, Southern China, and the Indian Archipelago. (4) The *Australian*, including Australasia. (5) The *Nearctic*, including North America down to the centre of Mexico; and (6) The *Neotropical*, including South and Central America. The following is a summary of his remarks.

Among the monkeys the anthropoid apes inhabit equatorial Africa, where the gorilla and chimpanzee are found; Sumatra and Borneo are the home of the orang outang; while the eastern portion of India, Burmah, and the Indian Archipelago constitute the habitat of the various species of gibbon. The catarrhine monkeys, including the green monkeys (*Cercopitheci*), and the macaques inhabit Africa and India respectively; the latter, however, extending into Africa north of the Sahara, as far as Apes Hill and the Rock of Gibraltar. The platyrrhine monkeys, among which are the spider monkeys, the howlers, and the marmosets, are found in the Neotropical region, except in its southern and western parts. The lemurs are mostly confined to the island of Madagascar, some few inhabiting Eastern India, and two forms occurring in Western Africa.

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* We should recommend lecturers using this apparatus to see that the wood is well seasoned; the one exhibited soon ceased to act satisfactorily, owing to the warping of the board.

Among the large order of the Carnivora the lion is a denizen of the forests of the Ethiopian region, and spreads slightly beyond it into India. The tiger is found in the Indian region, and spreads up into China and Central Asia, where its coat becomes coarser in texture. The leopard is distributed over the districts of the lion and tiger; it is also found in Borneo and Ceylon, whilst the lynx occurs in the Neartic and Palæarctic regions. The dogs are cosmopolitan, though it is doubtful whether the single form of Australia has not been introduced by man in early times. The bears inhabit the Palæarctic, the Nearctic, and the Indian regions, being also found in the Andes of Peru.

Among the odd-toed, or Perissodactylate Ungulates, the horses and asses are strictly Old-World forms, the exact place of origin of the former being uncertain. The asses are spread over the Indian and Ethiopian regions. The tapir is very aberrant in its distribution, one species appearing only in Sumatra and the Malay Peninsula, while in the northern portion of South America and Central America three others occur. The rhinoceroses are from the Indian and Ethiopian regions only, the Asiatic species all being now or having lately been exhibited in the Zoological Gardens. Those from Africa are less perfectly known, only two species having been accurately determined.

Among the even-toed, or Artiodactylate Ungulates, the camels are very peculiar in their habitats, the Llamas of the Andes and the camels of Africa, Arabia, and part of Russian Asia being the only known forms; those from the last-named locality being the only known wild true camels of the present day. The giraffe is purely Ethiopian. The bison in North America represents the oxen of the Indian region, which in Africa and Arabia are in great measure replaced by the antelopes, so varied in form and size. The Cervidæ are not found in the Ethiopian nor Australian regions. The hippopotamus inhabits all the large rivers of Africa, the smaller species being found in and about Liberia. Of the Swine-family the peccaries are the representatives in the Neotropical region, whilst the quaint Wart-Hog and Red River Hog are exclusively African.

The hyrax, or coney of Scripture, whose zoological position is so uncertain, is found in Arabia and parts of Africa only.

There are only two species of elephant known, the Indian being from the Indian region, and the African from the Ethiopian. In very recent times they abounded in Siberia, and earlier still in many other parts of the world.

The Neotropical region abounds in peculiar Edentate animals, as the armadillos, sloths, and ant-eaters. The scaly ant eaters or Pangolins, and the ant-bears or Orycteropus, are found, the former in India and Africa, the latter in Africa only.

Among the Insectivora, the peculiar Solenodon inhabits St. Domingo; the gilded mole, South Africa; the Tenrec, Madagascar; and the Tupaias, the Malay districts.

Among the Rodentia the porcupines, divided into two well-distinguished sub-families, inhabit, one the Old and the other the New World. The Neotropical region, however, is the head-quarters of the *Hystricidæ*; the capybara, together with the agoutis, and numerous other forms being from that locality. There are also found the chinchilla and viscacha. The beaver abounds in the Nearctic region, and used to do so in Europe, till the increase of population has almost exterminated it. The hare and rabbits have a wider distribution, as have also the squirrels.

It will be noticed that Australia has been scarcely mentioned in the above remarks, and that the dog which is spoken of in connection with it is not known certainly to be indigenous. This is because the mammalian fauna is almost entirely represented by animals of the Marsupial order, the kangaroos, bandicoots, phalangers, wombat, koala, thylacine, and dasyures being peculiar to it and Van Dieman's land. Among Marsupialia the group of opossums is only found in the Neotropical region, extending quite through Mexico into the United States.

The Monotremata, including only the duck-bill or ornithorhynchus and the echidna, are confined to New South Wales and Tasmania.

(*To be continued.*)

THE FLUCTUATIONS OF THE AMERICAN LAKES AND THE DEVELOPMENT OF SUN-SPOTS

In the course of an investigation, undertaken in my capacity as Geologist to the B.N.A. Boundary Commission, as to late changes of level in the Lake of the Woods, bearing on the accuracy of certain former surveys, I found it desirable to tabulate the better-known fluctuations of the great lakes for a series of years as a term of comparison. The observations of secular change in Lake Erie are the most complete, and these, when plotted out to scale, showed a series of well-marked undulations which suggested the possibility of a connection with the eleven-yearly period of sun-spot maxima. A comparison with Mr. Carrington's diagram of the latter confirmed this idea, and as I do not remember to have seen these phenomena connected previously, I have been induced to draw out the reduction of both curves here presented, and the table of the height of water in the lakes.

The changes of level affecting the great lakes are classed as follows by Colonel Whittlesey, who has given much attention to the subject:—

1. General rise and fall, extending through a period of many years, which may be called the "Secular Variation."
2. Annual rise and fall within certain limits, the period of which is completed in about twelve months.
3. A sudden, frequent, but irregular movement varying from a few inches to several feet. This is of two kinds, one due to obvious causes, such as winds and storms; another, described as a slow pendulum-like oscillation, has been somewhat fully discussed by Whittlesey in a paper read before the American Association at its last meeting, and is due probably to barometric changes in the superincumbent atmosphere.

The first class is the only one directly included in the present inquiry.

I.—*Table of Great Lakes.*—In Mr. Lockyer's new work on Solar Physics, chap. xxvi., entitled "The Meteorology of the Future," exhibits the parallelism of periods of solar energy, as denoted by the outburst of sun-spots, with the maximum periods of rainfall and cyclones, and for the southern hemisphere, by a discussion of his own and Mr. Meldrum's results. In the table (p. 505) I have arranged the more accurate numerical observations of the height of the lakes from registers kept for the last few years, in a method similar to that there adopted.

Prof. Kingston's observations of Lake Ontario were taken at Toronto, and measured upward from an arbitrary mark. They extend from the year 1854 to 1869, and include the minimum periods of 1856 and 1867, and the maximum of 1860. Taking the mean annual level for each minimum and maximum epochal year, and one year on each side of it, as is done by Mr. Meldrum, and deducing a mean from each of three tri-yearly periods, the agreement is close between the solar periods and those of fluctuation in the lakes.

Table showing the height of water in the American Great Lakes, measured upward from an arbitrary mark, for the years surrounding periods of maxima and minima in the occurrence of Sun-spots.

Sun-spot Periods.	Year.	Lake Ontario, from Prof Kingston's observations		Lake Ontario, from U S. Lake Survey Observations.		Lake Superior, from U S Lake Survey Observations		Lake Michigan, from U.S. Lake Survey Observations.		Lake Erie, from U S Lake Survey Observations.	
		Yearly means.	Tri-yearly means	Yearly means.	Tri-yearly means	Yearly means.	Tri-yearly means.	Yearly means.	Tri-yearly means.	Yearly means.	Tri-yearly means.
Min	1855 1856 1857	17 8 20 6 27 5	21·7								
Max.	1859 1860 1861	28 6 18 3 27·4	24 7	30·48 22 68 33·12	28·80	40 32 30·24 28·26	33 12	29·88 23 76 23·88	25·92	33 60 28 32 29 76	30·60
Min.	1866 1867 1868	9 3 19·7 4 6	11·2	18·36 28·86 13 68	19·20	18·12 20 16 18·24	18·84	3 72 7 68 4 32	5·28	17 76 18 00 13·80	16·56

Mean of maximum periods in Lakes Ontario, Superior, Michigan, and Erie, from U.S. Lake Survey Obs. 29·61
Mean of minimum " " " " " " " 14 97
The measurements are in inches and decimals. Difference 14 64

The remaining observations are those of the U.S. Lake Survey, and include only one period each of maximum and minimum in solar spots. The measurements of the U.S. Survey are reckoned *downwards* from a mark representing the high water of 1838 in each of the lakes, but in the table here given they have been reduced so as to read upwards from an arbitrary line chosen 4 feet below that datum. They are thus rendered more intelligible and made to agree in sense with Prof. Kingston's measurements.

The result is the same in each of the lakes, only differing in amount by a few inches. A mean deduced from the U.S. Lake Survey observations in Lakes Superior, Michigan, Erie, and Ontario, gives a difference between the years surrounding the maximum of 1860 and the minimum of 1867 of 14·64 inches in favour of the former.

2. *Diagram of Curves.*—The curve representing the fluctuation of Lake Erie from 1788 to 1857 inclusive is constructed on a careful discussion of the evidence collected by Col. Whittlesey and given by him most fully in the "Smithsonian Contributions to Knowledge" for 1860.

From 1788 to 1814 there are no accurate measurements to any well-recognised datum line, and I therefore give below the measurements and approximations on which the general curve for these years has been constructed. The description of the fluctuation of the lake will be seen in many cases to apply with verbal accuracy to the sun-spot curve.

"1788—1790. By tradition derived from the early settlers very high; according to some as high as 1838, but this is doubtful.

"1796. By the first emigrants and surveyors reported as very low—5 feet below 1838.

"1797. Rising rapidly.

"1798. Water continues to rise, but 3 feet below June 1838.

"1800. Very high; old roads flooded.

"1801. Still high.

"1802. Very low; reported by old settlers as lower than 1797.

"1806. Very low; reported by old settlers as lower than 1801—2, and declining regularly to 1809—10 when it reached a level by many considered as low as that of 1819.

"1811. Rise of 6 inches in the spring over 1810, by measurement, and a fall of 2 inches.

"1812. Rise of 14 inches in spring over 1810, by measurement, and a fall of 3 inches.

"1813. Rise of 2 feet 2 inches in spring over 1810 by measurement.

"1814. Rise of 2 feet 6 inches in spring above general level of 1813."

From 1815 to 1833, both inclusive, occasional measurement to fixed data exist; the supplementary notes are here given.

"1815. Rise of 3 feet above average level of 1814. (This statement is not confirmed by an actual measurement made in August, and is probably exaggerated).

"1816. Water still high, but falling, and continued to fall till 1819.

"1819. Lowest well-ascertained level of the waters in Lake Erie.

"1820. Stated to be in August as low as 1819.

"1821. Rising.

"1822. Rising; in the spring 4 feet below June 1838.

"1823. Rising; in the spring 3 feet 3 inches below 1838.

"1824. Rising gradually.

"1825. Rising; lowest level 3 feet below June 1838.

"1826. Rising; lowest level 2 feet 10 inches below June 1838.

"1827. About the general level of 1815.

"1829. Water still rising.

"1830. General level same as 1828.

"1831. Lower than last year; yearly change at least 3 feet.—Col. Whiting. (Probably an error as this would place the water unprecedentedly low. Col. Whiting probably ascertained that the lake was falling and erred in taking some former high-water mark for that of the preceding year).

"1832. General average 2 feet 10 inches below June 1838.

"1833. General average 3 feet 2 inches below June 1838."

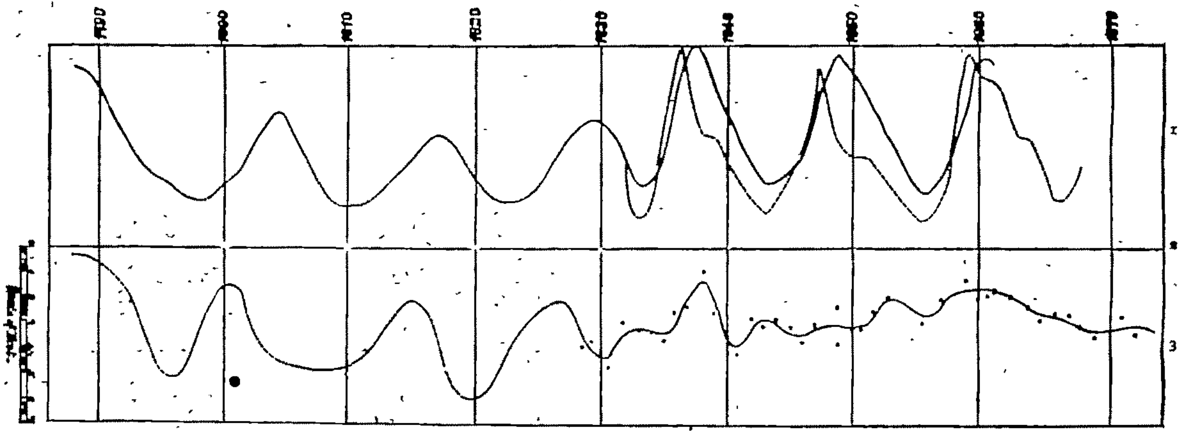
From this date to 1857 many actual measurements are given by Whittlesey, and from these the curve for those years has been constructed. The whole of the observations are reduced as nearly as possible to the average level for each year by comparison with a mean annual curve for about 10 years constructed from monthly averages of bi-five-day means given by the U.S. Lake Survey. 1859 to 1869 both inclusive are from yearly means derived from continuous observations at Cleveland by the U.S. Survey. 1871 to 1873 are from information kindly furnished by Gen. Comstock, Director of the Lake Survey. I have no data for 1870.

The earlier and less systematic observers of the fluctuations of the lakes would scarcely give attention to any but the more important changes of level, and it is possible that these in many cases may have been exaggerated in amount. It would seem improbable, however, from the number of observations which have come down to us, that any variations of importance have escaped notice.

In the upper part of the diagram, the unbroken line represents Carrington's curve founded on the number of sun-spots. The broken line is a reduction of a mean curve based on the area of the spots given by De la Rue, Stewart, and Loewy in the Philosophical Transactions for 1870; and is introduced as showing the solar periods to a later date.

3. *General Remarks.*—The first four maxima of sun-spots represented in the table being separated by long intervals of years with few spots, and not being very intense, would appear to have been closely followed by L. Erie. More especially 1837, the year of greatest known intensity according to both spot curves (333 new groups of spots according to Schwabe), was marked in its effects on the lakes, giving rise in 1838 to the highest recorded level of the waters in Erie and Ontario, and probably also in Superior, though here the data are not so certain. The high-water mark of 1838 has since been employed as the datum to which all the measurements of the Lake Survey are reduced.

The three last periods of maxima of sun-spots are extreme, and the intervals characterised by their deficiency so short that the lakes seem to have been unable to follow them as closely as before. One period of high water being to a great extent merged in the next, and resulting in a general high state of the lakes for the last thirty years, which may be connected with the Wolfian Cycle of fifty-six years in the development of sun-spots. The lakes do not seem to have responded to the maximum of 1848, but by a reference to the curve of area of sun-spots, it will be seen that the intensity of this period was not so great as of those on either side of it, and the period of maximum was maintained for a very short time only. The important sun-spot maximum of 1859-60 was evident in its effect on the lakes even at their present general high level. With regard to the Lake of the Woods the data are slight, but it may be mentioned that this lake is known to have been very low in 1823, and in 1859 to have attained a point which it has never touched since, and which is about 3 feet higher than the present level. The lake is also known to have been for a good many years higher than usual, and at least one well-marked high water took place between 1823 and 1859, which may very probably have been synchronous with that of 1838 on the great lakes. This lake derives its water from the western slope of the same Laurentian range which feeds Lake Superior.



Comparative Diagram of the Fluctuations of Lake Erie, and Periods of greater or less Solar Activity as indicated by the occurrence of Sun-spots. 1 Solar Spot Curves 2. High Water, June 1838 3. Lake Erie.

The correspondence between the periods of maxima and minima in solar-spot cycles and in the fluctuation of the great lakes, though by no means absolute, seems to be sufficiently close to open a very interesting field of inquiry, and to show the extension of the meteorological cycle already deduced by Messrs. Meldrum and Lockyer for oceanic areas in the southern hemisphere, to continental ones in the northern.

The great lakes in their changes of mean yearly level probably show a very correct average of the rainfall over a large area, and thus indicate the relative amount of evaporation taking place in different seasons. It is to be observed, however, that the actual mean annual outflow of the lakes would be a better criterion, and that from the form of the river valleys giving exit to the waters, this must necessarily increase in a much greater ratio than the measured change of level in the lake itself. It is much to be desired that such observations should be systematically made. The occurrence of seasons of great activity of evaporation and precipitation, as indicated by the lakes synchronously with those of maximum in solar-spot production, would tend to confirm the opinions previously formed as to the coincidence of the latter with periods of greater solar activity. Wolf, as quoted by Chambers, states from an examination of the Chronicles of Zurich, "that years rich in solar spots are in general drier and more fruitful than those of an opposite character, while the latter are wetter and stormier than the former." Gautier, from a more extended series of observations, including both Europe and America, has deduced an exactly opposite conclusion, which, from the evidence of the great lakes, would appear to be the correct one.

It is quite possible, however, that both may be true (see "Solar Physics," p. 430). The great lakes lying at the base of the Laurentides, where moisture-bearing winds from the southward and westward are interrupted in their course, and meet with cold currents journeying over these hills from the north, are essentially in an area of precipitation, and greater precipitation would here be the natural result of greater solar energy. In other regions excessive evaporation may result from the same cause, and this may account for the gradual desiccation which on the authority of many observers is going on at present over great areas of the inland plains of the west.

The observations here given cannot be accepted as conclusive, but derive additional importance from the large area which they represent, and may suggest more systematic investigation of the subject, and the accumulation of accurate observations, which in the course of years may lead to results of greater value.

G. M. DAWSON

POLARISATION OF LIGHT*

VIII.

A QUARTZ plate cut parallel to the axis, when examined with convergent light, gives curves in the form of hyperbolas. These curves are wider in proportion to the thinness of the plate, but if the plate be thick enough to render the curves moderately fine, the colour becomes very faint. They may, however, be rendered distinct by using homogeneous light. The dark and light parts exchange positions when the analyser is turned through 90°. Two such plates with their axes at right angles to one another give coloured hyperbolas perfectly visible with the white light. Plates of Iceland spar exhibit similar phenomena, but the lines and curves are far more closely packed.

If the plate be cut in a direction inclined at 45° (or at any angle differing considerably from 0° or 90°) to the axis, the curves are approximately straight lines perpendicular to the principal section of the plate. Two such plates placed with their principal planes at right angles to one another give straight lines bisecting the angle between the principal planes. On this principle Savart constructed the polariscope which bears his name. It consists of two such plates and an analyser, and forms a very delicate test of the presence of polarisation. The lines are, of course, always in the direction described above, and the delicacy of the test increases in proportion as their direction becomes more and more nearly perpendicular to the original plane of vibration.

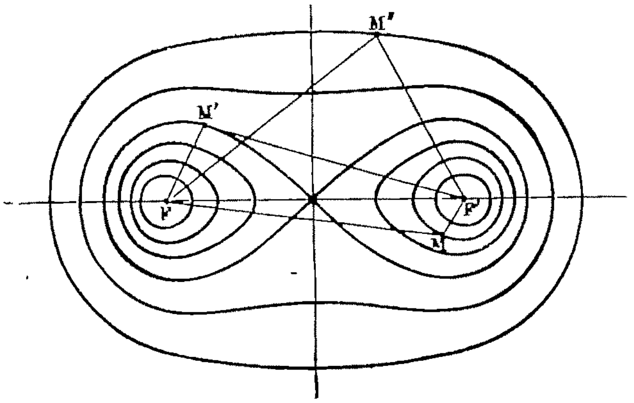


Fig 26.

Bi-axal crystals exhibit a more complicated system of rings and crosses, or brushes as they may in this case be better termed. If such a crystal be cut in a direction perpendicular to the line which bisects the angle between the two optic axes (or the middle line, as it is called), the extremity of each of the axes will be surrounded with rings similar to those described in the case of the uni-axal crystals. The larger rings, however, are not strictly circles, but are distorted and drawn out towards one another; those which are larger still meet at a point midway between the centres, and form a figure of 8, or lemniscata; beyond this they form curves less and less compressed towards the crossing point, and approximate more and more nearly to an oval (see Fig. 26).

The vibrations of the two rays emerging from any point of a bi-axal crystal are as follows:—Of the two rays produced by the double refraction of a bi-axal crystal neither follows the ordinary law of refraction; but one does so more nearly than the other, and is on that account called for convenience the ordinary ray. And if through any point of the field of view we draw two lines to the points where the optic axes emerge, the directions of vibration of the two rays will be those of the bisectors of the angles made by the two lines. If, therefore, the crystal be so placed that the line joining the extremities of the two axes coincides with the plane of vibration of either polariser or analyser, it is not difficult to see that there will be a black cross passing through the centre of the field, with one pair of arms in the line joining the extremities of the axes and the other pair at right angles to it. But if the plate be turned in its own plane round the central point, the points, for which the vibrations are parallel or perpendicular to those of the polariser or analyser, will no longer lie in straight lines passing through the centre, but will form two branches of a hyperbolic curve, passing respectively through the extremities of the optic axes.

If the analyser be turned round, the dark hyperbolic brushes, or the black cross, will undergo the changes analogous to those shown in the cross in the case of uni-axal crystals; but the most interesting effects are those seen when the polariser and analyser are crossed, and the crystal is turned in its own plane.

The angle between the optic axes in different kinds of crystals varies very much; in those where the angle is small it is easy to exhibit both at once in the field of view, but in others where the angle is large it is necessary to tilt the crystal so as to bring the two successively into view. In the latter case the crystal is sometimes cut in a direction perpendicular to one of the axes. The rings are then nearly circular, especially towards the centre, and in that respect they resemble those of a uni-axal crystal; but the character of the specimen can never be mistaken because the rings are intersected by a black bar, or two arms in the same straight line, instead of by four arms at right angles to one another, as would have been the case if the crystal had been uni-axal. The following are the angles made by the optic waves in a few crystals:—

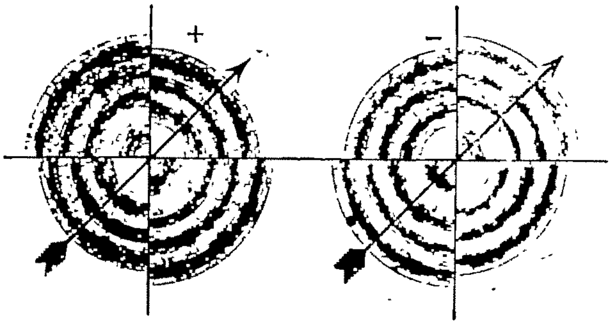


Fig 27.

	°	′
Carbonate of lead	5	15
Saltpetre	5	20
Talc	7	24
Titanite	30	0
Borax	28	42
Mica	30 to 37	0
Carbonate of Ammonia	43	24
Topaz of Brazil	49 to 50	0
Sugar	50	0
Gypsum	57	30
Felspar	64	0
Topaz of Aberdeen	65	0
Oxyde of Lead	70	25
Cyanite	81	48
Chrysolite	87	56

These angles are determined by placing the crystal for an examination into an apparatus adapted to show the rings, and attaching it to an arm whereby the plate can be turned about an axis in its own plane. The axis is furnished with a circle divided into degrees and seconds, and an index. If this axis be horizontal, the plate is so placed that the line joining the centres of the two systems of rings is vertical, and the crystal is first turned so as to bring one centre into the centre of the field of view (usually marked by cross wires); the index is then read, and the crystal turned so as to bring the centre of the second system of rings to the centre of the field. The index is again read, and the difference of the two readings noted. This, however, gives not the true angle of the optic axes, but the apparent angle in air, that is, the angle between the rays as affected by refraction on emerging from the crystal. (See Fig. 27.)

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* Continued from p. 466.

In some crystals the optic axes have different angles of inclination for the different rays of the spectrum. Of this titanite or sphene is an example. All rays have a common middle line, and lie in the same plane, but the optic axes for the red rays are more widely separated than those for the blue, and consequently the part of the field which would exhibit a dark brush if red light were used is deprived of the red rays but not of the blue. The brushes, therefore, appear broader than with ordinary crystals, and are tinged with blue on the edges farthest from the middle point, and with red on the edges nearest to it. It is said that a similar distribution of the optic axes, or its opposite in which the red rays are least separated and the blue most, is found in all crystals belonging to the rhombic system.

In other crystals, the axes all lie in one plane, but all have not the same middle line, so that the two ring systems are unsymmetrical. This is the case with borax. In others the optic axes for different colours lie in different planes, all of which pass through the middle line.

Lastly, we may mention the crystals brookite and tartrate of ammonia soda and potash, in which the optic axes for the two extremities of the spectrum lie in planes at right angles to one another, both passing through the same middle line. If the systems of rings be examined with light which has been so widely dispersed that the portion illuminating the field in any given position is practically monochromatic, and the position of the instrument shifted through the different parts of the spectrum (or what is more convenient, if the different parts of the spectrum be successively thrown on the polariscope by means of a totally reflecting prism), the optic axes will be seen to draw gradually together until the figure closely resembles that of a uni-axal crystal; after which the axes open out in a direction at right angles to the former, until they have attained their greatest expansion. This experiment requires a strong light, but it is instructive, as showing the exact distribution of the optic axes for different rays.

In some bi-axal crystals, notably in gypsum, the distribution of the optic axes varies with the temperature. When the crystal is heated the angle between the optic axes diminishes until the crystal appears uni-axal; with a further increase of temperature the axes again open out, but in a direction at right angles to the former. When the crystal is cooled the axes generally resume their original directions. Sometimes, however, when the heating has been carried to a great degree, or has been continued for a long time, the axes never completely return to their normal condition; and in such a case the crystal may appear permanently uni-axal. Such an appearance, when permanent, has been considered a test of former heating; and this phenomenon, when presented by crystals found in a state of nature, may be taken as evidence that the rocks in which they have been formed have been subject to high temperatures.

In the production and examination of the rings hitherto described, we have used light which has been plane-polarised and plane-analysed; but there is nothing to prevent our polarising the light or analysing it circularly, or indeed doing both.

If a quarter-undulation plate be placed between the polariser and the crystal to be examined, with its axis inclined at 45° to the plane of original vibration, the light will fall upon the plate in a state of circular polarisation; and as the polarisation will then have no reference to any particular plane of vibration, the black cross will disappear. A system of rings will be produced, but they will be discontinuous. At each quadrant, depending upon the position of the analyser, the rings will be broken, the portions in opposite quadrants being contracted or expanded, so that in passing from one quadrant to the next the colours pass into their complementaries. If either the direction of the axis of the quarter-undulation plate be changed from 45° on one side to 45° on the other side of the plane of vibration of the polariser; or if the crystal be changed for another of an opposite character (*i.e.* negative for positive, or *vice versâ*), the quadrants which were first contracted will be expanded, and those which were first expanded will be contracted. Hence for a given position of the quarter-undulation plate the appearance of the rings will furnish a means of determining the character of the crystal under examination.

Similar effects are produced if the quarter-undulation plate be placed between the crystal and the analyser; that is, if the light be analysed circularly.

In the case of bi-axal crystals under the action of light polarised or analysed circularly, the black brushes are wanting, but they are replaced by lines of the same form marking where the segments of the lemniscatas pass from given colours into their complementaries.

If the light be both polarised and analysed circularly, all trace of direction will have disappeared. In uni-axal crystals the rings will take the form of perfect circles without break of any kind; and in bi-axal they will exhibit complete lemniscatas.

To pursue this matter one step farther. Suppose that, the arrangements remaining otherwise as before (viz., first, the polariser; secondly, a quarter-undulation plate with its axis at 45° to the principal plane of the polariser; thirdly, a uni-axal crystal; fourthly, a quarter-undulation plate with its axis parallel or perpendicular to the first; and, lastly, the analyser), the analyser be turned round; then in any position intermediate to 0° and 90° the rings will be contracted and extended in opposite quadrants until at 45° they are divided by two diagonals, on each side of which the colours are complementary. Beyond 45° the rings begin to coalesce, until at 90° the four quadrants coincide again. During this movement the centre has changed from bright to dark. If the motion of the analyser be reversed the quadrants which before contracted now expand, and *vice versâ*. Again, if the crystal be replaced by another of an opposite character, say positive for negative, the effect on the quadrants of the rings will be reversed. This method of examination, therefore, affords a test of the character of a crystal.

A similar process applies to bi-axal crystals; but in this case the diagonals interrupting the rings are replaced by a pair of rectangular hyperbolas, on either side of which the rings expand or contract, and the effect is reversed by reversing the motion of the analyser, or by replacing a positive by a negative crystal. The test experiment may then be made by turning the analyser slightly to the right or left, and observing whether the rings appear to advance to, or recede from, one another in the centre of the field. In particular if, the polariser and analyser being parallel, the first plate have its axis in a N.E. direction to a person looking through the analyser, the second plate with its axis at right angles to the former, and the crystal be so placed that the line joining the optic axes by N.S., then on turning the analyser to the right, the rings will advance towards one another if the crystal be negative, and recede if it be positive.

W. SPOTTISWOODE

(*To be continued.*)

FLOWERS OF THE PRIMROSE DESTROYED BY BIRDS

WE have received a number of answers to Mr. Darwin's letter on this subject in NATURE, vol. ix., p. 482; these we have thought it advisable to bring together here. On the general question of the destruction of flowers by birds, Prof. Thiselton Dyer writes as follows:—

MR. DARWIN remarks that he has never heard of any bird in Europe feeding on nectar. There is perhaps one well-authenticated instance in Gilbert White's "Selborne" (illustrated edition, p. 186): "The pettichaps . . . . runs up the stems of the crown imperials, and putting its head into the bells of those flowers, sips the liquor which stands in the nectaine of each petal." This is the more curious, because, according to Kirby and Spence ("Entomology," 7th edition, p. 384), this plant "tempts in vain the passing bee probably aware of some noxious quality that it possesses." I do not know how far this is true, but it has a peculiar odour which makes it rather unpopular as a garden plant.

I have, in my note-book, another instance, also from the *Liliaceæ*, of a plant visited for nectar in an extra-tropical country. Mrs. Barber relates that in South Africa "the long tubular flowers of the aloe are well supplied with nectar, and this provision affords during the winter season a continued store of food for our beautiful sun-birds," the numerous species of the genus *Nectarinia* (Journ. R. Hort. Soc., n.s., ii. 80).

Two other cases of the destruction of flowers by birds occur to me. I was assured this year that the flowers of the common crocus are persistently destroyed by sparrows, at least in the neighbourhood of Hammersmith. The base of the perianth tube, which is the usual seat of any secretion of nectar, is here beneath the surface of the ground; perhaps, however, the style and stigma are attractive to the birds. I did not investigate the matter at all closely, but my informant was an observant person, who I think would be likely to have satisfied himself that the sparrows really did the mischief, the effects of which were obvious enough. If so, we have a clear instance in crocus-eating of an acquired habit on their part.

The other case, that of the destruction of flower-buds of fruit-trees by bullfinches, is probably well known. The mischief is said to be out of all proportion to any benefit the birds can derive from it, as regards food. Such a visitation would obviously tell heavily against the plants in any country where they formed part of the indigenous flora, and had to take their chance with the rest.

Dr. J. H. Gladstone writes, that in his garden the flowers of the primroses have been similarly bitten off, and the crocuses also. He says—

ONE morning some weeks ago I especially remember seeing the beds and the gravel walks strewn with the yellow petals of the latter flower, which were severed from their stalks, and bore abundant marks of the sharp beaks which had torn them asunder. I cannot learn that anyone saw these London birds at their destructive work, which was probably done before any of us were stirring.

Mr. T. R. Archer Briggs, of Plymouth, writes—

I HAVE been familiar with the fact to which Mr. Darwin directs attention for as long a period as that during which he says it has engaged his own, without, however, my being able to point out the author of the mischief. In the neighbourhood of Plymouth it is no uncommon thing to find the flowers both of the primrose and polyanthus bitten off and lying around the plants exactly as Mr. Darwin has described; indeed, so often does this occur here, that I have known it a source of annoyance to cultivators of the latter plant. When residing some years ago at a house in the parish of Egg Buckland, about four miles from Plymouth, I remember to have repeatedly seen the polyanthus flowers in the grounds so destroyed, and to have heard it asserted that the redbreast was the culprit; but of this no proof was brought forward. The locality is a land of springs and streams, and it could not have been a want of water that led the destroyer to do the work there.

The tubular portion of the primrose is much infested by small insects (*thrips?*), and I have sometimes thought that a bird, for the sake of feeding on these, might be led to bite the flowers; but, on the other hand, they are so minute that one can scarcely think they would attract its notice.

I would say, in reply to Mr. Darwin's queries, that primroses are in profusion about Plymouth (at least beyond the immediate neighbourhood of the town, whence they have been rooted out by wretched fern- and wild flower-grubbers), but I have never seen the flowers bitten off to such an extent as in the small Kentish wood he refers to, or in a sufficiently large quantity to materially affect the numbers of the species here.

The Rev. H. C. Key, of Stretton Rectory, Hertford, says that primroses being in great abundance in his neighbourhood, he was led by Mr. Darwin's letter to make a careful search for flowers bittten off in the way he describes, but he failed to find even one.

IT is obvious that the abundance of other food for which birds have a preference—such as apple, pear, plum, and cherry blossoms afford—may possibly have saved our primrose flowers from destruction; but, taking into consideration the fact that animal food must necessarily be supplied to the young birds at this season, I should be disposed to suggest that the primroses Mr. Darwin speaks of have been mutilated by birds rather for the sake of procuring thrips and other beetles, which are attracted by the nectar, than for the nectar itself.

I find the untouched primrose flowers here swarm with beetles and acari; but the great profusion of apple, and pear-blossom, &c., close at hand, may prove more attractive to the birds from the flowers being more open, and therefore more easily accessible.

Mr. G. M. Seabroke writes—

I HAVE observed the same thing as he relates in my small garden in this town. Nearly all the early buds from some twenty primrose plants were bitten off, and birds of some sort were undoubtedly the perpetrators of the mischief. I laid the blame on the sparrows, but did not see them in the act. This is the first year that I have noticed this form of depredation.

Mr. T. R. Stebbing, of Torquay, writes as follows:—

A FORTNIGHT ago the bank on either side of the road from Kingsbridge Road Station to Salcombe were covered, for many miles, with a brilliant profusion of primroses in bloom. In all this long range of country, eighteen miles in all, there was no appearance anywhere of that destruction of blossoms as to which Mr. Darwin makes inquiry. The attention of my companion and myself was especially directed to the primroses throughout our route, not merely by the lavish and unexpected beauty of the display, but by the look-out which we were keeping up for white or red varieties. Among the myriads of plants with the ordinary yellow blossom we noted five with white and two with pinkish flowers. On returning over a portion of the same road ten days later, we detected as many as seven plants with the pale-red or pink flowers, but none of these were blooming freely like the white and the yellow flowering-plants in the same district.

It may be worth noticing that this great stream of primroses flowed down from the rather bleak upland near the railway right into the fertile and sheltered valley of Salcombe, so that in one district or the other the birds might have been expected to seek the nectar, had they been to the manner born, in this part of the country.

A correspondent, E. T. S., says that—

IN the north-west corner of Hampshire the birds have the same taste as in Kent for the nectar of primroses and polyanthuses. A few weeks ago a correspondent wrote thence that this spring the blackbirds "were as bad as peacocks," whose well-known habit of cutting off the blossoms of polyanthuses, carnations, lilies, and any particularly choice tropical plant that they can get hold of, makes them a gardener's despair. A peacock who resided for a short time in the neighbourhood referred to, might possibly have taught the native birds the trick, but this is hardly probable, as he died three winters ago, and the present year, when all spring flowers have bloomed earlier and more abundantly than usual, is the first in which his example has been extensively followed. I should doubt the practice being limited to a single species. Sparrows certainly gather flowers very carefully; I have seen them almost strip a bed of the variegated arabis, though in this case the flower-stalks were carried away and used, not for food, but in nest-building. Does any other bird use fresh flowers for that purpose?

JOHN PHILLIPS

BORN DECEMBER 25, 1800: DIED APRIL 24, 1874

THE daily press has already spread the sad tidings from Oxford that Prof. Phillip met with an accident which suddenly cut short his life while in good health and such full vigour that we still expected work from him. A few days ago he was here amongst us in London, bearing himself with form as erect and step as elastic as if the last ten years had but further mellowed though in no way lessened his energy. Now we learn that a stumble over a door-mat, on leaving a friend's rooms in All Souls, followed by a heavy fall, has deprived Oxford of one of her brightest ornaments, and men of science of a genial friend.

Another bond is broken which linked together by a living presence the geologists of to-day with those who watched the infancy of the science which, in place of wild phantasies of the imagination as to the origin of our planet, substituted a patient and careful investigation of its structure, as far as observation was possible. From the time when William Smith in 1792-3 surveyed the ground between High Littleton and Bath for the Somersetshire Coal Canal, and proved an unvarying sequence in the strata of England, and their identification by their fossil contents, every "cosmogomy" and "theory of the earth" was doomed. Fact henceforth took the place of fancy.

Among the earliest of those trained in the new school was young John Phillips. Born at Marden, in Wiltshire, on Christmas-day (N.S.) 1800, he lost his father when he was but seven years old, and his mother dying soon after, his training fell into the hands of his mother's brother, the renowned William Smith, "Father of English Geology."

We have never heard that there was anything to be recorded of his father beyond that he was the youngest son in a Welsh family, settled for many generations on their own property at Blaen-y-ddol, in Caermarthenshire, who was destined for the Church, but became an officer of the Excise, and that he married the sister of William Smith. Mr. F. Galton, a few weeks ago, read a paper at the Royal Institution, in which he gave statistics about eminent scientific men, showing the number of cases in which the greatness was due to the father, and the number of cases in which it was due to the mother. Whether Prof. Phillips was included we do not know, but he most certainly was an instance in which the influence of the mother preponderated. The mould of the features were distinctly those of the Smith family, and the likeness between Prof. Phillips and the busts and pictures of William Smith has often been remarked. His habit of thought was so much due to the direct training of his uncle that we cannot trace how much of it was hereditary. No particular school could have much influenced him, for he passed through four schools before he was ten, and then for a short time went to the excellent old school at Holt Spa, in Wiltshire. It is said that Latin, French, and Mathematics were his favourite studies, and the enjoyment of Latin authors seems to have grown on him, for in the writings of no other geologist will be found so many quotations from the Latin classics. The Rev. Benjamin Richardson, Rector of Farley Hungerford, near Bath, was one of his earliest instructors in natural history. Very little, indeed, is known of Mr. Richardson; he had the reputation of being in his time the best naturalist in the west of England, and the obituary notices at the time of his death mention that he was a member of Christ Church, Oxford. One fact about him which has an historical interest is certain, and that is that it was his hand which, from the dictation of William Smith, "first reduced to writing at the house of the Rev. Joseph Townsend, Pulteney Street, Bath, 1799" the table of "the order of the strata and their imbedded organic remains in the vicinity of Bath." The original document is in the keeping of the Geological Society, and is regarded as a memorial of the first step towards the examination of strata on a definite plan, the first step in the science of geology as contrasted with cosmogony. During the year that young Phillips spent at the pleasant rectory of Farley, he heard continually of the importance attached to the discoveries of his uncle and of the results which, in the estimation of Richardson and Townsend, were to flow from it. Under Mr. Richardson's direction he spent a large portion of his time in searching for fossils through the valleys around Farley, and in making drawings of the fossils he found and of the recent forms that were most nearly allied to them in Mr. Richardson's extensive collections. Prof. Phillips always spoke with pleasure of his recollections of Mr. Richardson, and attributed to him both his early taste for natural history and the ready use of his pencil, which so often not only reproduced faithfully a geological section but artistically included the foliage and background recording the pleasant accompaniments of the work which principally engaged his attention. Mr. Richardson though a kind was not a flattering guide to the young man, for a frequent remark on being shown the drawing of a fossil was, "Very good John, now put that in the fire and try and do even better."

At the end of the happy year at Farley, young Phillips went to live with his uncle in London, to share with him his labour, his hopes, and his disappointments. William Smith had then just removed to Buckingham Street, after the fire in Craven Street, which had so disarranged his work. Here, however, he rearranged his collection of fossils, the first collection in which fossils were placed in their stratigraphical sequence. Made first at Cottage Crescent, Bath, removed to Trim Street, then to Craven Street, and Buckingham Street, this historical collection finally found a resting-place in the British Museum. Each separate stratum recognised by Smith had one or more shelves sloping to represent the dip as he knew them in the typical ground of the Dunkerton Valley, near Bath, where he first studied them. This was the collection from which young Phillips first derived his ideas of a geological museum for teaching purposes, and which he saw so often referred to by his uncle in explaining to his many visitors his new ideas, when urging upon them the national importance of his discovery as regarded agriculture and mining. William Smith was then working at his map of England, and to this his best energies were given and all his money devoted. In the "Memoirs" of his uncle, published in 1844, Prof. Phillips has described all the delays and trials that attended the production of this, the first geological map of England ever produced. The indomitable courage shown by Mr. Smith in the face of every discouragement could not fail to impress young Phillips with the importance of his uncle's work, and to win respect for him. How he was attached to him, and how he valued his teaching, is apparent in many places in his writings. In the preface to the "Memoirs" he speaks of himself as "an orphan who benefited by his goodness, a pupil who was trained up under his care." The map was issued in 1815, and Mr. Smith's professional engagements rapidly increased, requiring him to visit all parts of the county. He conceived the plan of producing county geological maps on a scale considerably larger than that of the map of England, and on almost every journey his nephew was his glad companion, "haud passibus æquis;" and according to an established custom on all such tours, was employed in sketching parts of the road and recording on maps the geological features of the country. In 1821, the map of Yorkshire, in four sheets, was published, which were prepared and coloured by his own hands. Throughout the "Memoirs" we have indications of the way in which he worked under his uncle's direction. Here is one which gives an insight into the way in which he gained his intimate knowledge of the strata of the country. "The whole of the remainder of 1821 was devoted to long and laborious wanderings. Two lines of operation were drawn through the country which required to be surveyed. On one of these Mr. Smith moved with the due deliberation of a commander-in-chief; the other was traversed by his more active subaltern, who afterwards found the means to cross from his own parallel to report progress at head-quarters." In this way 2,000 miles were traversed in six months, and he thus learned to rely on his own judgment. His work delighted him. "Innumerable rambles," he says, "led up every glen and across every hill, now sketching waterfalls, anon tracing the boundaries of rocks or marking the direction of diluvial detritus." As greater accuracy in tracing the boundary of different strata was thus acquired, the successive issues of the map of England were modified. The lines of these alterations were mostly traced by Mr. Phillips himself, and thus it was that differences appeared in maps which apparently belonged to the same "edition."

At length, in 1824, Mr. Smith was asked to deliver a course of lectures on his geological work at the newly-formed Yorkshire Philosophical Society. For this "new maps were coloured, new sections drawn, and even the distant cabinet of Mr. Richardson at Farley was laid under contribution, to supply illustrations for these discourses." Lectures at Hull, Scarborough, and Sheffield soon followed. The share that Mr. Phillips took in the preparation of these lectures brought him under the notice of the executive of the Yorkshire Philosophical Society; he was offered the curatorship of the new museum, and accepted it. This was one of the important events of his life. His work no longer came before the public in his uncle's name, he had an individuality of his own, "and commenced to make his own reputation." I was delighted to find in the prosecution of this duty innumerable proofs of the truth of Mr. Smith's views respecting the distribution of organic fossils, and saw very clearly that many of the strata in the north-eastern part of Yorkshire might be confidently identified with well-known formations in the south of England. Soon after (in 1826) he read before the Society the first paper he wrote. His subject was: The Direction of the Diluvial Currents of Yorkshire, and it was thought worthy of being reprinted in the "Philosophical Magazine." From this time his pen was ever active. His early geological papers were on Yorkshire, and with that county his name is indissolubly connected. In addition to the curatorship of the museum he was appointed one of the secretaries of the Society, and delivered courses of lectures, and in 1829 he published his illustrations of the Geology of Yorkshire.

It was not till 1834 that Mr. Phillips communicated a paper to the Geological Society, and in the same year he published his "Guide to Geology," was appointed Professor of Geology in King's College, London, and was elected a Fellow of the Royal Society. His recommendature to election into the Society is of sufficient interest to be printed, and is as follows:—

"John Phillips, Esq., of York, Fellow of the Geological Society of London and Secretary of the Yorkshire Philosophical Society, a gentleman well versed in geology, meteorology, and various branches of natural science, and author of "Illustrations of the Geology of Yorkshire," being desirous of becoming a Fellow of the Royal Society, we whose names are hereunto subscribed do, from our personal knowledge, recommend him, as highly deserving of the honour he solicits, and likely to prove a valuable and useful member.

"Rod. I. Murchison, Wm. Buckland, G. B. Gresnough, William Clift, Edw. Turner, Adam Sedgwick, John Taylor, H. T. De la Beche, C. Daubeny, John Edw. Gray, Geo. Peacock, John Lindley, B. Powell.

"Elected April 10, 1834."

Not only was he associated in work with the "father" of Geological Science, from which such valuable practical results have flowed, but he was one of the band who, in his own words, "stood anxious but hopeful by the cradle of the British Association." It is well known how through his activity the first meeting at York was a success in September 1831, and how till 1863 he was the courteous assistant-secretary of the Society.

Among other posts Prof. Phillips has filled are the Chair of Geology at Dublin, to which he was appointed in 1844; the Presidency of the Geological Society in 1859-60; Rede Lecturer in Cambridge in 1860; and the Presidency of the British Association in 1866. The Chair at Oxford he has held since 1853.

He not only helped to lay the foundations of English Geology, he has been to the last an active worker and an industrious writer. Besides more than sixty papers communicated to Societies' proceedings and to magazines, he was largely a contributor to the "Penny Encyclopædia," the "Encyclopædia Britannica," and the "Encyclopædia Metropolitana."

In 1841 was published his "Palæozoic Fossils of Cornwall, Devon, and West Somerset, after he had examined the country in company with Mr. William Sandars.

In 1842 he began an examination of the Malvern district, and having settled his data at Malvern, Abberley, and Woolhope, he extended his observations to May Hill, Fortworth, and Usk. The work was given to the world in 1848 as one of the Memoirs of the Geological Survey. "The Rivers, Mountains, and Sea-coast of Yorkshire" appeared in 1853, and his Essay in the "Oxford Essays," in 1855.

His contribution to the Palæontographical Society on the Belemnitidæ, and his "Geology of the Thames Valley," are well known; and he has also written many smaller works which we have not space to notice.

For many years he has been Keeper of the Museum at Oxford, and his lectures have had such a reputation for being popular that they have been largely attended by ladies. The Professor had also given much time to meteorology and astronomy, and had made many observations in his own observatory. He was an honorary M.A. and D.C.L. of Oxford, and LL.D. of Cambridge and Dublin.

NOTES

Dr. Lyon Playfair, C.B., has given notice that, on the House of Commons going into committee on the Education Estimates, he will call attention to the deficient ministerial responsibility under which the Votes for Education, Science, and Art are administered, and will move for a Select Committee to consider how such ministerial responsibility may be better secured. We believe that Dr. Lyon Playfair's views are strictly in accordance with those of the best scientific men of the country, namely, that the only satisfactory way of dealing with the subject will be by the appointment of a Minister for Education, Science, and Art.

The 15th or 16th of June has been fixed for the inauguration of the physical laboratory, the gift of the Duke of Devonshire to the University of Cambridge.

The following is a list of candidates selected and recommended by the Council of the Royal Society for election as Fellows:—Isaac Lowthian Bell, F.C.S.; W. T. Blanford, F.G.S.; Henry Bowman Brady, F.L.S.; Dr. Thomas Lauder Brunton, Sc.D.; Prof. W. Kingdon Clifford, M.A.; Augustus Wollaston Franks, MA.; Prof. Olaus Henrici, Ph.D.; Prescott G. Hewett, F.R.C.S.; John Eliot Howard, F.L.S.; Sir Henry Sumner Maine, LL.D.; Edmund James Mills, D.Sc.; Rev. Stephen Joseph Perry, F.R.A.S.; Dr. Henry Wyldbore Rumsey; Alfred R. C. Selwyn, F.G.S.; Major Charles William Wilson, R.E.

IT is stated that Dr. J. H. Gladstone, F.R.S., has been nominated to succeed Dr. Odling at the Royal Institution.

THE funeral of the late Professor Phillips will be solemnised at York to-day at 11 A.M. It is understood that this locality was fixed on by himself, other members of his family being buried there.

A DEPUTATION, consisting of Sir Bartle Frere, president of the Royal Geographical Society, Sir James Watson, Lord Provost of Glasgow, Sir William Stirling Maxwell, M.P., and several other members of Parliament, waited on Lord Derby last Friday, to lay before him the claims which exist for an official recognition of the late Dr. Livingstone's arduous services in the cause of humanity and of Science during his long tenure of office as one of Her Majesty's Consuls. The memorial, which was handed to Lord Derby, was signed by many eminent and well-known names, and his lordship said he agreed with the deputation that something ought to be done for the members of Livingstone's family. There seems no doubt that Government will meet the wishes of the country in this matter.

ON Monday night, at the usual meeting of the Royal Geographical Society, the principal business of the evening consisted of reading extracts from the letters of Dr. Livingstone to Sir H. Rawlinson, to Sir R. I. Murchison, to Sir Bartle Frere, and to some private friends. This correspondence extended over six or seven years. Very voluminous materials have been preserved, but the work of editing them has yet to be performed. Sir Bartle Frere was happy to say that the son of the illustrious traveller accepted the duty of editing the materials left by his father, and had resigned a promising career in Egypt for that purpose.

THE *Spectator* proposes that as an appropriate memorial to the late Dr. Livingstone, some Exploration Scholarships should be founded, to be called by the explorer's name.

MR. F. J. SCHUSTER has made a donation of 225*l.* to the Physical Laboratory of Owens College, Manchester, for the purpose of buying apparatus.

THE Royal Irish Academy has sanctioned the following grants from the fund at its disposal for aiding scientific researches by providing suitable instruments and materials:—30*l.* to Messrs. Studdert and Caldwell for the chemical analysis of the mineral waters at Lisdoonvarna, in the county of Clare; 30*l.* to Prof. Macalister, to be expended in the purchase of rare insectivora and other mammals for dissection, in order to enable him to report on the myology of mammals; 40*l.* to Mr. W. H. Bailey, to investigate the fossils of the coal districts in Ireland, with a view to their comparison with those of British and other coal-fields; 50*l.* to Prof. Haughton, to complete an investigation into the chemical and mineral composition of the successive lava-flows of Vesuvius; 39*l.* 17*s.* 11*d.* (being the remainder of the fund) to Dr. David Moore, for the investigation and cataloguing of the Irish Hepaticæ. Gentlemen purposing to undertake scientific researches during the coming year, and desirous to obtain grants from this fund, are invited to send in their applications to the secretary of the Academy without delay.

THE Ogham inscribed stones, ten in number, purchased by the Royal Irish Academy from the representatives of the late Mr. Windele, have been arranged in the crypt of their Museum with the other Ogham stones belonging to the Academy, one being set vertically in the floor, and the others placed either on iron stands in the bays at the south side, or on the dwarf walls forming the bays. These stones are now all easy of access, and, in the daytime, have the advantage of a light well adapted to the examination of their respective inscriptions. The Academy is in possession of 134 photographic negatives of Ogham inscriptions, representing about eighty different texts. It is intended to print these in autotype, and thus to afford to inquirers in this curious branch of study authentic copies of considerably more than half the whole number of such inscriptions known to exist. They will be accompanied by short notices, strictly limited to a statement respecting the localities where the inscriptions were found, and other matters of fact respecting them; the philological discussion and interpretation of them being left to the free competition of scholars.

REAR-ADMIRAL CHARLES H. DAVIS was ordered, on Feb. 23, to the duty of superintendent of the Naval Observatory at Washington, U.S., in place of Rear-Admiral Sands, who has been detached and placed on the retired list, in accordance with the rules of the service. Admiral Sands, during his tenure of office, has merited the respect and goodwill of British Astronomers, who will view with regret the necessary termination of his functions.

AMONGST the estimates passed by the House of Commons on Friday last was 80,000*l.* to continue the works on the New Natural History Museum at South Kensington, with which rapid progress is now being made.

IN answer to a question in the House of Commons on Tuesday Viscount Sandon stated that arrangements consequent on the retirement of Mr. Cole were now the subject of consideration by the Science and Art Department, but had not yet been completed.

THE Council of the Paris Observatory is said to have protested against a ministerial decision which allowed the Bureau des Longitudes to take the half of the astronomical library, which has been forming during centuries, and which is one of the richest in the world. It is almost certain that the decision will be cancelled, M. Leverrier having given the alternative of leaving the whole of the books in the hands of the Bureau, and refusing to be a party to such a mutilation. When the library shall be saved, it will be open to the public under certain regulations.

WE recently announced the oppressive treatment to which M. Alglave, editor of *La Revue Scientifique* and Professor of Law at Douai, had been subjected; there is no doubt now that his suspension by the Minister of Public Instruction has been caused by his refusal to resign the editorship of the journal just mentioned and of the *Revue politique et littéraire*, of which he is also editor. On Monday week, on his going to open his class for the term, he received a letter from the Under-Secretary of the Education Department informing him that his course would be suspended until further notice. Science has many difficulties to contend with in this country, but happily vexatious interference on the part of the State is not one of them.

A PAPER on the grasses and fodder plants which may be beneficial to the squatter and agriculturist in South Australia, by Dr. Richard Schomburgk, director of the Adelaide Botanic Gardens, has been officially published by order of the Governor.

IT is stated in the *Scientific American* that the well-known and much admired Japan lacquer-work, the secrets of which were supposed to be known only to the Easterns, has been successfully reproduced, or rather imitated, in Holland. The lacquer is prepared from Zanzibar copal, coloured black with Indian ink. The articles are painted with several coats of this lacquer, in which the pieces of mother-o'-pearl or other substances used for ornamentation are placed before it becomes hard. The lacquer is then dried by placing the articles in a heated oven or furnace, after which another coat of lacquer is applied, and when dry smoothed with pumice, which is repeated until all cracks are filled up and the surface has become perfectly smooth, when the whole is polished, or rather burnished, with tripoli.

THE Russian Scientific Expedition to the Amu Daria was to set out on Monday last. The expedition will be commanded by the Grand Duke Nicholas Constantinovitch, assisted by Colonel Stoletoff and Dr. Moreff, secretary. It will include 25 persons, whose work will be divided into four sections:—(1) The Trigonometrical and Topographical. (2) The Meteorological Section, which will construct two stations on the Amu Daria, at one of which hourly observations will be made of all the meteorological phenomena. (3) The Ethnographical Statistical Section. (4) The Natural History Section.

THE meeting of French Astronomers took place last week at the Ministry of Public Instruction, under the presidency of M. Leverrier. It was composed of M. Dumeril, director of the *Enseignement*, the astronomers from Paris, Toulouse, and Marseilles Observatories, and Officers from the General Staff of the Trigonometrical Survey. Four sittings were held, and an account of them will be issued shortly. Steps have been taken for the determination of the latitude of Algiers, by telegraph. M. du Barail, Minister of War, and M. Saget, his Staff-Officer, visited the Observatory last Saturday, in order to see for themselves how the work may be begun without further delay.

THE additions to the Zoological Society's Gardens during the last week include four Bladder-nosed Seals (*Cystophora cristata*) from Greenland, presented by Capt. Alex. Gray; a White-winged Whydah Bird (*Urobrachya albonotus*) from West Africa, presented by Mr. J. Fairchild; a Rose-crested Cockatoo (*Cacatua moluccensis*) from the Moluccas, presented by Mr. H. Baldwin; an Azara's Fox (*Canis azaræ*) from South America; a Snowy Owl (*Nyctea nivea*) from South America; a Green-cheeked Amazon (*Chrysotis viridigenalis*) from Columbia, purchased.

ON THE REFRACTION OF SOUND[*]

THE principal object of this paper is to show that sound, instead of proceeding along the ground, is lifted or refracted upwards by the atmosphere in direct proportion to the upward diminution of the temperature; and hence to explain several phenomena of sound, and particularly the results of Prof. Tyndall's recent observations off the South Foreland.

The paper commences with the explanation of the effect of wind upon sound, viz., that this effect is due to the lifting of the sound from the ground, and not to its destruction, as is generally supposed. The lifting of the sound is shown to be due to the different velocities with which the air moves at the ground and at an elevation above it. Owing to friction and obstructions the air moves slower below than above, therefore sound moving against the wind moves faster below than above, and the bottom of the sound waves will thus get in advance of the upper part, and the effect of this will be to refract or turn the sound upwards; so that the rays of sound which would otherwise move horizontally along the ground actually move upwards in circular or more hyperbolic paths, and may thus, if there is sufficient distance, pass over the observer's head. This explanation was propounded by Prof. Stokes in 1857, but it was discovered independently by the author.

The paper then contains descriptions of experiments made with a view to establish this explanation.

These experiments were made with an electric bell, over a nearly flat meadow, and again over the same when it was nearly covered with snow, and it was found (as indeed it was expected) that the condition of the surface very materially modified the results in two ways. In the first place, a smooth surface like snow obstructs the wind less than grass, hence over snow the wind has less effect in lifting the sound moving against it than over grass; and it is inferred that a still greater difference would be found to exist in the case of smooth water. In the second place, the ends of the waves of sound travelling along in contact with the rough ground are continually destroyed by the roughness, and the sound from above slowly diverges down to replace that which is destroyed, and this divergence gradually weakens the intensity of the lower parts of the waves, so that, under ordinary circumstances, the sounds which pass above us are more intense than those we hear. The general conclusions drawn from these experiments are:—

1. The velocity of wind over grass differs by ½ at elevations of 1 and 8 feet, and by somewhat less over snow.

2. That when there is no wind, sound proceeding over a rough surface is destroyed at that surface, and is thus less intense below than above; owing to this cause the same sound would be heard at more than double the distance over snow at which it could be heard over grass.

3. That sounds proceeding *with* the wind are brought down to the ground in such a manner as to counterbalance the effect of the rough surface (2), and hence, contrary to the experiments of Delaroche, the range of sound over rough ground is greater with the wind than at right angles to its direction or than when there is no wind. When the wind is very strong it would bring the sound down too fast in its own direction, and then the sound would be heard farthest in some direction inclined to that of the wind though not at right angles.

4. That sounds proceeding against the wind are lifted off the ground, and hence the range is diminished at low elevations. But that the sound is not destroyed and may be heard from positions sufficiently high (or if the source of sound be raised) with even greater distinctness than at the same distances with the wind.

5. In all cases where the sound was lifted there was evidence of diverging rays. Thus although on one occasion the full intensity was lost when standing up at 40 yards the sound could be faintly and discontinuously heard up to 70 yards. And on raising the head the sound did not at once strike the ear with its full intensity nor yet increase quite gradually; but by a series of steps and fluctuations in which the different notes of sound were variously represented, showing that the diverging sound proceeds in rays separated by rays of interference.

On one occasion it was found that with the wind sound could be heard at 360 yards from the bell at all elevations, whereas at right angles it could be only heard for 200 yards standing up, and not so far at the ground; and against the wind it was lost at 30 yards at the ground, at 70 yards standing up, and 160 yards at an elevation of 30 feet, although it could be distinctly heard at this latter point from a few feet higher.

It hence appears that these results agree so well with what might be expected from the theory as to place its truth and completeness beyond question.

The author then goes on to argue from the action of wind upon sound to another phenomenon which admits of a somewhat similar explanation. The effect of wind together with that of a rough surface in lifting the sound may be shown to account for many of the apparently capricious variations in the intensity with which sounds can be heard at different times; and it gives a reason for the custom which prevails of elevating church bells, platforms, &c., where the sounds are intended to be heard at a distance. But it does not explain a fact, which has often been observed, namely, that distant sounds can be heard much better during the night than during the day, and on dull cloudy days better than on bright hot days. This phenomena has engaged the attention of Humboldt, Delaroche, and recently of Prof. Tyndall, who have all assumed that the sound is obstructed or destroyed in the bright hot air, and have suggested causes which they thought might produce this effect. These suggestions are all more or less open to objection, and none of them meet the difficulty that any heterogeneous condition of the air which could obstruct sound must more or less refract or reflect light and so render vision indistinct. In this paper the author gives another explanation, in which he shows how, as in the case of wind, the sound may be lifted and not destroyed.

It is argued that since wind raised the sound simply by causing it to move faster below than above, any other cause which produces such a difference in velocity will lift the sound in the same way. And since the velocity of sound through air increases with the temperature—every degree from 32 to 70 adding 1 foot per second to the velocity—therefore an upward diminution in the temperature of the air must produce a similar effect to that of wind and lift the sound. Whereas Mr. Glaisher has shown by his balloon observations that such a diminution of temperature exists, and further he has shown that when the sun is shining with a clear sky the variation from the surface is 1° for every

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[*] On the Refraction of Sound by the Atmosphere. By Prof. Osborne Reynolds, Owens College, Manchester. Abstract of paper read before the Royal Society April 23.—Communicated by the Author.

100 ft., and that with a cloudy sky it is only half what it is with a clear sky. These results were from the mean of his observations; under exceptional circumstances the variations were both greater and less. It is hence shown that rays of sound otherwise horizontal would be bent upwards in the form of circles, the radii of which with a clear sky are 110,000 ft., and with a cloudy sky 220,000 ft., so that the refraction is double as great on bright hot days as it is when the sky is cloudy, and still more under exceptional circumstances, and comparing day with night.

It is then shown by calculation that the greatest refraction—110,000 ft. radius—is sufficient to render sound from a cliff 235 ft. high inaudible on a ship's deck 20 ft. high at 1¾ miles, except such sound as might reach the observer by divergence from the waves above, whereas when the refraction is least—220,000 ft. radius—or where the sky is cloudy, the range would be extended at 2¼ miles with a similar extension for the diverging waves. It is hence inferred that the phenomenon which Prof. Tyndall observed on July 3, and other days—namely that when the air was still and the sun was hot he could not hear guns and sounds from the cliffs of South Foreland, 235 ft. high, for more than two miles, whereas when the sky clouded, the range immediately extended to three miles, and as evening approached much farther,—was due, not so much to stoppage or to reflection of the sound by invisible vapour as Prof. Tyndall has supposed, but to the sounds being lifted over his head in the manner described; and that had he been able to ascend 30 ft. up the mast, he might at any time have extended the range of the sound by a quarter of a mile at least. Or had the instruments on the top of the cliff been compared with similar instruments at the bottom, a very marked difference would have been found in the distances at which they could be heard.

It seems that there were instruments at the bottom, and it is singular that throughout his report Prof. Tyndall makes no comment on their performance, unless they were at once found to be so inferior to those at the top that no further notice was taken of them; this seems possible, since beyond mentioning that they were there, Prof. Tyndall throughout his report never refers to them.

It also seems that besides those results of Prof. Tyndall's experiments, there are many other phenomena connected with sound, of which this refraction affords an explanation, such as the very great distances to which the sound of meteors has been heard as well as the distinctness of distant thunder. When near, guns make a louder and more distinctive sound than thunder, although thunder is usually heard to much greater distances. In hilly countries, or under exceptional circumstances, sounds are sometimes heard at surprising distances. When the Naval Review was at Portsmouth, the volleys of artillery were very generally heard in Suffolk, a distance of 150 miles. The explanation being that owing to refraction (as well as to the other causes) it is only under exceptional circumstances that distant sounds originating low down are heard near the ground with anything like their full distinctness, and that any elevation either of the observer or of the source of sound above the intervening ground causes a corresponding increase in the distance at which the sound can be heard.

SCIENTIFIC SERIALS

Memorie della Societa degli Spettroscopisti Italiani, February.—Father Secchi contributes a paper On his Observations of Solar Prominences from April 23 to October 2, 1873. From his tables it appears that the sun was observed on 127 days, when 1,052 prominences were seen, being more than 8 a day, the maximum number visible on any one day was 13, and the minimum 2. The greatest number of prominences over 64″ high occurred in lat. 30° 40′ N. and 20° 30′ S. The greatest number of prominences of all kinds were in lat. 20° 30′ N. and 10° 20′ S. The same author also makes some remarks on the spectroscopic observations of the transit of Venus.

Astronomische Nachrichten, Nos. 1,980–1,981.—These numbers contain a large quantity of observations of positions of the minor planets and comets made in 1873 by Leopold Schulhof. He also gives the positions of more than 100 variable stars, with remarks on a new variable position for 1850, RA 23° 10′ 35″ Dec. −19° 39′·7. Prof. Peters gives the position of Planet 135, Feb. 18, 1874, at 14h. 37m. 49s., Hamilton College, M.T., RA 11h. 19m. 42·7s. Dec. +4° 25′ 5″·11 mag. G. Spoerer gives the positions of spots and prominences for February 1st. Palisa gives the position of the planet discovered by him March 18, 4h. 46m. 39s. RA 12h. 22m. 2·12s. Dec. −3° [illegible]

No. 1,982 contains a long paper On a Method of Computing Absolute Perturbation, being in great measure similar to that of Laplace.

Journal of the Franklin Institute, March.—This number contains an account, by Mr. Crew, of the "prismoidal" one-rail railway (of his invention), of which he has made two years' trial in Alabama, with encouraging results. The cars are kept securely on the prismoidal track by a combination of wheels; a centre one, at either end, on the rail, kept on the track by revolving flanged wheels at either side; and wheels on the sides of the prismoid, with strong wrought-iron bars to the side of the car; these keep the car upright. One proposed application of the system is that of elevated rapid transit by steam through crowded streets in populous cities. As to speed, Mr. Crew thinks even 100 miles an hour would be possible; there is no oscillation through lateral motion.—Mr. Richards continues his Principles of Shop Manipulation for Engineering Apprentices, treating of belts, gearing, hydraulic and pneumatic apparatus as means of transmitting power, and of "machinery of application" of power.—Mr. Isherwood points out a method of ascertaining what portion of the feed-water admitted to a boiler is entrained in the form of spray by the escaping steam.—Details with reference to the Girard Avenue Bridge (which will form the chief entrance to the West Park, at Philadelphia), are furnished by Mr. Hering.—Prof. Thurston claims for Count Rumford a higher place in connection with thermo-dynamics than has hitherto been assigned to him; affirming that he first, and half a century before Joule, determined with almost perfect accuracy the mechanical equivalent of heat, while the sole credit of discovering the true nature of heat is due to him.—We may note, in addition, a paper On Railway Crossings and Turnouts, by Mr. Evans, and one On the Sanitary Care and Utilisation of Refuse in Cities, by Dr. Leas, who describes, more especially, the system followed in Baltimore.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 23.—On some points connected with the Circulation of the Blood, arrived at from a study of the Sphygmograph Trace, by A. H. Garrod, B.A., Fellow of St. John's College, Cambridge.

The author commences by giving a table containing a fresh series of measurements of the ratio borne by the cardiosystole * to its component beat in the cardiograph trace. These tend strongly to substantiate the law previously published by him, viz., that the length of the cardiosystole is constant for any given pulse-rate, and that varies as the square root of the length of the pulse-beat only, being found from the equation $xy = 20\sqrt{x}$ when x = the pulse-rate and y = the ratio borne by the cardiosystole to the whole beat.

A similar series of fresh measurements are given in proof of the law previously published by him, that in the sphygmograph trace from the radial artery at the wrist, the length of the sphygmosystole † is constant for any given pulse-rate, but varies as the cube-root of the length of the pulse-beat, it being found from the equation $xy' = 47\sqrt[3]{x}$, where x = the pulse-rate, and y = the ratio borne by the sphygmosystole to the whole beat.

By measurement of sphygmograph tracings from the carotid in the neck and posterior tibial artery at the ankle, it is then shown that the length of the sphygmograph in these arteries is exactly *the same* as in the radial; so that the above-stated law as to the length of the sphygmosystole in the latter applies to them also, and must therefore equally apply to the pulse in the aorta.

Such being the case, by comparing the equations for finding the length of the cardiosystole with that for finding the aortic sphygmosystole, the relation between the whole cardiac systolic act and the time during which the aortic valve remains open can be estimated with facility; for by subtracting the shorter sphygmosystole from the longer cardiosystole a remainder is obtained which can be nothing else than the expression of the time occupied by the ventricle at the commencement of its systole in raising its internal pressure to that of the blood in the aorta, which must occur before the aortic valve can open up. This interval is named the *syspasis*. Its length is found to decrease very rapidly with increase in the pulse-rate, and to become *nil* at a pulse-rate of 170 a minute. An attempt is made to explain these phenomena.

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* The *cardiosystole* is the interval between the commencement of the systole and the closure of the aortic valve in each revolution.

† The *sphygmosystole* is the interval between the opening and closure of the aortic valve in each cardiac revolution.

If the above considerations are correct, certain independently obtained measurements ought on comparison to correspond; for by reference to one of the papers in the Society's Proceedings it is shown that the length of the there-termed second cardio-arterial interval (which may be called the second cardio-radial interval, as the artery under consideration was the radial), can only represent the time taken by the second or dicrotic wave of the pulse in travelling from the aortic valve to the wrist. This being so, there is every *à priori* reason in favour of the earlier primary wave taking the same time in going the same distance; which can be expressed in other terms by saying that the length of the first cardio-radial interval, from which that of the syspasis has been subtracted, ought to be exactly the same as that of the second cardio radial interval. That such is the case is proved by the two measurements, which have been arrived at independently, agreeing in all cases *to three places of decimals*, which is great evidence in favour of the accuracy of the methods and arguments employed.

The latter part of the paper is occupied with the description of and the results obtained by the employment of a double-sphygmograph, by means of which simultaneous tracings are taken from two arteries at very different distances from the heart. The arteries experimented on are the radial at the wrist and the posterior tibial behind the ankle, 29 and 52¼ inches respectively from the aortic valves. From the resulting traces the time occupied by the pulse-wave in travelling the difference of distance—(52·5 - 29) = 23·5 inches is given—is found to be 0·0012 of a minute in a pulse of 75 a minute, and it is shown that this varies very little with difference in pulse-rate, as other considerations would lead us to expect; it is also proved that *there is a marked acceleration of the pulse-wave as it gets further from the heart*.

By superposing the simultaneous trace from the wrist on that from the ankle, direct verification is obtained of the earlier proposition, that the sphygmosystole at the wrist and at the ankle are of exactly similar duration. The peculiarities of the ankle trace are also referred to.

Geological Society, April 15.—John Evans, F.R.S., president, in the chair.—The following communications were read: About Polar Glaciation, by J. F. Campbell. The author commenced by referring to a reported statement of Prof. Agassiz, to the effect that he supposed the northern hemisphere to have been covered in glacial times from the pole to the equator by a solid cap of ice. He described his observations made during thirty-three years, and especially those of last summer, when he travelled from England past the North Cape to Archangel, and thence by land to the Caucasus, Crimea, Greece, and the south of Europe. His principal results were as follows:—In advancing southwards through Russia a range of low drift hills occurs about 60° N. lat., which may perhaps form part of a circular terminal moraine left by a retreating polar ice-cap; large grooved and polished stones of northern origin reach 55° N. lat. at Nijni Novgorod, but further east and south no such stones could be seen. The highest drift beds along the whole course of the Volga seem to have been arranged by water moving southwards. In America northern boulders are lost about 39°, in Germany about 55°, and in Eastern Russia about 56° N. lat., where the trains end and fine gravel and sand cover the solid rocks. Ice-action, in the form either of glaciers or of icebergs, is necessary to account for the transport of large stones over the plains, and the action of moving water to account for drift carried farther south. There are no indications of a continuous solid ice-cap flowing southward over plains in Europe and America to, or nearly to, the equator; but a great deal was to be found on shore to prove ancient ocean circulation of equatorial and polar currents, like those which now move in the Atlantic, and much to prove the former existence of very large local ice-systems in places where no glaciers now exist.—Note regarding the Occurrence of Jade in the Karakash Valley, on the southern borders of Turkestan, by Dr. Ferdinand Stoliczka, Naturalist attached to the Yarkund Mission. In this paper the author described the jade-mines on the right bank of the Karakash river formerly worked by the Chinese. There are about 120 holes in the side of the hill, and these at a little distance look like pigeon-holes. The rocks are a thin-bedded, rather sandy syenitic gneiss, mica- and hornblende-schists, traversed by veins of a white mineral, apparently zeolitic, which in turn are traversed by veins of jade.

Zoological Society, April 21.—Viscount Walden, F.R.S., president, in the chair.—The secretary read a report on the additions that had been made to the Society's Menagerie during the month of March 1874. Amongst these particular attention was called to a scarce Parrot (*Chrysotis finschi*), of which a specimen had been presented by Mrs. Chivers.—A communication was read from Mr. Morton Allport On the capture of a Grilse in the River Derwent, in Tasmania, showing that the salmon had really been successfully introduced into the colony.—Communications were read from Dr. J. E. Gray, F.R.S., On the very young of the Jaguar, *Felis* (*Leopardus*) *onca*; On the short-tailed Armadillo, *Muletia septemcincta*; On the young of the Bosch Vark, *Potamochærus africanus*, from Madagascar; and On the Skulls of the Leopard in the British Museum.—A communication was read from Dr. O. Finsch, containing the description of a new species of Penguin, from New Zealand, which he proposed to call *Eudyptula albosignata*.—Mr. Edwin Ward exhibited the skull and horns of a fine specimen of the Persian S[illegible]g (*Cervus maral*) from the Crimea.—A communication was read from Capt. W. H. Unwin, containing an account of the breeding of the Golden Eagle (*Aquila chrysaetos*) in North-Western India.—Mr. J. E. Harting read a paper On a new species of *Tringa*, from St. Paul's Island, Alaska, which he proposed to name *Tringa gracilis*.—A communication was read from Lieut. R. Wardlaw Ramsay, giving the description of an apparently new species of Woodpecker, which he had obtained in a teak-forest, about six miles to the north of Tanghoo in British Burmah. Mr. Ramsay proposed to name it *Gecinus erythropygius*.—Messrs. W. T. Blanford and H. E. Dresser read a monograph of the genus *Saxicola*, Bechstein, being an attempt to reduce into some order the excessively confused nomenclature of the species composing this genus.

Royal Horticultural Society, April 15.—Scientific Committee.—M. T. Masters, M.D., F.R.S., in the chair.—Mr. Worthington Smith exhibited a drawing of a very curious fasciation in the aerial roots of *Aerides crispum*, in which the roots presented the curious flattened appearance so often met with in the branches of the ash, the shoots of asparagus, &c.—Mr. W. G. Smith also showed a drawing of the very rare *Angræcum ellisii*, from the collection of Mr. Day, of Tottenham. Mr. Smith remarked that the flowers turn brown when bruised.—Mr. Smith also showed a wood-engraving made on the wood of green ebony, *Brya ebenus*. Mr. Smith reported that for engraving purposes this was as good as bad box.—Prof. Thiselton Dyer showed dried specimens of a variety of *Hibiscus rosa-sinensis* from Zanzibar, where it was found wild by Dr. Kirk. The petals are palmately cut, as in *Clarkia*, *Schizopetalum*, &c. Dr. Masters made some remarks on the analogy the divided petals of this plant presented with the stamens of mallows, which it is now supposed consist of five primary organs, subsequently dividing into numerous anther-bearing filaments. It is doubtful whether *Hibiscus rosa-sinensis* has been heretofore observed in a truly wild condition. The discovery of the plant in east tropical Africa is therefore particularly interesting. It is possible, however, that it may prove a distinct species.—Prof. Thiselton Dyer also showed an elegant white fungus, having the appearance of lace, from Santarem. The Rev. M. J. Berkeley considered it probable that it was the fungus published by Kunze as *Rhizomorpha corynephora*.—Mr. Andrew Murray exhibited a fungoid production existing on trees over a considerable space in the Yosemite Valley, in California. Mr. Berkeley considered it near to the fungus called *Dothidea morbosa*, but there was also a gall on the same shoot.—Mr. Murray exhibited larvæ of a beetle closely allied to *Hammaticherus heros*, a beetle very destructive to timber in Germany, found feeding on the roots of fir near Enfield. Specimens of the perfect living insect have from time to time been found in the gun-stocks of walnut wood in the small-arms factory. It seems, therefore, a fair inference that the insects had escaped thence, and may perhaps have become naturalised—a most undesirable thing, for the larva is very destructive to timber. Mr. Blenkins remarked that he was familiar with the insect in the Crimea.

General Meeting.—H. Little in the chair.—The Rev. M. J. Berkeley commented on the plants exhibited *Arthropodium cirrhatum* was an interesting plant of striking habit from New Zealand. When first introduced into this country, some years ago, it was supposed to have come from New Holland.

Physical Society, April 18.—Dr. Gladstone, F.R.S., in the chair.—Dr W. H. Stone read a paper On Wind Pressures in the human chest during performance on wind instruments. The author's object was to ascertain (1) what was the extreme height of a column of water which could be supported by the muscular act of expiration transmitted by the lips: this was found to be about 6 ft.; and (2) what was the actual pressure corresponding to the full production of a note on each of the principal wind instruments. It was found that with the majority of wind instruments the pressure required for the high notes is considerably greater than that required for the low notes, each instrument having a pressure-ratio of its own. The clarinet is an exception to the rule.—Mr. Tribe illustrated by experiments the action of hydrogen upon finely divided metals, such as are produced by precipitation.

EDINBURGH

Royal Physical Society, April 22.—R. Scot Skirving, president, in the chair.—Recent Modes of determining the Impurity of Milk, by J. Falconer King, City Analyst. The only sure way to determine the quality of milk is to make a proper and careful chemical analysis of it.—Additional Note on the Suspension of Clay in Water, by Wm. Durham. Finely-powdered silica was found to behave in a manner generally similar to clay. Experiments seem to show that each solution has a specific capacity of sustaining clay, and also that this capacity varies in a specific manner according to the strength of the solution.—Note on the Formation of Boulder Clay, by D. J. Brown. Mr. Brown advocated that the usually accepted theory of the land origin of boulder clay would not explain the nature of this remarkable deposit, and considered that it was formed at the line of junction of the Arctic glacier with the sea.—On Fused Stones, showing Columnar Structure from a Pictish Tower, by the Rev. Jas. M. Joass, Golspie. These stones, in their columnar structure, illustrate, though on a small scale, an important geological phenomenon. The instance usually cited in illustration of the development of columnar structure in a melted mass is that of grain-tin, which forms rude columns on cooling. The author ventures to think that these fused stones afford a new and rather better illustration of the geological phenomenon, more closely analogous to the case of lavas, inasmuch as we have, in fact, a fused silicate, an artificial lava, forming columns the same in character as those of the Giant's Causeway, Samson's Ribs, or the pillars of Fingal's far-famed cave.

PARIS

Academy of Sciences, April 20.—M. Bertrand in the chair.—The following communications were read:—Letter relating to a calculation, by Pouillet, on the cooling of the sun's mass, by M. Faye. The author showed that Pouillet's calculation tacitly implied that the sun's mass was not susceptible of contraction, and again restated his belief that solar radiation is not maintained by external causes, but is to be looked for in the formation of the sun itself, and in the enormity of its mass.—Observations concerning a communication, by M. Crocé Spinelli, on the lines of aqueous vapour in the solar spectrum, a letter from P. Secchi to the perpetual secretary. The author stated, that although the elements of water would be dissociated at the high temperature of the sun, their combination might take place in the ascending currents accompanying spots and eruptions owing to the lowering of temperature in these currents produced by expansion.—Tenth memoir on the formation of various crystalline substances in capillary spaces, by M. Becquerel.—New researches on the cyanogen series, by M. Berthelot. A continuation of this author's valuable researches in thermo-chemistry.—Heat of formation of the Cyanogen compounds, by M. Berthelot.—On Phylloxera and the American vines at Roquemaure (Gard), a note by M. J. E. Planchon.—Collimating level and its employment for foggy horizons, by M. G. M. Goulier.—On Orometric dials, specially applicable to pocket barometers, by the same author.—On partial differential equations which can be integrated without arbitrary functions, by M. de Pistoye.—On the "singular points" of algebraical plane curves, by Mr. Halphen.—On the rôle of salts in the action of potable waters on lead, by M. Fordos. The author recommended, as the results of his experiments, the filtration of all water issuing from leaden conduits.—Mode of preservation of the wood employed in large manufactures and in railways, by M. Hubert. The preservative is hydrated ferric oxide.—On the absorption of oxygen and the emission of carbonic acid by leaves kept in darkness, by MM. P. P. Dehérain and H. Moissan. The authors have proved that leaves kept in the dark give off a quantity of CO_2 increasing with the temperature, that the quantity of CO_2 given off is comparable to that given off by cold-blooded animals, that the leaves absorb more oxygen than they give off CO_2, and that they continue to evolve CO_2 in an atmosphere deprived of oxygen.—Facts concerning the vibration of the air in sonorous pipes, by M. E. Gripon.—On a new thermo-electric pile, by M. C. C. Clamond.—On a volume regulator for gas currents, by M. H. Giroud.—On tetra-iodide of carbon, by M. G. Gustavson. This substance has been obtained by the action of tetrachloride of carbon upon dialuminic hexiodide, according to the equation $3CCl_4 + 2Al_2I_6 = 3CI_4 + 2Al_2Cl_6$; the two substances being dissolved in carbon disulphide. It was described as a red crystalline substance decomposed by heating in the air into CO_2 and free iodine.—New researches on black phosphorus, by M. Blondlot.—Action of pure hydrogen on silver nitrate, by M. H. Pellet. The author stated that a neutral or slightly acid solution of the salt is not reduced in the cold by pure hydrogen, and that an alkaline solution is reduced in the cold to an extent proportional to its alkalinity, elevation of temperature increasing the reducing action.—Researches on soluble phosphates used in agriculture, by M. A. Millot.—On the direct determination of the degree of intensity of explosive mixtures: application of the method to gunpowders, by M. Chabrier.—Action of bromine on dibromsuccinic acid; tribromsuccinic acid, by M. E. Bourgoin. The following substances are obtained by the action of bromine and water on the acid: tribromsuccinic and dibrom maleic acids and dibrominated ethylene dibromide.—On the alcohols contained in the acid liquors of starch manufactories and in the products of the butyric fermentation of glucose, by M. G. Bouchardat. These are ethylic, normal propylic, and butylic alcohols.—On the determination of alcohol in water, wines and [illegible] liquors, by M. Salleron.—General method for the transformation of alcohols into nitric ethers, by M. P. Champion. The reagent employed is nitro-sulphuric acid.—On phenyl-allyl, by M. B. Radziszewski.—On pyrogallol in presence of iron salts, by M. E. Jacquemin.—On the colouring matter of wine, by M. E. Duclaux.—On the volatile acids of wine, by the same author.—Movements excited in the stamens of *Mahonia* and *Berberis*, anatomical conditions of this movement, by M. E. Heckel.—On the direction of the wind in the high and low (atmospheric) regions during the storm of April 13, 1874, by M. Chapelas.—During the meeting a commission was appointed to prepare a list of candidates for the vacancy of foreign associate caused by the death of M. De la Rive.

BOOKS RECEIVED

ENGLISH.—Handbook of Practical Telegraphy. [illegible]: R. S. Culley (Longmans).—Mental Physiology: W. B. Carp[illegible] King & Co.).—The Design and Construction of Harbours: Thos. Stevenson (A. & C. Black).—Our Inheritance in the Great Pyramid: C. Piazzi Smyth (Isbister & Co.).—Longevity. John Gardner (H. S. King & Co.).—The New Chemistry: Josiah P. Cooke (H. S. King & Co.).—Hydrostatics and Pneumatics: Lardner and Loewy (Lockwood).—Geology of Suffolk: J. E. T[illegible] (White).—The Universe and the Coming Transits: R. A. Proctor (Longmans).—Haydn's Dictionary of Dates. 14th edit.: B. Vincent (Moxon).

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